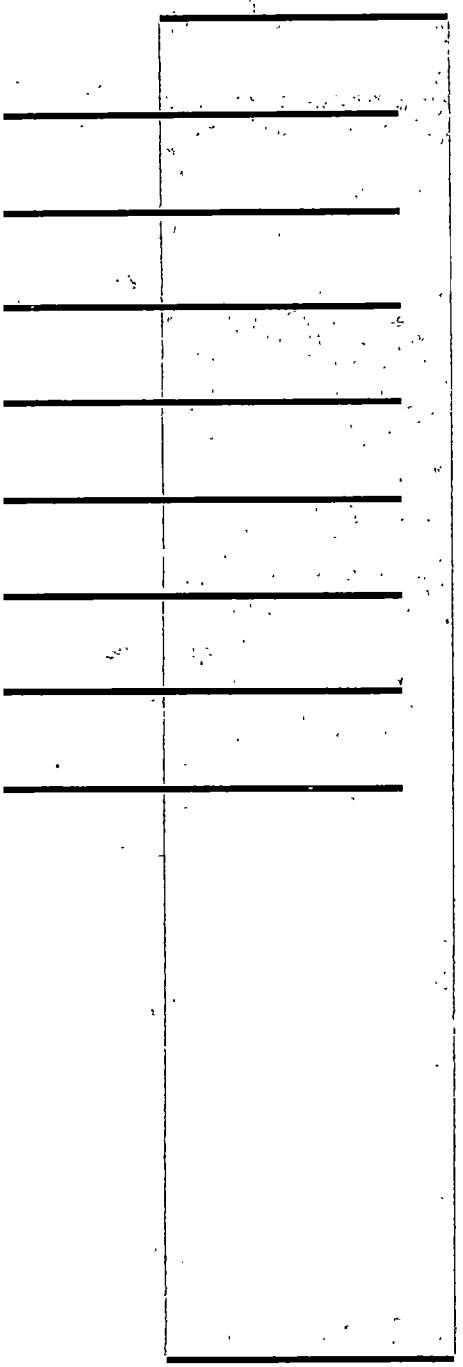


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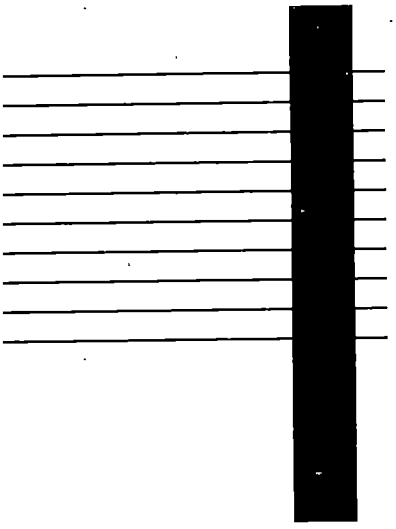
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ARTICLES



THE STATE IN POLITICAL SCIENCE: HOW WE BECOME WHAT WE STUDY

THEODORE J. LOWI *Cornell University*

American political science is a product of the American state. There are political reasons why particular subdisciplines became hegemonic with the emergence of the "Second Republic" after World War II. The three hegemonic subdisciplines of our time are public opinion, public policy, and public choice. Each is a case study of consonance with the thought-ways and methods of a modern bureaucratized government committed to scientific decision making. Following Leviathan too closely results in three principal consequences: (1) failure to catch and evaluate the replacement of law by economics as the language of the state, (2) the loss of passion in political science discourse, and (3) the failure of political science to appreciate the significance of ideological sea changes accompanying regime changes.

This presidential pilgrimage is over, and I can report that the American Political Science Association is alive and well. But a pilgrimage is not a journey into happiness. A pilgrimage is a search, and no pilgrimage is fulfilled until the the pilgrim returns and shares the pains of discovery.

From out of their early pilgrimage, the Quakers cried, "Speak truth to power." From out of my pilgrimage I responded, "Who's listening?" and "What truths do we have to impart?" On my pilgrimage I listened in on the conversation between political science and power, and it is my duty to report that the terms of discourse have been set by power. We are not the teachers we thought ourselves to be.

The insights of my pilgrimage began with my awakening to three facts: (1) U.S. political science is itself a political phenomenon and, as such, is a product of the state; (2) there is not one science of politics but several, each the outcome of a particular adaptation to what it studies; and (3) even assuming that we are all sincerely searching for the truth (and it is more interesting to assume that), there are reasons other than the search for truth why we do the kinds of political science we do and why particular subdisciplines become hegemonic. In sum, every regime tends to produce a politics consonant with itself; therefore every regime tends to produce a political science consonant with itself. Consonance between the state and political science is a problem worthy of the attention of every political scientist.

To explore the relation between the state and political science, I have chosen case studies of the three hegemonic subdisciplines of our time—public opinion, public policy, and public choice—preceded by an overview of the transformation from the old to the new state and the old to the new political science. I will conclude with a brief evaluation of the consequences for political science of being a "dependent variable."

There is no need to document for political scientists the contention that the American state until the 1930s was virtually an oxymoron. The level of national government activity was almost as low in 1932 as it had been in 1832. However, although a number of large social movements had failed to expand the national government after the Civil War, they had succeeded in nationalizing the focus of U.S. politics. The Civil War and industrialization made us one nation in fact. *Wabash, St. Louis, and Pacific Railway* v. *Illinois* of 1886 (118 U.S. 557) contributed with the doctrine that the state governments were constitutionally incompetent to confront the nationalizing economy. The media transferred their dependence from the highly localized political parties to the corporations seeking mass sales through advertising.

Political science as a profession was a product of this nationalization of political focus. Intellectual historians such as Somit and Tannenhaus (1967) and Seidelman (1985) report that the APSA was part of the progressive reform movement. Somit/Tannenhaus report that only 20% of the first decade's membership were "professors and teachers" (p. 55). From out of the beginnings in the 1890s, where the writing was "legalistic, formalistic, conceptually barren and largely devoid of what would today be called empirical data" (p. 69), the founders of the association were committed to political realism, which meant facts, the here and now, and the exposure of the gap between the formal institutions and the realities. James Bryce in his address as the fourth association president in 1909, urged political scientists to "Keep close to the facts. Never lose yourself in abstractions. . . . The Fact is the first thing. Make sure of it. Get it perfectly clear. Polish it till it shines and sparkles like a gem" (quoted in Somit and Tannenhaus 1967, 70). The title of Woodrow Wilson's presidential address to the seventh annual meeting of the APSA was "The Law and the Facts." Early in his speech he said, "I take the science of politics to be the accurate and detailed observation of [the] processes by which the lessons of experience are brought into the field of consciousness, transmuted into active purposes, put under the scrutiny of discussion, sifted, and at last given determinate form in law" (1911, 2). But these were not

facts for themselves alone. Some early political scientists were active reformers, others were radical muckrakers, and a few may have been completely aloof. But facts were to be put in the service of assessment: Did a given political institution meet its purpose? According to Wilson, political scientists should serve as a kind of "self-constituted commission . . . to discover, amidst our present economic chaos, a common interest, so that we might legislate for the whole country instead of this, that, or the other interest, one by one" (pp. 6–7).

There is no evidence to suggest that the founding generation were trying to form an *intelligentsia*, defined as an organization of intellectuals in opposition to the state. There was, in fact, no state to organize against. If anything, there was a memory trace of the two states that conducted the most devastating total war in history up to 1865. But both states were dismantled quickly after the Civil War and were folded back into the "stateless polity" of the restored Union (Bensel 1990). One could say, however, that the early APSA was a kind of *counter*intelligentsia formed in defense of a state that did not yet exist. The political science of the entire first generation of the APSA was formed around politics—the observable, the immediate, and the short-run purpose to be served. But politics was not only a phenomenon, it was a problem. For example, to Goodnow, the purpose of the political science was to show "particularly from a consideration of political conditions as they now exist in the U.S., that the formal governmental system as set forth in the law is not always the same as the actual system" (quoted in Ross 1991, 274). And for most of them, there was a handy solution to the problem of politics—government, properly characterized as the "building of a new American state" (Skowronek 1982). This goal of a new American state can, in turn, be characterized as a stateless government, or an enlightened administration. Woodrow Wilson, while still an obscure professor of political science at Johns Hopkins, sounded the call for the study of administration in 1887. This should be understood, however, within the context of his still larger declaration that the era of constitution making was closed "so far as the establishment of essential principles is concerned" (quoted in Ross 1991, 275). Administration could be a solution to politics because, in Wilson's words, we could have the Prussian state breathe free American air (Wilson 1887). As Seidelman puts it, "the study of politics for Wilson thus had to evolve into a study of America's cultural uniqueness and European administration" (1985, 44). Wilson was confirming the unarticulated major premise of political science, namely, that the American system was permanent and that the science of politics involved the study and assessment of political things within a permanent and unique context. We were one republic, then and forever. Political scientists could remain a counterintelligentsia not because all members shared the Lockean liberal consensus but because they were scientists in the state-building business even while, as with Bentley, they were attacking the very concept of the state as "soul stuff" (ibid., 70–71). For the same reason, political science was atheoretical. Works produced by the founding generation stand up well even by today's standards of science and are superior to most of ours in the quality of the knowledge they brought to bear and in their use of the English language. But the work remained essentially empirical and became almost technocratic in its participation in the reform movement, primarily because it had no concept of an alternative regime in the United States.

It should have been unmistakably clear to any political scientist of 1887 or later that the American system after the Civil War was a new regime, deserving a new name. Why not the Second Republic? The answer is that that would have suggested an impermanence to the American regime. If a Second Republic, why not a Third and Fourth? My wife sometimes introduces me to her friends as her first husband. That is a sobering sobriquet. Political science was atheoretical because it had no concept of a Second Republic or of any other alternative regime. Eventually, political scientists would virtually rewrite democratic theory to accommodate political parties and would rewrite republican theory to accommodate the devolution of constitutional powers from Congress to the presidency. But this was not a self-conscious act of political theory; it was part of "the study of political conditions as they exist." In the stateless polity of the founding epoch, the science of politics was the study of politics and of political institutions within a timeless, as well as a uniquely American, framework.

In my opinion, the golden age of U.S. political science came toward the end of this founding epoch, which corresponds, of course, with the end of the stateless polity. Works of political science of the 1930s and 1940s were magnificent in their ability to describe a complex political whole; thorough, honest, and imaginative in their use of statistics to describe a dynamic reality; and powerful and cogent in pointing out flaws and departures from U.S. ideals. But this was the sentimental part of my journey. To yearn for those particular studies of elections, case studies of interest groups and policymaking, histories of party systems, and representation in Congress is to yearn also for the luxury of the First Republic, now that we are irreversibly in the Second and possess at least the bare beginnings of an awareness of the possibility of regime change in the United States.

Surely by now there has been, in fact, a change of regime, which I call the Second Republic, for lack of an established enumeration. It is not the French state or the Prussian state; but at least, we can say that the American state is no longer an oxymoron. Here, all too briefly, are its relevant high spots: (1) it is a positive, not a reactive, state, from the start centered on the executive branch; (2) constitutional limits on the powers of the national government over the economy and on the distribution of power among the branches within the national government were very quickly laid to rest; (3) many aspects of politics that had traditionally been private (e.g., registration, bal-

lots, election administration, nomination, job patronage, polling, and campaign finance) have been governmentalized—that is, modern government has assumed responsibility for its own politics; (4) political parties, like nuclear families, have declined for lack of enough to do; (5) bureaucracy, independent of party and Congress, has expanded in size and scale approaching autonomy as a social force; and, (6) intimately connected with (5), government has become intensely committed to science. This was no accident, and it is no mere policy. Science is an inherent part of the new, bureaucratized state, in at least two dimensions: First, it involves a commitment to building science as an institution, that is, a commitment to government *for* science; and second, it involves a commitment to government *by* science—that is to say, it involves scientific decision making. This has been properly characterized as *technocratization*, which I take to mean "to predict in order to control" (compare Mills 1959, 113). But another to-me-more-interesting but less appreciated part of this aspect of the expansion of science is that *economics has replaced law as the language of the state.*

What Tocqueville said of the First Republic we may say of the Second: "A new science of politics is needed for a new world" (quoted in Wood 1969, v). But life is not quite so simple. If modern states are differentiated, there are almost certain to be several sciences of politics, rather than just one. We tend to call these subdisciplines; but despite continuities and overlaps, they are quite distinct. Each can be understood as a product of the phenomena it studies; but I am concerned here not to explain or place them all but only to understand the "hegemonics" of disciplines—why public opinion, public policy, and public choice became hot topics and when.

Some call public opinion behavioral science. I think I am more accurate calling it public opinion. Observers from an alien intellectual planet would find it most peculiar that the study of individual opinions and attitudes could be called behavioral—until they deconstructed the discourse between the new bureaucratized state and the new political science. Here is my deconstruction:

1. If science is to be public, it must be neutral.

2. It must also be rational and therefore concern itself with rational phenomena, that is, orderly, repeatable, predictable phenomena. This is precisely what makes science and bureaucracy so compatible. Karl Mannheim, in 1929, over 20 years before the behavioral revolution, wrote, "Bureaucratic thought is permeated by measurement, formalization, and systematization on the basis of fixed axioms . . . [such that] only those forms of knowledge were legitimate which touched and appealed to what is common to all human beings" (1936, 167).

3. Science also has to be microscopic, down to the irreducibly smallest unit. It is no paradox that as our state grew larger, the units of analysis in our social science became smaller. This is a profoundly important aspect of rationality: out of small units, large numbers grow; and large numbers behave according to the regularities of mathematical probability. (In this context it is easy to understand why Arthur Bentley's appeal "to fashion a tool" with the group as the smallest unit of analysis was first uttered in 1907 and not really heard, or responded to, until over 40 years later [see Seidelman 1985, 72–74].)

4. Science, like administration, has to follow a prescribed method. As Robert Wiebe put it, "Bureaucratic thought . . . made 'science' practically synonymous with 'scientific method'. Science had become a procedure . . . rather than a body of results" (1967, 147).

5. The language itself has to be microscopic; that is, science has to be translated into the language of variables.

The phenomena and methodology of public opinion obviously meet all the requirements of a science that would be consonant with bureaucratic thinking. And now consider the units of analysis within the sample surveys that give public opinion its link to political behavior: voting and participation. These display an even stronger consonance with the state, in that these are approved political behavior (i.e., political behavior sponsored by the state and needed by regimes and elites to maintain their legitimacy).

Some see behavioral science as a large step toward hard science and, through that, an advancement toward greater enlightenment about society and politics. I do not disagree. But my political analysis tells me also that the hegemony of the subdiscipline of behavioral science or public opinion was to a large extent a product of its compatibility with bureaucratic thought-ways, rather than the result of successful discourse within political science.

It is important to emphasize, however, that the hegemony of the subdiscipline of public opinion is a case of natural selection, not one of political maneuvering or intellectual opportunism. Anyone personally acquainted with the people who made the behavioral revolution in political science would agree that if political skill were required to succeed, there would be no survey research centers—probably no behavioral science at all. It is their very lack of attention to playing the political game that makes the success of their field so interesting. The explanation is to be found not in politics in the vulgar sense but politics in the higher sense—the politics of state building.

The Second Republic, having put a new emphasis on science, also determined what that science would be. The capacity to engage in public opinion research in political science had been in existence since at least the late nineteenth century. Statistics, which takes its name from *state* and *statist*, reached maturity still earlier in the nineteenth century and grew in importance as states democratized and individuals began to "count" for something. Sampling was also well advanced and widely practiced, especially in the agricultural sciences (Porter 1986, 23–25). Even opinion polling in political campaigns was actually tried at least as early as 1892, albeit over the objections of many defenders of the sanctity of elections (Jensen 1969, 228–229)—and was picked up by advertising

companies and newspapers soon after. Yet public opinion did not become the hegemonic subdiscipline of political science until the Second Republic.

Public policy as a subdiscipline of political science has an even longer genealogy than public opinion, although it was more than a decade later in emerging as a hegemonic subdiscipline. The study of public policy begins, of course, with the study of legislation, whose history is usually traced out from divine law through common law to something called positive law, to indicate the demystification of law and the deliberateness of modern laws. There is, then, one later stage called public policy, indicating the intervention of administration between legislature and citizen. Public policy is a term of art reflecting the interpenetration of liberal government and society, suggesting greater flexibility and reciprocity than such unilateralist synonyms as *law, statute, ordinance, edict*, and so on. Public policy began to gain some currency in public administration in the 1930s; and public administration had been one of the hegemonic subdisciplines in the political science of the stateless polity I refer to as the First Republic. The decline and transfiguration of public administration gives us the key to public policy. Traditional public administration was almost driven out of the APSA by the work of a single, diabolical mind, that of Herbert Simon. Simon transformed the field by lowering the discourse. He reduced the bureaucratic phenomenon to the smallest possible unit, the decision, and introduced rationality to tie decisions to a system—not to any system but to an economic system. His doctorate was in political science; his Nobel award was in economics.

Now, Simon did not accomplish this all by himself. His intellectual tour de force was made possible by actual changes in the administrative institutions of the Second Republic. Administrative authority in the First Republic partook of a fairly well established tradition of separating public from private life by a variety of legal rules and procedures that comprise what Joseph Vining calls the "masterful myth of the 'rule of law' "; in the Second Republic, these rules and myths broke down—not spontaneously, but in face of the rise of economic thinking in the corporate world, as well as in government (1978, 27).

It is in this context that modern public policy became a hegemonic subdiscipline in political science, overshadowing behavioralism itself. The study of public policy in the political science of the First Republic drew upon public law and institutional economics. Some of that old-fashioned public policy study exists today. But the modern approach is more appropriately called public policy analysis, which draws upon macroeconomic methods and economic systems thinking. The best way to demonstrate the size and character of this new subdiscipline of political science is to point to the presence of the policy analysis courses within political science departments and the explosive growth of the separate policy analysis programs and the economics requirements in the schools of public affairs and public policy and in the law schools. All the students in those places are learning the new language of the state.

It does no disservice to the subdiscipline of public choice to tie it to another of Karl Mannheim's observations dating from 1929, namely, that in the political science of a bureaucratic state "an economic man, a political man, etc., irrespective of time and race, could be constructed on the basis of a few axiomatic characteristics" (1936, 167–68). Mannheim continues: "Only what could be known by the application of these axioms was considered as knowable. Everything else was due to the perverse 'manifoldness of the real', concerning which 'pure' theory need not worry itself" (p. 168). Compare this to Kenneth Arrow's assertion made in a boastful spirit nearly 40 years later that any assumption other than the rational actor leads to mysticism and irrationality (Goldfield and Gilbert 1990, 14–15). This gives us a start toward a political explanation for why public choice has become probably the hottest thing going in political science today.

Quite aside from whatever merits it may have as a method and however true its truths may be, public choice is hegemonic today for political reasons or (to be more dignified about it) for reasons of state. Let me dramatize this in quite tangible rational actor terms: most of the luminaries in this subfield of political science came from, serve in, or are substantially associated with the same freshwater universities that kept burning the flame of laissez-faire ideology: Chicago, Rochester, Washington University of Saint Louis (nor should we overlook the Saint Louis Federal Reserve staff) (Johnson 1991). Here, again, we are confronting not political opportunism but institutional consonance—a symbiotic relationship between state and political science.

The affinity between modern bureaucratic government and economics—already strong—was further strengthened by the revival of the political popularity of laissez-faire ideology within the Republican party. For most of this century, laissez-faire liberalism (erroneously called conservative) had been the Republican party's center of gravity; but after the Depression, it had had little effect on voters and even less on intellectuals in the social sciences. Few intellectuals figured in national Republican party circles. *Conservative intellectual* was just another oxymoron. Today, of course, Republican administrations are overflowing with intellectuals, as are affiliated think tanks and the op-ed pages of the major newspapers. I see no signs yet of a Republican takeover of the APSA; but I do see one beneficiary of the Republican party era in political science, namely, public choice. People of merit inhabit this subfield, but its hegemony has little to do with their merit. Their success as a group was entirely fortuitous.

We political scientists enjoy the primitive wisdom of Mr. Dooley; and probably Mr. Dooley's best-known scientific proposition was, "No matter whether th' constitution follows th' flag or not, th' supreme court follows th' iliction returns." A more dignified Dooley would say, "The APSA follows

Leviathan." I conclude with what are to me the three principal consequences of following Leviathan too closely. First, we have as a consequence failed to catch and evaluate the significance of the coming of economics as the language of the state. Second, we have failed to appreciate how this language made *us* a dismal science like economics. Third, having been so close to Leviathan, we failed to catch, characterize, and evaluate the great ideological sea changes accompanying the changes of regime.

First, then, why economics? Of what use is economic analysis to politics? Since economics was always a deeply flawed predictive science, why was it so attractive to policymakers and bureaucrats in the new state? Why was it so attractive to political science? My evaluation was inspired in part by an observation made 30 years ago by the distinguished economic philosopher Joan Robinson: "Economics . . . has always been partly a vehicle for the ruling ideology of each period as well as partly a method of scientific investigation" (1962, 1). My answer is that economic analysis is politically useful because it closes off debate, especially in a highly public representative assembly like Congress. The rise of economics as the language of the state parallels the decline of Congress as a creative legislature. (I argue this point more extensively in Lowi 1991.) Policymaking powers are delegated less to the agency and more to the decisionmaking formulas residing in the agency. The use of economic analysis to close off debate was strengthened as Republicans discovered that economic analysis could be used as effectively for them as for the Democrats—by manipulating the cost, rather than the benefit, side of cost–benefit analysis. I recommend John Schwarz's evaluation of Murray Weidenbaum's outrageous manipulation of the "costs of regulation" that supported the Reagan administration's commitment to deregulation (1988, 90–99). But I must confess that both the Democratic and the Republican politicians were smarter than the political scientists, because they took the stuff as weaponry, while we took it as science. We swallowed economics before subjecting it to a political analysis.

We should have seen that economics rarely even pretends to speak truth to power. If substantive truths were claimed, there would be room for argument. But economics, particularly as a policy science, stresses method above all. And the key to the method is the vocabulary of economics, which is the *index*. An index is not a truth but an agreement or convention among its users about what will be the next best thing to truth. M_1, the Dow, the CPI, unemployment, GNP: This is the new representative government—an index representing a truth. Indices have analytic power because they fit into defined systems; and of course, systems are also not truths but only useful fictions. (This, by the way, is not an attack on indices *or* systems. It is just a political evaluation of indices and systems.)

Now to the second of my consequences, that is, that the modern bureaucratic state has made political science just another dismal science. By *dismal* I do not mean merely the making of gloomy forecasts, in the Malthusian tradition: I mean the absence of *passion*.

During my pilgrimage, the most frequent complaints I heard were against the *American Political Science Review*. I join in at least one of these, which I do not limit to the *APSR*. Too few of the articles seek to transcend their analysis to join a more inclusive level of discourse. There is consequently little substantive controversy. The response is that a scientific journal must be dedicated to replication and disproof. But actually, very few pieces independently replicate anything; and even if they did, replication alone is dismal stuff. Political science is a harder science than the so-called hard sciences because we confront an *un*natural universe that requires judgment and evaluation. Without this, there can be no love of subject, only vocational commitment to method and process. The modern state has made us a dismal science, and we have made it worse by the scientific practice of removing ourselves two or three levels away from sensory experience. Political scientists have always quantified whatever and whenever they could, and most tried to be rigorous; but they stayed close to sensory experience. Even with our original mechanical helper, the counter–sorter device, it was possible to maintain a sensory relation to the data. What a pleasure it was to watch the cards seek their slots! And what a pity today that the empiricists have only their printout!

Finally, I turn to our failure to catch or evaluate adequately the ideological sea changes accompanying the changes of regime. Time permits only the barest inventory of missed opportunities, but I think they will speak for themselves.

The perspective of nearly 50 years makes it easy to see what we did not catch about the New Deal as a regime change. Although political scientists caught the new liberalism in the air, they failed to evaluate whether all the elements of this ideology were consistent with liberalism or with constitutionalism. They failed, for example, to capture and evaluate the significance of "administrative law." They noticed, but merely celebrated, the delegation of power from Congress to the executive branch. At the time it meant only the fulfillment of the New Deal program. Even as time passed, our tendency was to render each change consistent with our existing model of the political system. There was virtually no serious political science inquiry into whether the changes in constitutional doctrine, governmental structure, and policy commitments constituted a regime change. Some Republicans suggested that the United States had become a socialist regime, but political science did not respond to this challenging formulation. It should be a matter of ultimate interest, as well as enjoyment, to fight intensely over the identification of criteria for determining when a political change is sufficient to constitute a regime change. The New Deal helped give us a new political science but did not provide sufficient inclination to evaluate what was new.

We are at this moment in the presence of another

failure, namely, a failure to catch the nature and significance of the ideological shift accompanying the current Republican era. The inability of the Reagan administration to terminate any important New Deal programs should at least have led to a reflection on the nature of the New Deal as a regime change. Even a post hoc evaluation would be useful. Meanwhile, the Republican era has brought with it some profound ideological changes that political science is failing to capture even though our own public opinion polls are sensing them. Political science has failed to catch and evaluate the two separate components of the Republican coalition: the old, laissez-faire liberalism and the genuine, native conservatism. Political science has stood by and permitted Republican candidates and staff intellectuals to treat the traditional laissez-faire core of the Republican party as conservative and then to compound the felony by stigmatizing liberalism as an alien belief system akin to socialism. This profound misuse of rich terminology is literally poisoning political discourse in the United States, and political science has to take a lot of the blame for this. We also did not catch the rise of the genuine conservatism; although our polls were picking up significant reactionary movements, we continued to treat the Falwell phenomenon and such predecessors as the Christian Anti-communist Crusade as aberrant. And we have passively witnessed the joining of laissez-faire liberalism with genuine right-wing conservatism as though they are consistent in their opposition to big government. Laissez-faire liberal Republicans, supported by their economists, embrace an ideal of radical individualism and view all government as a threat to freedom. In contrast, genuine conservatives are not individualists but statists. The state they want consists of tight and restrictive police control by state and local governments, but they are statists nevertheless. Genuine conservatives were never really at home with purely market relations; and they have never espoused the ideal (much less the methodology) of rational individualism. (Many conservative Catholic lay intellectuals have tried in vain to establish a comfortable concordance with free market liberalism.) Conservative intellectuals are now writing the poetry of executive power and are the authors of most of the writing that bashes Congress and the politics of representative government. Just as political scientists did not catch the ideological significance of the propresidential power writings of the New Deal supporters in the 1950s and early 1960s, we are now not catching the significance of the fact that most of the current propresidential power writing is by the Far Right. The far right-wing intellectuals are also writing a significant proportion of the new work on the founding intended not only to contribute to historical scholarship but to reconstitute the constitution in such a way as to place the presidency above the law and affirmative action beneath it.

No effort has been made to camouflage my antagonism to Republican era ideology. But my own personal position is irrelevant. Political scientists of the Left, Right, and center are a unity in their failure to maintain a clear and critical consciousness of political consciousness. Causal and formal analyses of the relations among clusters of variables just will not suffice. Nor will meticulous analysis of original intent. It is time we became intellectuals.

At the end of my pilgrimage, I have come to the conclusion that among the sins of omission of modern political science, the greatest of all has been the omission of passion. There are no qualifications for membership in the APSA; but if I had the power to establish such standards, they would be that a member should love politics, love a good constitution, take joy in exploring the relation between the two, and be prepared to lose some domestic and even some foreign policy battles to keep alive a positive relation between the two. I do not speak for the passion of ideology, though I do not count it out. I speak for the pleasure of finding a pattern, the inspiration of a well-rounded argument, the satisfaction in having made a good guess about what makes democracy work and a good stab at improving the prospect of rationality in human behavior.

Regime changes throughout the world since 1989 ought to give us a clearer perspective on some new sciences of politics. Although only a few of the world's regime changes will be liberal democracies, they are stimulating tremendous demand for transferrable insights about the workings of liberal democratic institutions, especially U.S. institutions. May this demand draw U.S. political scientists out of the shadow of Leviathan upward and outward toward a level of discourse worthy of the problem. This is not an opportunity to play philosopher–king. It is an opportunity to meet our own intellectual needs while serving the public interest. And we need not worry how to speak truth to power. It is enough to speak truth to ourselves.

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DEMOCRATIC THEORY AND SELF-TRANSFORMATION

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Democratic theories that argue for expanding the scope and domain of democracy assume that democratic experiences will transform individuals in democratic ways. Individuals are likely to become more public-spirited, tolerant, knowledgeable, and self-reflective than they would otherwise be. This assumption depends on viewing the self as socially and discursively constituted, a view that contrasts with the standard liberal-democratic view of the self as prepolitically constituted and narrowly self-interested. The importance of the social and discursive view of the self is that it highlights how standard assumptions about the self help to justify limits to democratic participation. As now conceptualized, however, the transformational assumption does not meet standard objections to expanding democracy. I sketch an approach that distinguishes classes of interests according to their potentials for democratic transformation, and strengthens—by qualifying—transformative expectations in democratic theory.

Within the discourse of democracy there is a pervasive split between two broad groups of theories. One group seeks to balance democratic participation against other desirable features of political order (e.g., protections of rights and freedoms, pluralism, and governability) by limiting the spheres of society that are organized democratically. The other group of theories sees such limits to democracy as an important cause for many of the ills of contemporary liberal democracies. As is well recognized, the disagreement stems in part from differing assumptions about the self and its relation to politics (Barber 1984, chap. 4; Bowles and Gintis, 1986, chap. 5; Cunningham 1987, 188–93; Dryzek 1990, 13–15; Gould 1988; Macpherson 1977; Mansbridge 1980, chap. 2; Mansbridge 1990a; Offe 1985, chap. 7; Pateman 1970). Theories that see trade-offs between democracy and other goods often hold that the self is defined by interests that are formed prepolitically, either reflecting fixed desires or formed by social institutions and other circumstances that are outside of institutionalized politics. Democracy, on this view, is primarily a means for aggregating prepolitical interests and should be limited in scope and domain just because it is instrumental to prepolitical interests and not a good in itself. I shall refer to these theories as *standard liberal democracy*.

In contrast, theories that argue for increasing the scope and domain of democracy (*expansive democracy*) hold that standard liberal democracy fails to articulate goods that are inherent in democracy and exaggerates the threats posed by democracy to other goods. On this view, these limitations follow from a more general failure of standard liberal democracy to appreciate the transformative impact of democracy on the self, a failure rooted in its view of the self as prepolitically constituted. On the expansive view, were individuals more broadly empowered, especially in the institutions that have most impact on their everyday lives (workplaces, schools, local governments, etc.), their experiences would have transformative effects: they would become more public-spirited, more tolerant, more knowledgeable, more attentive to the interests of others, and more probing of their own interests. These transformations would improve the workings of higher-level representative institutions, as well as mitigate—if not remove—the threats democracy is held to pose to rights, pluralism, and governability.

In making this argument, expansive democrats rely on three assumptions that run counter to standard liberal democracy. The first is that increased democracy transforms individualistic and conflicting interests into common and nonconflicting ones, in the process developing capacities of citizenship that reduce factional threats to rights and pluralism. Second, because these transformations reduce conflict, they allow reduced use of power as a medium of political interaction. This would increase consensus and governability, as well as being desirable in its own right. Third, far from being a threat to the dimensions of the self protected by rights and freedoms, democracy is necessary to the values of self-development, autonomy, and self-governance—the values that rights and freedoms presumably are designed to protect. Together, these assumptions make up what I shall refer to as the *self-transformation thesis*.

I shall examine the self-transformation thesis and the conceptions of the self that sustain it. My general argument is that when the thesis is properly identified, qualified, and refined, it is both defensible and essential to democratic theory. My support for the thesis is, however, critical. As now presented in the literature, it does not meet its burden of proof. More specially, I argue that the different kinds of attachments that constitute the self present different kinds of transformational problems. I sketch an approach that distinguishes classes of interests (expressions of these self-defining attachments) according to their potentials for transformation in nonconflicting ways and identifies transformations that are consistent with, or increase, the autonomy (capacity for self-direction) and development of the self. Thus, I identify spheres of interest within which expansive dem-

ocratic theory ought to locate its transformative expectations, as well as spheres within which these expectations are undesirable. In the latter, democratic theory should recommend institutions designed to deal with conflict and based on decision rules that do not require transformations of the self.[1] I intend these refinements to address common objections to the self-transformation thesis, as well as to suggest new directions for expansive theories of democracy.

ASSUMPTIONS ABOUT THE SELF IN DEMOCRATIC THEORIES

With regard to assumptions about the self, democratic theory falls into two broad groups. *Standard liberal democracy* includes a number of otherwise diverse theories, including those of Madison and James Mill and the pluralist–elite theories of Weber and Schumpeter, as well as American pluralism and neoconservatism. While these theories differ in important respects, here I consider them together: they all assume that democracy is primarily of instrumental value for protecting or realizing the interests that belong to individuals or groups independently of political life. That is, standard liberal-democratic theorists conceive interests as prepolitical preferences. Political institutions aggregate preferences, but they do so without fundamentally changing them. Of course, these theories recognize as an empirical matter some transformative effects (as when leaders develop a sense of public responsibility or cooperative norms emerge between adversaries in Congress). But these recognitions are ad hoc and not theorized in ways that could help to identify transformative possibilities in other arenas of political life.

These characteristics of the standard view are reinforced by a more general assumption about the nature of politics. Politics, on this view, is an allocative or economic kind of activity, operating in a world of scarce values. For this reason, the paradigm of standard liberal democracy limits its conception of politics to scarcity-induced conflict. Its problems have to do with protecting individuals from excesses of political power, governing in the face of conflicting demands, and finding grounds for obligation to political order in prepolitical interests. Following from this way of problematizing politics, standard liberal democrats argue for institutional limits on democracy, seeing expansions of democracy as potential threats to other goods—especially freedom, pluralism, governability, informed decision making by political elites and experts, and efficient delivery of governmental goods and services.

Expansive democracy includes participatory democracy, democratic socialism, and the more radical strains of liberal democracy that stem from Rousseau, John Stuart Mill, T. H. Green, and John Dewey. These theories argue for increased participation in, and control over collective decision making, whether by means of direct democracy in small-scale settings, or through stronger linkages between citizens and institutions that operate on broader scales. They are often concerned with expanding democracy beyond traditional political spheres into economic and social spheres, that is, into arenas of power that are privatized in standard liberal democracy. Whereas, in the standard view, *democracy* most often refers to institutional processes such as competitive elections, in expansive theories it refers more broadly to the effects of institutions in increasing individuals' control over self-determination and self-development. This is why expansive theories recommend relating decision making to the persons who are affected whenever possible. Closely related, these theories assume that democracy has more than an instrumental value to ends such as freedom, protection of private satisfactions, security, and order. Democracy generates values that are intrinsic to political interaction and closely related to self-development. Limiting democracy to, say, electoral competition between elites (Schumpeter 1942, chap. 22) deprives nonelites of access to conditions of their own development.

Theories of expansive democracy should not be confused, as they often are, with communitarian critiques of liberalism (MacIntyre 1981; Sandel 1982; Sullivan 1982; Taylor 1979). Expansive democrats do share with communitarians an appreciation of the embodied and contingent nature of the self and a concern with alienation from public life. The differences lies in their approaches to reasserting connections between self and community. Communitarians often take what Seyla Benhabib calls an "integrationist" approach, leaving ambiguous the role of political empowerment in developing new communities (1989, 383). In contrast, expansive democrats insist on the centrality of democratic empowerment, dialogue, and interaction, holding that integration without democracy threatens to sacrifice self to community (Connolly 1991, chap. 3).

STANDARD OBJECTIONS TO EXPANDING DEMOCRACY

A list of standard liberal-democratic objections to expanding democracy will help identify the burden of argument that the self-transformation thesis must bear and will also identify key assumptions about the self in the standard view.

One common objection to expanding democracy stems from Hobbesian assumptions about political judgment that were absorbed into the Madisonian, utilitarian, and even Kantian traditions and more recently assimilated by rational choice theory (Mansbridge 1990a). On this view, individuals are rational maximizers of preferences, so their political judgments will follow self-interest in ways that are likely to threaten the rights and liberties of other individuals. For this reason, institutions should be designed to separate political judgments from those of individuals; democratic decision making should be repre-

sentative, rather than direct. As Madison put it, "No man is allowed to be a judge in his own cause because his interest would certainly bias his judgment, and, not improbably, corrupt his integrity" (Madison, Hamilton, and Jay 1961, no. 10). Expansive democrats must show that democratic participation would mitigate this danger by developing individuals' appreciation of common interests, as well as their respect for the differing interests of others.

A second objection to expansive democracy follows from a related Hobbesian assumption, namely, that political interests are inherently conflicting because they are oriented toward scarce resources. On this view, politics is an allocative (or economic) activity that takes place within an environment of scarcity, an assumption incorporated into standard liberal and pluralist definitions of politics (e.g., Easton 1965, 50). For this reason, standard liberal democracy justifies institutions that are, in Jane Mansbridge's terms, essentially "adversarial" because it assumes, and is designed to deal with, interests that are conflicting. The limitation on democracy in this case is indirect: the general limitation on the sphere of political possibility rules out institutions that cultivate common goods through participatory interaction and dialogue (Elkin 1985; Macpherson 1977; Mansbridge 1980; Xenos 1987). To justify expansions of democracy in this case, expansive democrats need to show that not all activities of politics are allocative (or economic) in nature because not all politically relevant interests are oriented toward scarce goods.

Third, according to standard liberal democracy, even if resources were not scarce, it is undesirable to subject a plurality of individual ends to politics (as expansive democracy would), since doing so inevitably subjects individual wills to a general will, and this leads to coercive results (Nozick 1974, 310; Riker 1982, chap. 1; Sartori 1987, 315–20). Kant, for example, referred to democracy as "necessarily a *despotism*" because it "establishes an executive power through which all the citizens may make decisions about (and indeed against) the single individual without his consent" (1970, 101; emphasis original). Likewise, Madison feared that expanding political participation would threaten a tyranny of the majority against minorities. His solution, of course, was to limit democracy by limiting the ability of majorities to use the powers of government. Standard liberal democracy holds, in effect, that it is desirable to depoliticize as many spheres of society as possible, rather than to democratize them. Depoliticization also means limiting spheres of democracy and leaving social life to be ruled by nongovernmental powers. To deal with the possibility that more democracy will lead to majority tyranny, expansive democrats must show that democratic experiences develop individuals' respect for persons, pluralism, and freedom.

A fourth objection to expansive democracy within standard liberal democracy is most closely associated with American pluralism, and has been articulated by neoconservatives as the "demand overload" thesis (Crozier, Huntington, and Watanuki 1975). According to pluralist theory, democracy requires a balance between conflict and consensus (Lipset 1960; see also Alford and Friedland 1985, chap. 3) or, in other versions, between activism and apathy (Berelson, Lazarsfeld, and McPhee, 1954, chap. 14; Sartori 1987, 118–23). A competitive democratic process can produce a working government, on this view, only if there is an underlying consensus about the rules of the game and only if participants are willing to accept compromises and losses. Apathy and nonparticipation turn out to be functional for democracies because they limit the number of conflicting interests that democratic processes must aggregate, thereby lending stability to the system as a whole. Crozier, Huntington, and Watanuki warn that increased activism "disaggregates" interests by challenging elites who manage demand levels by filtering, articulating, and aggregating interests (1975, 165). Such challenges are likely to destabilize the very governments designed to be relatively responsive to popular pressure. Against this possibility, expansive democrats must show that extending democracy transforms interests in ways that allow for, or even increase, governability—at least where there are not deeply rooted structural conflicts of interest.

There are, of course, other objections to expansive democracy;[2] but these four follow most closely from the view of the self as a maximizer of prepolitical interests in a world of scarce resources. It is important to note, however, that it is not enough for expansive democrats to point to the implausibly static view of the self within standard liberal-democratic theory. There are two more reasons why standard liberal democrats might hold this view of the self even if they were to see it as inaccurate or incomplete.

A fifth objection to expansive democracy has less to do with the accuracy of assumptions about the self than with their proper role in democratic theories: a viable theory ought to build on the worst possible limiting cases. Once again, the point is Hobbesian: even if most people are not self-interested maximizers of prepolitical values, institutions based on more optimistic assumptions would fail even if only a few people act in this way. Expansive democrats do not take into account the political viability of inclusive institutions. Against this objection, expansive democrats need to show that they can retain the protective dimensions of standard liberal democracy against the worst possible cases, while also showing that using such cases as a standard for institutional design tends to reinforce just these cases.

A sixth objection to the transformative assumptions of expansive democracy is epistemological. Even if political interactions produced transformative effects, there are few, if any, criteria that could be used to show what an individual's interests would be in the absence of these effects and thus to distinguish desirable from undesirable transformations. To provide such criteria would only be to rely on something more arbitrary than individuals' revealed preferences. To understand political institutions as having an autonomous impact on politics (as expansive dem-

ocrats do) is to open the door to theories of false consciousness, theories that can justify Leninist vanguardism or Tory paternalism in the name of democracy. To meet this objection, expansive democrats must show that the impact of politics on preference formation cannot be understood without distinguishing interests from revealed preferences, and that such distinctions can be made in intrinsically democratic ways.

THE SELF-TRANSFORMATION THESIS

From the perspective of expansive democratic theory, these objections are to a large extent based on standard liberal-democratic assumptions about the self, assumptions that close the standard view to the full potentials of democracy. Providing an alternative account of the self, however, has so far proved more difficult than critique. Part of the reason is that expansive democratic theories also suffer from underdeveloped assumptions about the self. Nonetheless, certain of the objections I have listed are dealt with by more recent formulations of the self-transformation thesis, four dimensions of which I shall elaborate.

First, the interests and capacities that define the self are determined not only by prepolitical factors but also by the constraints and possibilities of political institutions. The selves that often seem endemic to liberal democracies—selves characterized by selfishness, apathy, alienation, lack of knowledge, and prejudice—are reflections as much of limited means for meaningful political discourse and participation as of more general limits to the capacities of citizens for self-governance. As Bowles and Gintis put it: "Interests are not causally prior to practices. Rather, the interests of a group will in general depend, among other things, upon the membership of the group, its internal discursive and structural organization, and its location in an ecological system of social actions" (1986, 149). Preferences "are as much formed as revealed in the exercise of choice. Individuals choose in order to become, and the nature of the opportunities given for the expression of choice affects the formation of the will" (p. 138; see also Barber 1984; Elkin 1985; Hirst 1986; Lindblom 1990, 17–27; Sunstein 1991).

Second, transformations of the self are important for expansive democrats because they view democracy as justified not so much because it allows maximization of prepolitical wants or preferences as because it maximizes opportunities for self-governance and self-development (Dahl 1989, chap. 7). That standard liberal democrats view "preferences as given allows us to recognize democracy's contribution to the proper aggregation of wants through the counting of votes, but it obscures the contribution of democratic institutions to human development—their unique capacity to foster in people the ability intelligently and creatively to control their lives" (Bowles and Gintis 1986, 123). Similarly, C. B. Macpherson (1977) distinguishes between the image of the self as a consumer in the Benthamite and pluralist traditions of standard liberal democracy and the self as an exerter and developer of capacities in participatory traditions. And Carol Gould (1988) argues that democracy is valuable primarily because humans value both activities that allow them to grow and develop and control over processes of growing and developing. Standard liberal democracy does not, of course, a priori rule out that individuals might be motivated by desires for self-development and self-governance. Indeed, a key justification of standard liberal democracy is that it secures private space within which such values can be realized. What distinguishes expansive democracy is the view that political participation is necessary for self-governance; self-governance is not simply a "private" matter.

This dimension of the self-transformation thesis, however, is particularly vulnerable to one objection I have referred to, namely, that a strong relationship between individual and collective governance is inherently oppressive, especially when conceived in terms of Rousseau's view that individuals become free and self-governing when they replace a plurality of different wills with a generalizable one. Some recent theorists of expansive democracy (especially those influenced by poststructuralism) have sought to recognize this difficulty without giving up the linkage between democracy and self-governance that is essential to the self-transformation thesis. Not unlike pluralists, they argue that conceptions such as the general will indefensibly include an assumption that a world of unified interests exists behind their apparent diversity (Connolly 1988, chap. 3; Connolly 1991; see also Hirst 1986). Translated into a political ideal, the general will produces an insensitivity to difference. To be sure, we may create a general will on any given issue, but it is just as likely that collective decisions will diverge from even the most considered individual preferences. In these cases, individuals maximize self-governance by identifying these differences, not by viewing the general will as their own.

But if expansive democrats give up the Rousseauian understanding of self-governance based on an identity of individual and collective will, does not democracy become simply the equal chance to influence collective outcomes, as in the standard approach? Expansive democrats increasingly argue that there remains an important relationship between democracy and self-governance that does not depend on identifying individual and collective wills, a relationship missing in the standard approach. Democratic participation has a unique capacity to foster and develop *autonomy*, a key element of self-governance (Benhabib 1986; Graham 1986, chaps. 5–6; Habermas 1989; Held 1987, 267–77). Autonomy describes a capacity of judgment: individuals are autonomous if their preferences, goals, and life plans are not the result of manipulation, brainwashing, unthinking obedience, or reflexive acceptance of ascribed roles but, rather, a result of their examining and evaluating

wants, needs, desires, values, roles, and commitments (Dworkin 1988; Frankfurt 1989; see also Dahl 1985; Elster 1983, 15–23; Mansbridge 1980, 24–25; and Warren n.d.).

Although standard liberal-democratic theory places a high value on autonomy, it conceives autonomy as a prepolitical capacity, maximized by clear separations of private and public life. For expansive democrats, however, autonomy is an inherently social capacity that individuals develop through their interactions with others, by coming to know others both as separate human beings with their own unique capacities, problems, and interests and as beings with whom one shares at least some experiences, problems, and interests. Ideally, everyday interactions would cultivate within individuals the capacity to distinguish the wants, desires, and commitments that lend coherence to their identity from the wants, desires, and commitments that they have, perhaps uncritically, accepted from their culture and may experience as a source of unhappiness. The process of self-discovery (or self-creation) should not, however, be viewed as reinforcing narrow selfishness. On the contrary, a narrowly selfish individual would probably discover a life entangled with others in ways he or she previously did not understand and an identity dependent on commitments and responsibilities to publics, communities, and groups in ways he or she previously did not recognize.

Nor does autonomy imply that one could engage in critical judgment in isolation from others. This is why expansive democrats see participation as important: democratic discourse, negotiation, challenge, compromise, and consensus building foster autonomy. Autonomy is not only desirable for its own sake but essential to the mechanisms of representation and voting that make large-scale democracies possible, since the legitimacy of these mechanisms depends on people's knowing what they want (Sunstein 1991, 10–13).

The expansive argument has become more Tocquevillean than Rousseauian: democracies ought to distribute power in ways that multiply public spaces. These, in turn permit claims and arguments to be publicly tested, altered, and justified (Habermas 1989). This formulation takes a step toward meeting the objection that there is no way to distinguish between preferences and interests without paternalistic or Leninist conceptions of false consciousness. Only democratic implications follow from the view that individuals have interests (no matter what their preferences) in institutionalized processes that enhance their autonomy and self-development (Warren 1990).

A third dimension of the self-transformation thesis is closely related: democracy has an intrinsic, as well as an instrumental, value. Although some values of democracy are means to nonpolitical values, others grow out of democratic processes themselves. Participation completes individuals, in part by enabling them to discover and develop their public dimensions, in part by providing the kinds of interactions that develop capacities for autonomous judgment. Jürgen Habermas has developed this dimension of the self-transformation thesis as carefully as anyone, arguing in *The Structural Transformation of the Public Sphere* (1989) that liberal democracy requires—but is increasingly devoid of—public spheres in which opinions can be put to the test of discourse and changed, and public wills formulated. The point of democracy, in Habermas' view, is not simply to reconcile conflicting interests (as in standard liberal democracy) but to design institutions that encourage discourse, which, in turn, is necessary to identify and distinguish plural, common, and emergent interests (Habermas 1975, 108). Without the experience of argument and challenge within democratic public spheres, individuals will have little sense for what relates them to, and distinguishes them from, others; and this deprives them of an essential condition of self-development. By raising one's wants, needs, and desires to the level of consciousness and by formulating them in speech, one increases one's sense of identity and autonomy—aside from any advantages that might accrue from the substantive outcomes of collective decisions. To the extent that democracy is rooted in discourse, it produces (not just mediates) individual and public wills—an idea with roots in Tocqueville, articulated by J. S. Mill and Dewey, and finding contemporary expression not only in Habermas, but also in Hannah Arendt and others (Barber 1988; Benhabib 1986; e.g., Cunningham 1987, 189–90, 261–63).

The self-transformation thesis includes a fourth argument as to why we should expect increased democracy to enhance governability rather than (as in the standard view) threaten the overload and breakdown of democratic processes. On the expansive view, the "demand overload" thesis overgeneralizes from contemporary liberal democracies, which lack institutional encouragements for transformational effects and rely almost exclusively on adversarial mechanisms that encourage conflict (Barber 1984; Burnheim 1985; Habermas 1989; Mansbridge 1980; Offe 1984, chap. 2). In contrast, increased participation is likely to encourage substantive changes in interests in the direction of commonality, transforming conflict in the direction of consensus. Assuming that structural causes of conflict (inequalities of wealth and power) could also be mitigated, these transformations are most likely to occur within frameworks that encourage interaction and discussion: "The unperceived effects of participation upon individuals' outlook and personality, and upon their development into social beings—make the cost-benefit calculus for self-interest inappropriate" (Bachrach 1975, 52; see also Barber 1984, 151; Graham 1986, 152). But even if individuals fail to discover common interests, they are likely to learn about reciprocity (Gould 1988, 296–97, 303–4); and failing that, they may learn about tolerance. As Frank Cunningham puts it: "The values of respect and tolerance are initially learned by people seeking the same goals but differing on means, and they can then be extended to respect and tolerance for people

sharing some goals but disagreeing on others. Democracy-inhibiting prejudices (racial, sexist, or national chauvinist) thrive on mutual isolation but begin to be called into question in the interaction that participation in joint projects facilitates" (1987, 76–77; see also Barber 1984, 153). For these reasons, we should expect democratic participation to transform individuals in ways that increase both governability and autonomy.

STRENGTHS AND LIMITATIONS OF THE SELF-TRANSFORMATION THESIS

The successes of the self-transformation thesis are primarily critical: the thesis points up the assumptions about the self in standard liberal democracy that close off explorations of democratic possibilities. As a set of positive claims, however, the thesis is not sufficiently developed to support an alternative democratic theory. For example, it is doubtless true that participation enlightens individuals, since it is hard to engage in an activity and not come to know more about it. But it has not gone unremarked that an active citizenry might become more enlightened without becoming more public-spirited, appreciative of commonality, and tolerant of difference (Dahl 1989, 92–93; Held 1987, 263; Mansbridge 1990b). Under what conditions would democratic interactions produce reciprocity, tolerance, and interest in social goods, rather than, say, providing new ways of politicizing resentment and intolerance? Why should individuals not simply become more effective at pursuing narrow self-interests? Similarly, we know that discourse is a good way to resolve political issues; but why should we also expect it to transform conflicting interests, rather than simply communicating preferences?

Questions such as these are all the more compelling when we consider the pressures of politics against self-transformation that I have noted. We have good reasons to expect consensual decision making, reciprocity, tolerance, and equal respect in institutions such as voluntary organizations, closely knit communities, friendship, and some families. And we tend to see these institutions as pathological when they do not cultivate such values. But politics has an unchosen and conflictive character and often proves to be an environment hostile to transformations such as these. In politics we are thrown together because we live at a particular time in a particular place and because we are confronted with common imperatives for action. Even under the best of circumstances, discourse is likely to be limited in duration and thus in transformative potential—if not because of immediate imperatives, then because there are so many imperatives worthy of discourse and time itself is a scarce commodity (Elster 1983, 37–40; Sirianni 1981, 62–63). No wonder standard liberal democrats presume conflict, then read their concept of the self off of this presumption! No matter how impoverished the concept of the self in standard liberal democracy, it may still seem more appropriate for the *political* sphere than alternative conceptions, not so much because of qualities of the self as a whole as because politics seems to select for precisely its fixed, possessive qualities.

It is unlikely that expansive democracy can deal satisfactorily with these issues as long as it lacks an account of the variety of interests and entanglements that link self-definition to public life. I do not mean that there is no image of a democratically transformable self in these theories. On the contrary, this image is increasingly sophisticated: such a self would be autonomous yet social, individuated yet defined by nonconflicting interests, rational but embodied in numerous different social relations, expressive of individuality yet public in orientation. I doubt neither that such selves are possible nor that we often fail to recognize these dimensions of the self when they in fact develop. The problem is that there is as yet no account of the self that would support this image as it relates to politics. What we find instead are various reactions to the privatized and static conception of the self in standard liberal democracy, each of which tends to emphasize one or more dimensions of the self missing in the standard image. The result is too often an image of the self that mirrors the standard image and is thus unable to reformulate those activities and spheres of interest from which the standard image draws its strength.

The Oversocialized Self

The most widely shared reaction to the privatized concept of the self is to emphasize the inherently social character of the self. Expansive democrats point out that the self is constituted by social relations, identities, and supports. It develops through its social relations and practices, any one of which might provide occasions for mutual interest. Whereas the standard view is that political actions reflect prepolitical interests, expansive democrats point out that different kinds of social practices and identities produce different definitions of interest. Standard liberal democracy reifies outcomes in which individuals are formed by hierarchical or anomic social relations. Were these relations to change, replacing bureaucratic hierarchies and anomic market relations with democratic relations (wherever feasible), self-identities would change in ways that underwrite expansions of democracy (Barber 1984, 153; Bowles and Gintis 1986, 150–51; see also Sunstein 1991).

In the best cases the socialized conception of the self does not rely on an identity between the individual and the collective (as might a traditional conservative or a communitarian view). Nor does it rely on a mechanistic account of social determination such that individual capacities for autonomy become incomprehensible. As I have suggested, these are problems for which there exist defensible positions in theories of expansive democracy. But behind even the best cases is often an assumption that demonstrating the social

origins, relations, and dependencies of the self is sufficient to show that conflicting interests are transformable into common ones.

The assumption is based on two kinds of conflations. The first conflation is the idea that social interdependencies reflect mutual interests, which in turn provide a basis for self-transformation in the direction of common interests. Of course almost everyone would recognize that there are many social relations within which individuals are mutually defined without having mutual interests (e.g., relations of domination). But there are also interests that despite their being encumbered with social interdependencies, cannot be transformed in nonconflicting directions without oppressive consequences. This is because they are defined (as standard liberal-democratic theory assumes) by scarce goods or goods that by their very nature can only be enjoyed by individuals or small groups. Self-defining interests have a potential for politically significant commonality only where the value of their defining goods depends on large-scale interaction or where they can only be provided in common. This is not to say, of course, that we do not have common interests in agreeing to distributive principles for goods that are intrinsically conflicting but only that in doing so we ought to mediate, rather than transform, interests in these goods. The point suggests that assuming the possibility and desirability of self-transformation in some spheres of interest does indeed risk the oppressive commonality that standard liberal democrats (as well as postmodern pluralists) fear. Expansive democrats should assume that conflict is a constant potential in such spheres and that it is undesirable to mitigate these conflicts except through impersonal mechanisms (see Mansbridge 1980).

The second conflation is a tendency to argue from the reciprocity and solidarity evident in many social relations to politics without recognizing the distinctiveness of political interactions. The view that the self is socially constituted in ways that produce mutual interests is supported mostly by the evidence of child rearing, schooling, friendship, voluntary organizations, workplace cooperation, and various kinds of group solidarity in collective actions. These kinds of social relations do provide important insights into transformations of the self, and they suggest that the range of possible transformations of political interests is far wider than standard liberal democracy assumes. But the analogy between social and political relations is complicated by the fact that these social relations are usually based on common interests (and self-select for common interests), so that conflict is often exceptional and (when it does arise) may be resolved simply by reaffirming common interests. In contrast, we tend to categorize social relations as "political" just when they have a potential for conflict in the face of necessary action. In such cases, common interests have to be created and discovered against a background presumption of conflicting interests. Social and political relations are indeed closely connected; but they are not homologous, suggesting that the self-transformation thesis needs to be reconceived in terms of the distinctive qualities of politics. A beginning would be to classify self-defining interests according to their differing potentials for commonality, conflict, and transformation.

The Overly Discursive Self

A related but distinct innovation in expansive democracy is the notion that the self is discursively constituted. If the self is discursively constituted, then the linguistic medium of politics has transformative potentials that do not show up in standard liberal-democratic theory. Consistent with its origins in the Enlightenment, the standard approach places a heavy burden on language by viewing argument as a substitute for power in collective decision making (Habermas 1989). But it also views language in instrumental terms: at most, language can express prepolitical interests and facilitate discoveries of prepolitical commonalities. Because the standard view does not see language as having constitutive features, it cannot understand how language might create new interests through its many roles in constituting the self.

In contrast, expansive democrats often argue that language serves not only the instrumental functions recognized in standard liberal democracy but also serves to explore mutuality, secure noncognitive relations of affection and affiliation, differentiate and maintain individual autonomy, express self-identity, and reformulate interests (Barber 1984, chap. 3). Each function suggests that language has transformative capacities rooted in its pervasive role in translating internal desires into consciousness and consciousness into something that can be communicated to others. As Habermas puts it:

> Language functions as a kind of transformer; because psychic processes such as sensations, needs and feelings are fitted into structures of linguistic intersubjectivity, inner episodes or experiences are transformed into intentional contents—that is, cognitions into statements, needs and feelings into normative expectations (precepts and values). This transformation produces the distinction, rich in consequences, between the subjectivity of opinion, wanting, pleasure and pain, on the one hand, and the utterances and norms that appear with a *claim to generality* on the other. (Habermas 1975, 10; emphasis original).[3]

Considerations such as these are enormously promising for the self-transformation thesis. They suggest an intrinsic relationship between the linguistic medium of politics and a community of understanding that involves shared commitments—even as conditions for the instrumental and manipulative uses of language typical of politics (Habermas 1984–87, vol. 1). The mistake that theorists of expansive democracy are prone to make in incorporating insights such as those of Habermas, however, is to read a teleology of common interests out of the commonalities inherent in communication. Habermas himself

does not presume common interests (1975, 108); but when Habermasian views of transformative potentials of language are combined with the view that the self is discursively constituted (e.g., Benhabib 1986, 338), then the conflation follows automatically. Similar consequences follow from relating poststructuralist views of self and language to democratic theory (e.g., Laclau and Mouffe 1985, chap. 4).

The notion that the self is discursively constituted is important but incomplete. It is one thing to note that self-identity is forged in the medium of language but another to identify the self with its linguistic constitution. Identity cannot escape language, and all identity may be (as poststructuralists argue) linguistic in nature. But the self is not just what we can articulate linguistically as its identity. It is also constituted by inarticulate strivings, desires, needs, compulsions, and sentiments, as well as connections to the world that escape language, such as nonverbal communications and sensuous, aesthetic, and spatial relations. One of Freud's most important insights (one that does not depend on the success or failure of psychoanalysis) was that every articulated self-identity leaves a residue. There is always a dislocation between the part of the self made up of an ensemble of desires and strivings and the articulate self, whose identity is formed by the roles that a prevailing discourse with its associated power relations has to offer (citizen, soldier, patriot, mother, boyfriend, caretaker, worker, boss), as well as by everyday rules of social interaction. In Freud's view, residues of the self are in fact signified—but in ways that make them incommunicable. Their significations are scrambled and nonsensical, an effect of their nonidentity with the public domain of language. Yet these dislocations can be accessed through discourse precisely because one's identity will indicate dissatisfactions not only with the fit between one's communicable identity and inarticulate discomforts of the body but within language itself—often through the strivings of the body to become articulate (Gendlin 1987).

We find parallel processes at the political level in discourses that marginalize groups with distinctive experiences, life-styles, and misfortunes (Foucault 1979; Scott 1985). Discursive marginalization may produce adjustments—attempts by these groups to normalize themselves by identifying with a dominant discourse. But such processes will also produce experiential residues that might become resistances. This is where, in fact, we find the importance of democratic discourse in relation to self-transformation. It is not so much that discourse constitutes the self as a subject of interests as that it provides successive identities, which in turn produce new residues and new interests (see Connolly 1991). Discourse can test strivings and desires; play on contradictions and discomforts; and thus articulate, distinguish, and transform interests. But discourse is also bounded by, and partly dependent for its meaning upon, nondiscursive relations to the world and incommunicable inner strivings. Just for this reason we should not expect that the intersubjective qualities of language will, by themselves, constitute selves that are possessed of generalizable interests. Language does not have these powers; and that is why a theory of discourse is not a theory of self-transformation. We can conclude only that the discursive dimensions of politics provide a medium of self-transformation should there be pressures, interests, motives, or strivings in that direction. This locus of discourse is neither appreciated by standard liberal-democratic theory nor properly articulated by expansive democratic theory.

The Underpluralized Self

Theorists of expansive democracy are often guilty of a silence on the question of how pluralistic self-identities relate to common interests and imperatives for political action. More specifically, under what conditions does distinctiveness generate conflict? Theorists of expansive democracy are right to assume that it is not necessary for distinctiveness to generate conflict and to reject the terms in which the issue is couched in standard liberal-democratic theory, that is, the view that individual and common interests have a zero-sum relation to one another except in cases where there are cooperative solutions to maximizing individual preferences (Cunningham 1987, 186–93; Elkin 1985). Moreover, theorists of expansive democracy are right when they make the Hegelian point that even distinctions are products of commonality. One must learn a common language to say distinctive things; one must enter into reciprocal relations of support with others as a condition of developing one's own capacities.

But expansive democrats too often assume that because commonality is necessary to difference, conflicting interests can be transformed into common interests without sacrificing pluralism (see Hirst 1986, chap. 1). This assumption is found, for example, in Bowles and Gintis's argument that markets ought to be eliminated because they allow problems to be solved through "exit" from collective decisions, rather than "voice"—thus undermining a "vibrant democratic culture" (1986, 135). There are many things wrong with markets; but that they allow for alternative ways of solving problems is not one of them. Bowles and Gintis's argument is antipluralistic because it assumes that underlying commonalities are so much more important than differences that they justify political blocks to exit from politics (see Cunningham 1987, 46; Elster 1983, 37).

Assumptions such as these keep us from asking key questions about the relation between commonality and pluralism. When does commonality repress desirable and politically benign differences? When do differences produce antagonisms that demand political responses? Can political life generate and sustain desirable and nonantagonistic distinctions? Sorting out differences, locating their associated values, and identifying the conditions under which they are antagonistic is essential if theories of expansive democracy are to do more than indicate the value of plural-

ism (see Connolly 1991). Doing so requires an account of the self that relates interests to commonality and plurality.

THE SELF AS A LOCUS OF INTERESTS

I shall offer some preliminary considerations about how we might think about issues of the self in democratic theory. I do not offer a theory of the self but (less ambitiously) a problematization of the self from a political perspective, oriented toward the issues of conflict, commonality, and consensus that divide the standard and the expansive approaches to democracy. I shall remain within the liberal-democratic tradition of conceiving the self as a locus of interests. The concept of *interest* will remain useful for democratic theory, however, only if we take into account certain of its attributes that are often lost in contemporary usage.

First, the term is irreducibly normative. Identifying the term with revealed preferences (as do many standard liberal-democratic theorists) removes the normative content of the concept in ways that have caused some expansive democrats to argue that it no longer has a useful place in democratic theory (Bowles and Gintis 1986, 149–50). But in spite of recent usage, the term identifies a normative problematic that remains essential. By viewing the self as in some way constituted by "interests," we implicitly view the self in light of its political competence, actual or potential. We say, in effect, that the self ought to be able to identify its needs and wants and communicate them to others (Connolly 1983, 64–68). Conceiving of the self as a locus of interests, then, is closely related to the assumption within democratic theory that individuals have a basic interest in their own autonomy. Recognizing that needs and preferences often do diverge (and that this diminishes autonomy), a theory of democracy should be oriented toward creating institutional environments that encourage the self-examination of preferences that brings them closer to needs (Bachrach 1975, 40; Offe 1985, chap. 7). One might even say that the concept of interests, when elaborated, includes the democratic problematic of self-transformation in the direction of enlightened political competence and autonomy (Dahl 1989, 180; Connolly 1983, 62–70).

Second, conceiving of the self as a locus of interests also locates the self within a structural context of conflict and consensus. An interest is always nonspecific until it is matched to a good, as in "I have an interest in X." If these goods relate to, or depend upon, other humans (and most do), then the interest has a potentially political nature, allowing us to formulate questions about commonality and conflict by looking at differing characteristics of these goods. This formulation also shows that conceiving the self as a locus of interest does not mean that interests express qualities of a prepolitical self. Rather, interests refer to the complex of relations through which the self defines itself by holding in view the goods that constitute these relations.

Although conceiving the self as a locus of interests does allow us to view the self from the perspective of conflict and consensus, it is important to note that this particular language of the self (like all other languages of the self) is limited by its problematic. For example, as my analysis will suggest, looking at the self as a locus of interests may highlight as a problem individuals who seek to satisfy nonconflicting social interests by pursuing conflicting goods (e.g., someone who seeks social recognition and acceptance by buying exclusive cars). But it cannot provide an account either of how such a personality comes into being or of how it might be transformed. It can only identify politically significant spheres of transformational possibilities. For these other tasks, complementary accounts of the self are necessary—accounts base on other kinds of problematics, such as those of welfare, therapy, communication, and morality. These problematics, in turn, require theories of needs, personality, discourse, and moral identity. Thus, in the differentiation of interests I shall offer, I make four general assumptions about the self, each of which would require a theory complementary to the account of the self as a locus of interests.

First, any particular articulation of *interest* is likely to coincide imperfectly with, or even conflict with, what an individual *needs* for a satisfactory life. This is only to say that since the self is not wholly fluid in its constitution, preferences can diverge from needs. Individuals have interests in the states of affairs that are amenable to their needs, and we usually want to insist on this no matter what their preferences are. Theories of needs are, however, notoriously difficult to develop beyond lists of basic requirements for livelihood and self-development (Braybrooke 1987). Fortunately, the self-transformation thesis requires only a very weak theory of needs, one that focuses on the general functional requirements of the self in relation to classes of goods that would satisfy them. This is because democratic theory is not a theory of welfare (although there are associated welfare requirements) but a theory of choice, public decision making, and self-governance. For this reason, the theory ought not to specify needs per se but, rather, (1) locate intrinsic possibilities for conflict and consensus according to interests that potentially express classes of needs and (2) locate conditions under which needs can be articulated and rearticulated (another way of talking about transformations of interests). A stronger account of needs is unnecessary and undesirable, since the point of expansive theories of democracy is to recommend institutional arrangements under which individuals develop control over their own need articulation (what I have referred to as *autonomy*).

Second, I assume that individuals require those mixes of satisfactions that maintain self-identity—a topic that requires psychodynamic, therapeutic, and cognitive accounts of the self; that is, interests are ordered as patterns of attachments that produce a

functioning personality (Warren 1990, n.d.). Accordingly, I assume that most possibilities for self-transformation emerge from those self-identities that attach nonconflicting needs (e.g., for membership or recognition) to conflicting goods ("membership" in the "family" of prestige car owners).

Third, as I have suggested, self-identity is in part a function of linguistic and symbolic identity since definitions of interest are both enabled and constrained by their media of publicity. A full account of identity-based interests requires a linguistic theory of meaning as it relates to self-constitution (see Habermas 1979, chap. 1; Harré 1987).

Finally, I assume that a description of the self in terms of interests can be redescribed in moral terms, in this way linking democratic and moral theory. This is because self-identity includes ideal representations of the self to the self, anchoring a person's conception of their interests in a moral representation of who they would like to be (Taylor 1989).

An account of the self in terms of interests, then, remains incomplete without complementary theories of needs, personality, language, and moral identity; yet the implications of these theories become clear for democratic theory only when appropriated within a theory of interests.

DIFFERENTIATING GOODS AND INTERESTS

With respect to the self-transformation thesis, the key point of a theory of interests is to distinguish between the self-defining interests whose transformation democratic theory ought to view as possible and desirable and those which cannot be transformed without undesirable results. I propose to do this by means of four dichotomies, which, while very general, will allow us to identify eight classes of goods, each of which present different kinds of transformational problems for their associated interests.

First, we can distinguish between goods that can be enjoyed by *individuals* or small groups of individuals, such as families and friends, and goods that are inherently *social* (e.g., a shared identity, whose value depends on larger-scale social interaction and recognition). What distinguishes an individual good is that it loses value the more widely it is shared even if possessing the good depends on complex social interdependencies. This is obviously the case with material goods (food, clothing, shelter). But it is also the case with nonmaterial goods that emerge from pairings of very small groups (e.g., love and friendship). While love between two people will draw on widely shared social goods for, say, rituals of interaction, it cannot be expanded to love of a city or nation or humankind without becoming a different kind of sentiment. Social goods, on the other hand, depend on broad sharing for their value. Citizenship or identification with a people or community has value only because it is widely shared. Likewise with language: in general, the more broadly shared its structures and conventions, the more value it has to individuals as a means of communication and expression. Of course, few, if any, activities are exclusively individual. For example, while the value of food in satisfying hunger is diminished as the same quantity of food is divided, the activity of eating almost always includes social goods that come into existence only through broad sharing, as is clear on such occasions as Thanksgiving and Seder.

The point of this distinction is that social goods can form bases for transformations away from conflict and toward commonality in ways that individual goods cannot. That a good is individual does not necessarily make it hostile to commonality; but neither should democratic theory seek transformational effects in the spheres defined by these interests. Attempts to transform individual goods in common directions will violate their inherent qualities, produce oppressive effects, and diminish individual autonomy and development. If, however, individuals seek individual goods for reasons that are primarily social (an important case), this will be a promising arena for transformational effects.

Second, we can distinguish goods according to whether they are characterized by *excludability* or *jointness of supply* (*nonexcludability*). Nonexcludable goods can be supplied to single individuals only if they are supplied to everyone (roads, parks, national security, aesthetically pleasing public spaces, clean air). These goods usually require collective action for their provision. The significance of this distinction is that although nonexcludable goods may serve individual interests, in most cases they require cooperation if they are to be provided. This suggests that democratic theory ought to look to arenas defined by nonexcludable goods for transformations of the self, since in pursuing these goods individuals may discover or create common interests. Excludable goods, although less promising, need not lead to conflict unless combined with scarcity. Love, friendship, and some kinds of group memberships are excludable but not inherently scarce, suggesting that they are neither promising arenas of self-transformation in the direction of commonality nor hostile to it.

Third, goods can be either *material* or *symbolic*. Material goods such as food, clothing, and shelter have physical presence and satisfy physical needs, although they rarely satisfy physical needs alone. Symbolic goods (including psychological goods) include recognition, self-identity, and symbolic resources (language, culture, life-style). Except where they combine with excludability, symbolic goods will more readily provide for the emergence of common interests because their value depends upon broad sharing.

Finally, goods can be *scarce* or *nonscarce*. Scarce goods are those for which demand is greater than supply in ways that engender potential conflicts between individuals regardless whether the source of the scarcity is physical (limited supplies of land) or social (a royal title) (see Hirsch 1976, chap. 2). Non-

TABLE 1

Generating a Typology of Goods That Present Different Kinds of Transformational Problems for Their Associated Interests

	INDIVIDUAL	SOCIAL
GOODS THAT ARE EXCLUDABLE AND MATERIAL:		
Scarce	Individual material goods	—
Nonscarce	Eclectic and plentiful material goods	—
GOODS THAT ARE NONEXCLUDABLE AND MATERIAL:		
Scarce	Public material goods	—
Nonscarce	Natural goods	—
GOODS THAT ARE EXCLUDABLE AND SYMBOLIC:		
Scarce	—	Positional and status goods
Nonscarce	Interpersonal identity goods	Exclusive group identity goods
GOODS THAT ARE NONEXCLUDABLE AND SYMBOLIC:		
Scarce	—	—
Nonscarce	—	Social identity goods

scarce goods are those for which there is a relatively unlimited supply (friendship, self-improvement, language).

There is, of course, a sense in which all goods are scarce because they are costly of time. Thus, pursuing friendships or becoming competent at the use of language often means that other kinds of goods cannot be pursued. This ontological scarcity has important ramifications for assessing trade-offs between goods of participatory democracy and other goods (Sirianni 1981). The distinction I am making here, however, has to do with the potential for conflict in the pursuit of goods. Pursuing friendship or literacy, for example, may be costly of time; but since their supply is limited neither by physical scarcity nor by social scarcity, we would not necessarily expect an increase of conflict were more people to pursue these goods. The significance of the scarce/nonscarce distinction is that shifts in self-defining interests from scarce to nonscarce goods will also reduce potentials for conflict.

Combining these distinctions produces 16 possible classes of goods (see Table 1). Of these classes 8 are empty of goods that are significant for democratic theory, generating a typology of 8 goods and their associated interests, each with different characteristics in respect to self-transformation.

1. Goods that are individual, material, scarce, and excludable I shall refer to as *individual material goods*. These include food, clothing, shelter, and other material consumables enjoyable by individuals or small groups such as families.
2. Goods that are individual, material, scarce, and nonexcludable I shall refer to as *public material goods*. These include public goods such as parks, libraries, roads, clean air, and territorial security.
3. Goods that are individual, material, nonscarce, and excludable I shall refer to as *eclectic material goods and plentiful material goods*. Eclectic goods are those that so few individuals enjoy relative to supply that there is no scarcity (dandelions). Plentiful goods are those in such great supply that there is no scarcity (fruit on a tropical island of natural abundance).
4. Goods that are individual, material, nonscarce, and nonexcludable I shall refer to as *natural goods*. These include attributes of our natural environment (oxygen, sunlight) that are normally in abundance.
5. Goods that are individual, symbolic, nonscarce, and excludable I shall refer to as *interpersonal identity goods*. These include the goods of personal identity that emerge out of close interpersonal relations such as love and friendship. (In principle these goods are not scarce, since every new relationship brings new goods into being without this necessarily leading to conflict; but they are excludable, since individuals partake of different interpersonal goods whose origins are intimate and not societywide.)
6. Goods that are social, symbolic, scarce, and excludable I shall refer to as *positional and status goods*. Positional goods are material goods (houses, cars, clothing) whose value is primarily symbolic and based on a broadly recognized identification with class or status. Status goods are symbolic goods

TABLE 2

A Typology of Goods That Present Different Kinds of Transformational Problems for Their Associated Interests

GOODS	QUESTIONS RELATING TO THE TRANSFORMABILITY OF INTERESTS		
	IS THE GOOD NONSCARCE AND THUS INHERENTLY NONCONFLICTING?	DOES THE VALUE OF THE GOOD DEPEND ON SOCIAL INTERACTION AND RECOGNITION?	DOES ACHIEVING THE GOOD REQUIRE COMMON DELIBERATION AND ACTION?
Individual material	No	No	No
Eclectic and plentiful	Yes	No	No
Natural	Yes	No	No
Interpersonal identity	Yes	No	No
Positional and status	No	Yes	No
Exclusive group identity	Yes	Yes	No
Public material	No	No	Yes
Social identity	Yes	Yes	Yes

(degrees, titles) that have value because they are broadly recognized but also excludable and scarce (see Hirsch 1976).

7. Goods that are social, symbolic, nonscarce, and excludable I shall refer to as *exclusive group identity goods.* These include goods of self-identity that depend on group distinctions (based on religion, language, and ethnicity or on special interests, skills, professional and workplace associations, fads, and life-styles) but do not depend on scarcity, since there is no inherent shortage of group identities (as there must be with, say, status identities).
8. Goods that are social, symbolic, nonscarce and nonexcludable I shall call *social identity goods.* These include communal resources (language, national culture), as well as identities and recognitions that attach to the goals, ideals, activities, and dialogue that constitute a society.

We can now arrange these goods and associated interests according to questions relevant to self-transformation. In Table 2, I ask three questions of each good: (1) Is the good nonscarce and thus inherently nonconflicting? (2) Does the value of the good depend on broad social interaction and recognition? (3) Does achieving the good require common deliberation and action? The goods are loosely ordered according to their compatibility with self-transformation. The three *nos* of individual material goods indicates that this is not a promising arena for self-transformation. The three *yeses* of social identity goods indicates that a democracy might encourage transformations of the self based on these goods without threatening autonomy. The mixed cases require qualification.

Note that the standard model tends to assimilate the characteristics of all goods to individual material goods, in this way failing to recognize the interests upon which expansions of democracy might build. On the other hand, expansive democrats tend to assimilate the characteristics of all goods to those of social goods, overlooking arenas in which demands for self-transformations are undesirable or misplaced.

Beginning at the top of the table, individual material goods have few inherent properties of commonality. They need not be supplied collectively, and they are potentially scarce. The demand for self-transformation in the direction of commonality here can only produce a collective denial of these interests. Individuals will find political demands for self-transformation in this arena to be oppressive, and they will experience commonality as self-sacrifice. What is called for, instead, is a common recognition of allocative problems, a recognition resulting in distributional mechanisms that operate according to generally agreed rules and principles.

These goods are, of course, the home domain of standard liberal democracy. Yet even here, the standard approach is inadequate. First, because it generalizes the allocative model of politics based on an assimilation of all interests to individual material interests, the standard approach is unable to appreciate situations in which a community might choose to serve certain common, social interests (e.g., when individuals view the poverty of others as inconsistent with their view of a good society) by agreeing on an appropriate method of allocation (e.g., modifying the workings of markets through the tax structure). These sentiments can develop, however, only because selves are partly defined by common, social interests (see Sunstein 1991).

Second, the standard approach is unable to identify situations in which individuals attempt to satisfy nonconflicting interests through conflictual goods, situations for which the self-transformation thesis is entirely appropriate. When two working parents compensate for the attention they deny their children by bringing home gifts, they introduce a relative scarcity of interpersonal goods and displace these onto individual material goods. Their children may later seek love and recognition through individual material goods, a confusion that reduces their auton-

omy and satisfaction. The confusion also increases potentials for conflict, not only because of the scarce nature of the goods they seek but also because their lack of satisfaction is likely to produce resentments that fuel the politics of scapegoating. We could even define a consumer culture as a set of social structures, institutions, and interpretations that displaces desires for nonconflictual, nonscarce goods onto individual, scarce, material goods (see Leiss 1976). Thus, even though individual material interests are not appropriate for self-transformation, increasing democratic dialogue and social interaction would provide opportunities to distinguish between interests that are irreducibly individual and material and interests that are inappropriately displaced into this arena. These are the cases in which expanded democracy might transform individual material interests in common directions while developing selves that are increasingly autonomous and have increasingly diverse possibilities for satisfaction.

Eclectic and plentiful goods and natural goods are similar in that they neither are inherently conflictual nor depend on commonalities for their existence or satisfaction. All other things being equal, they ought to fall outside of institutionalized politics, democratic politics included—which is to say that they ought to be politically protected but not politically allocated or transformed. Eclectic and plentiful goods—excludable goods of interest to only a few individuals or in such great supply that there is no competition for them—present no problems for politics and ought to be left alone, owing to their contributions to pluralism. Natural goods such as sunlight and breathable air present political problems only in the sense that they are necessary for all individuals yet can be spoiled by a few individuals because they are nonexcludable. When damaged, they in effect become scarce and present the problems of public goods.

In standard liberal democracy interpersonal identity goods are recognized under the guise of private protections (Warren 1989). Because the standard view assimilates these goods to possessive assumptions, it also assumes they are inherently conflictual. But there is no reason for them to be conflictual in principle because they are nonscarce: every social relation generates new identity goods—goods that did not exist before and that are emergent properties of the relationship. The exclusiveness of love and friendship often leads to conflict, as will a lack of reciprocity in recognitions. These conflicts are not, however, "political" in the sense that they cannot be addressed directly by collective actions: they do not present problems of allocation; nor can interpersonal recognitions be created politically, that is, outside of intimate relations.

This is not to say that these goods are not affected by contingencies of a political nature or that they might not be subsumed inappropriately into politics. It is here that questions about self-transformation become important. To return to a previous example, when economic circumstances force parents to work full-time, they may not be able to devote the time to their children or to each other that their relationships require. In such cases, interpersonal recognitions become scarce, and a collective failure to deal with distributive justice is reflected in a sphere of intimacy. Unsatisfied interpersonal interests may then be displaced into arenas with intrinsically conflictual properties (e.g., material, positional, or status goods) or even into politics itself. In the latter case, membership in a nation, ethnic group, or movement may vicariously substitute for scarce interpersonal relations. Such displacements are likely to lead to frustrations, since the good is inappropriate to the interest. If so, this would indicate reduced autonomy for individuals, as well as contribute to the conflictual politics of scapegoating, racism, and resentment.

In the arena of interpersonal identity goods, then, self-transformations can and ought to occur not by reorienting interpersonal identities toward democracy or seeking therapeutic effects from politics but by reinforcing the conditions (e.g., time and protected intimate space) that allow interpersonal identities to develop. In these cases, transformations of the self occur not through democratic interaction but within protected and enabled spheres of intimacy. Although these transformations are not achieved by directly political means, they are nonetheless of key importance to expanded democracy since we might expect them to reduce conflict by reducing the displacement of frustrated interpersonal interests onto conflictual goods.

Positional and status goods are the only social, symbolic goods that standard liberal democracy recognizes—perhaps because they are inherently conflictual, consistent with the assumptions of the standard view. Positional and status goods are both social and scarce because what makes them worth having is their widely acknowledged scarcity. Mercedes Benzes and medical degrees both have intrinsic merits; but when people pursue them because they are widely valued and relatively uncommon, they become positional and status goods and take on additional conflictual dimensions. Unlike individual material goods, positional and status goods are appropriate arenas of self-transformation because they are often avenues for pursuing goods that are not inherently conflictual. In a society (such as ours) where most social organizations are neither democratic nor egalitarian, a broad range of nonconflictual interests in differentiation, recognition, and personal identity has a narrow, often conflictual range of institutional means of expression and development. In these arenas of interest, expanding democracy might very well produce desirable transformations of the self.

In the arenas of interest constituted by exclusive group identity goods are found the most bitter, uncompromising, and explosive conflicts. Whereas individuals will usually negotiate and compromise individual material goods, they are often willing to die for their group identity. The irony is that exclusive group identity goods are not inherently conflictual because they are not scarce, in spite of the fact that not everyone in a society will identify with the

same groups. As with interpersonal identity goods, every new group identity produces new goods without necessarily diminishing the goods available to others (as with individual material goods) or depending upon invidious comparisons (as with positional and status goods). These group interests are appropriate for self-transformation—not to produce common identities out of plural ones but to transform different identities in ways that produce diversity and pluralism rather than stratification and conflict.

As in the case of positional and status goods, expansive democrats should explore the possibility that conflicting group identities are produced by social structures and personal biographies that link group identities to inherently conflicting goods. In the case of racism, for example, differences in culture, life-style, and physical appearance determine differences in access to jobs, housing, and education, linking nonconflictual interests to conflictual ones (in more traditional terms, *race* to *class*). This example suggests, however, that expanding democracy will positively transform the self only where a society also chooses to address problems of distributive justice at the same time: toleration and appreciation of differences are likely to develop only when there are no systematic, differential sacrifices of individual material interests between groups. Otherwise, dialogue will heighten conflict by exposing injustices—a process necessary to future consensus but one that is likely to increase conflict as long as injustices remain in arenas of conflictual goods. The transformational impact of expanded democracy in such cases will depend on how conflictual arenas of interest are arranged.

Other kinds of group conflicts may be less transformable through politics. Sometimes groups are formed through their identification with irreconcilable principles. The abortion issue, for example, may be so tied in with differences between secular and religious worldviews that any self-transformation would have to reach deeply into the organization of the self. If these kinds of differences can be mitigated by dialogue (and this is by no means clear), it would be of a kind unlikely to take place in a political context (Warren, n.d.). But even if some kinds of group conflicts are not likely to be transformed by expanding democracy, it is important to note the costs of adversarial institutions with limited democracy: they encourage political entrepreneurs to create regional, racial, ethnic, religious, and nationalist constituencies, producing conflicts where there may have been only differences (Burnheim 1985; Mansbridge 1980).

Public material goods present unique opportunities for self-transformation when compared with other goods. Although they are inherently conflictual and do not depend on commonality for their value, they can only be gained through common action. Combined with expanded democracy, this characteristic presents individuals with opportunities to change the way they make trade-offs between individual and public material goods, as well as to discover other, nonconflictual goods associated with the common deliberation and action that public goods require.

I have already suggested that social identity goods lend support to the self-transformation thesis. Here I shall add comment on one problematic feature of social goods. They can often be ideologically manipulated by individuals, groups, classes, or organizations who seek to portray their partial, conflicting interests as common ones. Ideological appeals of this sort can be successful just because social goods have a powerful impact upon individual identity (Warren 1989, 1990). Indeed, individuals often find that they must ignore some kinds of conflicts in order to gain social goods like membership, pride of belonging, and recognition. In terms of the distinctions I have developed here, one way of identifying situations in which social goods are used for these purposes would be to look at how appeals to social goods relate to other arenas of interest. In terms of the goods listed in Table 2, the more the goods toward the top of the table are the subject of appeals to commonality, the more one ought to suspect an ideological equation between partial and common interests. If an arrangement and its justification require differential sacrifices for the sake of a social good (e.g., military risk by the economically disadvantaged for "the pride of the nation"), then one should suspect that a social interest is being manipulated ideologically to produce a sentiment of commonality when most other interests are conflicting. This is where communitarians are often culpable, and this is why expansive democrats insist that self-transformation in the direction of commonality must occur through democratic dialogue—the context in which ideologically manipulated commonality is least likely to survive.

CONCLUSION

I have offered a sketch of how theorists of expansive democracy might take on the burden of argument with respect to the self-transformation thesis. The sketch is not yet a democratic theory: it lacks accounts of power, justice, allocation, judgment, and other features of institutional design. But it does suggest new ways of thinking about self-transformation, thus anticipating some of the more important objections to expanding democracy by standard liberal democrats. Qualifying the self-transformation thesis by differentiating goods and interests suggests that some interest arenas have potentials for conflict that must be dealt with through institutions designed for conflict even when there are common interests in resolving conflict. The qualifications also suggest that most differences are inherently neither conflictual nor hostile to commonality. Commonality can coexist with pluralism, however, only where institutions are appropriate to the kinds of interest they mediate and express. Finally, autonomy and self-development are increased by institutions that take into account the characteristics of each kind of interest as it relates to the self. Only by differentiating the social relations

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implied by these interests can we grasp the full potentials of democratic processes and commitments, potentials that are ultimately rooted in how democracy protects and facilitates persons.

Notes

1. My conclusion borrows from Jane Mansbridge's *Adversary Democracy* (1980) in calling for different institutional mechanisms for conflicting and consensual interests but relies on a different argument. Mansbridge argues that standard liberal democracy overlooks consensual interests, but she treats both consensual and conflicting interests as given parameters of institutional processes. In contrast, I focus on processes and constraints that constitute differing configurations of interests as conflicting or consensual. For a critique of Mansbridge on this point, see Pitkin and Shumer 1982. My approach is also loosely indebted to Michael Walzer's *Spheres of Justice* (1983). Walzer distinguishes spheres of value in terms of the goods intrinsic to the practices that define these spheres. This allows him to define domination in terms of the goods of one sphere converting into the goods of another (e.g., the buying of loyalty). Walzer assumes without developing, however, an account of the self that would show how the practices and goods of each sphere enable or disenable politically relevant capacities, such as autonomy.

2. These other objections have to do in part with problematic relations between democracy, expertise, and bureaucracy. See, e.g., Sartori 1987, 113–20. For suggestions, see Gould 1988, chap. 10 and Pollitt 1986. They also have to do with relations between democracy and markets; see Miller 1989.

3. See also Habermas 1984–87.

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POWERFUL PACIFISTS: DEMOCRATIC STATES AND WAR

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Democracies are less likely to fight wars with each other. They are also more likely to prevail in wars with autocratic states. I offer an explanation of this syndrome of powerful pacifism drawn from the microeconomic theory of the state. State rent seeking creates an imperialist bias in a country's foreign policy. This bias is smallest in democracies, where the costs to society of controlling the state are relatively low, and greatest in autocracies, where the costs are higher. As a result of this bias, autocracies will be more expansionist and, in turn, war-prone. In their relations with each other, where the absence of this imperialist bias is manifest, the relative pacifism of democracies appears. In addition, democracies, constrained by their societies from earning rents, will devote greater absolute resources to security, enjoy greater societal support for their policies, and tend to form overwhelming countercoalitions against expansionist autocracies. It follows that democracies will be more likely to win wars.

No less likely to fight wars in general, democracies are significantly less likely to fight each other. The relative pacifism of democracies remains a puzzle. Although it has attracted significant attention, most of the recent work has been empirical (Maoz and Abdolali 1989; Small and Singer 1976) or philosophical (Doyle 1983a). No *theory* presently exists that can account for this striking empirical regularity.

Equally important, but far less widely recognized, is the propensity of democracies to win the wars that they do fight. In view of the drawbacks commonly associated with the democratic conduct of foreign policy,[1] this finding poses even more of a conundrum. If democratic decisionmaking is often slow, inept, naive, and prone to stalemate, how and why do democracies typically triumph over their faster, more professional, sophisticated, and decisive autocratic brethren? In the ultimate contest of national strength and will, why are democracies more powerful?

I offer one possible explanation for this syndrome of powerful pacifism drawn from a larger theory of grand strategy (see Lake 1991). I argue that autocratic states, which typically earn rents at the expense of their societies, will possess an imperialist bias and tend to be more expansionist and, in turn, more war-prone. To the extent that democratic states tend to be more constrained by their societies from earning rents, wage their own wars of expansion under more restricted conditions, possess greater incentives to intervene in the domestic political affairs of autocracies, and become objects of autocratic expansion, there should be no significant overall difference in frequency of war involvement. In the absence of this imperialist bias, however, democracies should be relatively pacific in their relations with each other.

Moreover, democratic states, because they are constrained from earning rents, will tend to create fewer economic distortions, possess greater national wealth, and devote greater absolute resources to national security. They will also tend to enjoy greater societal support for their policies. To the extent that states balance threats rather than power, democracies will also tend to form overwhelming countercoalitions against expansionist autocracies. Together, these three propositions imply that democratic states should be more likely to win wars.

A THEORY OF THE RENT-SEEKING STATE AND FOREIGN POLICY

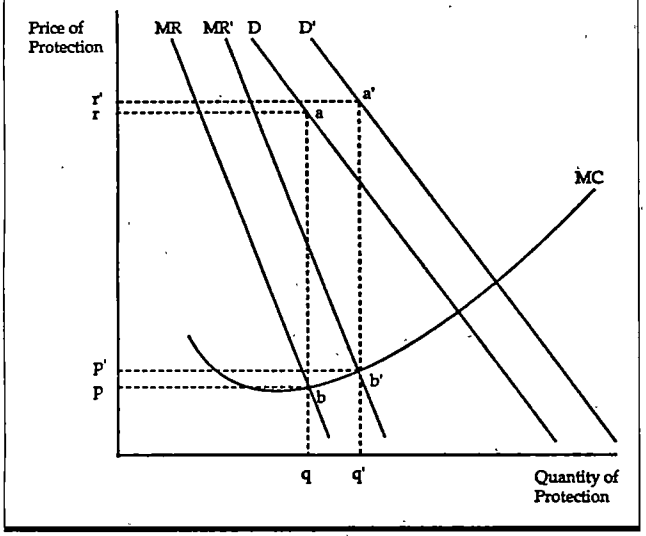
Stimulated by the pioneering work of Lane (1979), the microeconomic theory of the state conceives of the state as a profit-maximizing firm that trades services for revenues.[2] I define *profit* here as the difference between revenues acquired by the state and its real costs of producing services and collecting revenue. Real costs are determined, in turn, by the fair market value of the resources consumed by the state in the production and collection processes.[3] Thus, following normal usage, profit includes both what is commonly called *normal or economic profit*, defined by the opportunity cost of the factors of production or their return in their next-best occupation, and *supernormal profits or rents*, returns greater than what is necessary to sustain the factors of production in their present use. (See Tullock 1980.)

Foremost among the services provided by the state is protection. For purposes of the following analysis, protection is further restricted to mean only defense from external threats. While I focus on this single "industry," the argument can easily be generalized to other state services; and rents earned by the state on any service in any area under its control are sufficient to create an imperialist bias in its grand strategy.

Many competing definitions of the state exist. In this theory, the state, a service-producing firm, is defined by two functional attributes. Since the protection industry typically enjoys economies of scale over an extensive geographic area, the state forms a natural but local monopoly (Lane 1979, 23). As a result, one and only one state will exist in any area at

Figure 1

The Supply and Demand for Protection under Monopoly Provision



any given time. Similarly, as protection from foreign threats forms a local public good, whose suboptimal provision is otherwise ensured by the large number of citizens involved, the state will supply this service only if it is granted, or is able to obtain, some coercive ability over its society. These two aspects of the protection service, local monopoly and coercive supply, are consonant with Weber's classic definition of the state as a "compulsory organization with a territorial basis" that monopolizes the legitimate use of force (1978, 1, 58).

The Demand and Supply of Protection

All individuals possess a positive demand for protection; and as price declines, they increase the quantity they demand. Accordingly, the demand curve for protection slopes downward and to the right, as in Figure 1.[4] The precise slope is a function of societal preferences (which are considered to be exogenous) and the availability of substitutes (which, given the nature of protection, tend to be few), suggesting that demand is relatively inelastic and the curve correspondingly steep. Although theoretically possible, it is unlikely that the demand for protection is perfectly inelastic (i.e., that the demand curve is perpendicular to the horizontal axis), since this implies that society is willing to pay virtually any price for even small amounts of the service. To the extent that society places any value on goods and services other than protection, or, if there is any trade-off between guns and butter, the demand curve must possess at least a slight negative slope. It is equally unlikely that the demand for protection can be saturated. Historical experience suggests that even high levels of defense spending do not create feelings of total security. To the extent that the security dilemma holds at all (Jervis 1978), the demand for protection can be sated not at some high level of military spending but, rather, only in a world where everyone else is completely disarmed.[5] In short, under most feasible conditions, the demand curve for protection is sensitive to price.

The level of protection demanded by society, in turn, is primarily a function of the level of external threat. The greater the external threat, ceteris paribus, the higher the demand for protection will be. This is depicted in Figure 1 as a shift outward in the demand curve ($D' > D$).

As in any monopoly, the state enjoys some measure of market power and can, within the limits set by the demand schedule, control the quantity of the good (protection) supplied; and as with any profit-maximizing firm, the state will set its output at the level that equates marginal cost with marginal revenue and charge what the market will bear.[6] In Figure 1, price p represents the normal profit level; but since in all monopolies the profit-maximizing level of production lies below the demand curve, the final price charged to consumers will be higher and may be as high as r. The difference between p and the price charged, say r, defines the rent or supernormal profit earned by the state—represented graphically by the rectangle p-r-a-b.[7]

Unlike other monopolists, however, states can also act opportunistically against their own societies by artificially increasing the demand for their services through extortion or racketeering. Extortion occurs when states magnify, exaggerate, or "oversell" foreign threats to society, whether by supplying incomplete information or engaging in outright deception (see Ames and Rapp 1977; Lowi 1967). States conduct protection rackets by actively creating foreign threats, from which they then protect society (see Tilly 1985). In both cases, a state effectively shifts the demand curve outward ($D' > D$) and thereby earns greater rents (p'-r'-a'-b' > p-r-a-b).

Two important points follow from this analysis. First, the level of protection supplied by the monopoly state will always be less than that produced under conditions more closely approximating "pure competition."[8] While society would benefit from higher levels of protection, given prevailing costs of production, the profit-maximizing or optimal strategy for the state is to restrict supply—whether it can successfully capture the potential rents or not. Insecurity is an inherent feature of life under the modern state.

Second, the state faces strong incentives to seek rents at the expense of society. In other words, the state can benefit itself by charging consumers the monopoly price for protection (r in Figure 1) and by artificially inflating demand through extortion or racketeering.

Societal Constraints on State Rent Seeking

Consumers clearly prefer to purchase protection at the lowest sustainable price (p in Figure 1). The state, on the other hand, clearly prefers to sell protection at the highest possible price, which is determined by the slope of the demand curve and represented by r.

The actual price—and the level of rents extracted from society—is determined by how well individual citizens can control or regulate the rent-seeking behavior of the state. Society's ability to control the state, in turn, is influenced by the costs of three separate activities: monitoring state behavior, voice, and exit (see Hirschman 1970).

In order to control the state, individuals must first monitor its performance and acquire information on the strategies it is pursuing, its real costs of protection production, the level of foreign threat, and the like. Monitoring the state is analogous to principal–agent problems in publicly held corporations, where the stockholders (the principals) seek to ensure that the managers (the agents) work hard and faithfully in their interests.[9] The "problem" arises because no single stockholder typically has any incentive to invest in, or acquire information on, the manager's true performance—information, of course, it is assumed that the managers themselves possess. Collective action problems also stymie any group investment in information. The higher the costs of acquiring information, the less control the stockholders can exert over the firm and the more the managers can shirk or adopt policies that benefit themselves at the expense of their principals. The same is true of the state–society problem: no single citizen has any incentive to invest in information; and, because of the free-rider problem, collective investment in information occurs only at suboptimal levels.[10] The higher the costs of acquiring information regarding state performance, the greater latitude state officials possess to engage in rent-seeking behavior.

Once performance and the level of state rents have been assessed, individuals have two instruments through which to alter or change state behavior: exit and voice. Through exit, individuals move and deplete both the resource base of the state, raising its real costs, and its market for protection, lowering the price it can charge. Discipline is imposed upon the state, in other words, by reducing its profits, thereby punishing it for undesirable behavior.

As Hirschman notes, exit is the quintessential economic strategy: if a consumer ceases to like a product, he or she simply stops purchasing it (1970, 15). Consumers of state-provided services do not have quite the same freedom. Because protection is a public good supplied by a local monopoly, individuals cannot choose to consume varying amounts or qualities; indeed, because it is coercively supplied, individuals cannot choose whether or not to consume any protection at all. Nonetheless, political exit can occur, although it takes different forms and usually entails a higher cost than exit from private goods consumption. Individuals can choose to migrate or "vote with their feet" (or assets), moving from high-rent to low-rent areas.[11] Freedom of emigration is often one of the first rights obtained in the process of democratization (as was recently witnessed in Eastern Europe), suggesting that the right to exit and democracy will tend to be conjoined, with the former serving as the ultimate guarantor against failures of the latter. Similarly, territories can secede or threaten to secede. If large enough, they can try to form an independent state or, failing that, they can try to merge with other secessionist or democratic territories into a larger union. While possible, exit is costly to the individuals or territories that choose it. The higher the cost of exit, the greater the ability of the state to earn rents.

Voice, or political participation, disciplines the state by separating or threatening to separate state officials from their offices: the citizens stay, but the composition of the state changes. Voice can take many forms, from voting, to campaign contributions, to mass unrest, to active rebellion.

At the individual level, the costs of political participation are unevenly distributed across society. This occurs for a host of idiosyncratic reasons. For instance, not all countries have universal suffrage, the opportunity cost of a campaign contribution is significantly less for a multimillionaire than for a welfare family, and the military can overthrow a leader with greater ease than can unarmed civilians.

At the aggregate or national level, the costs of political participation vary by regime type. For instance, in most democracies, where elections are the primary focus of political participation for the majority of citizens, it is relatively costless to vote and exercise voice.[12] At the other extreme, autocratic states typically suppress political dissent; and voting, if it occurs at all, is ineffective in removing officials from power. In these countries, to replace, or effectively threaten to replace, a ruler requires either mass unrest or some form of armed rebellion—activities that carry considerably higher individual costs, including the possibility of death. In these polities, voice is very costly to the average citizen and, as a result, is seldom exercised. It follows that the higher the costs of political participation, on average, the greater the state's ability to earn rents will be.[13] The relatively low cost of political participation in democracies constrains the state's rent-seeking ability, whereas the relatively high cost of political participation in autocracies frees the state to earn rents.

In determining its level of feasible rents, the state will act as a discriminating monopolist, charging citizens in general and each separate citizen in particular a price for protection positively related to the costs of monitoring, exit, and political participation. Indeed, the state will charge up to—but in equilibrium not more than—the price at which individuals would be tempted either to exit or to engage in the lowest-cost form of political participation available to them that would effectively remove current state officials from power.

Expansion and the Rent-seeking State

In practice, there are always positive costs of monitoring state behavior and exercising voice and exit. As a result, all states possess some ability to earn rents, although the ability will be larger in autocracies than in democracies for the reasons just surveyed. To the

extent that a state can earn rents, state and societal interests will diverge and the state will be biased toward an expansionary foreign policy.[14] This relationship is continuous. The higher the costs to society of controlling the state, the greater will be the rent-seeking ability of the state, the more the interests of state and society will diverge, and the more expansionist the state will become.[15] This imperialist bias arises for three reasons.

First, expansion may increase the state's rent-seeking ability by reducing the benefits of exit. The net benefit of exit to any individual (and thus that individual's incentive to engage in this action) is determined by both the *push* of high rents at home and the *pull* of lower rents abroad. When all states extract equally high rents, there is no incentive to move. If through expansion a state can eliminate or engulf a low-rent competitor, it increases its own ability to earn rents. This suggests that low-rent states will often be objects of expansion for rent-seeking autocracies.

Second, a state may also expand so as to provoke others into threatening its own society. Both extortion and racketeering rest upon persuading citizens that foreign threats are larger and more real than they are or otherwise would be. If successful, the state convinces consumers to increase their demand for protection and, in turn, earns more rents. Even with incomplete and costly information, however, citizens may eventually discern the true level of threat and lower their demand. Through expansion short of universal empire, the state lends credibility to extortion and supports racketeering, thereby strengthening its ability to earn rents at the expense of society.

Third, and most important, the larger the state's rent-seeking ability, the higher the total revenue earned by the state. The more revenue (ceteris paribus), the larger the optimal size of the political unit. These relationships are depicted in Figure 2.

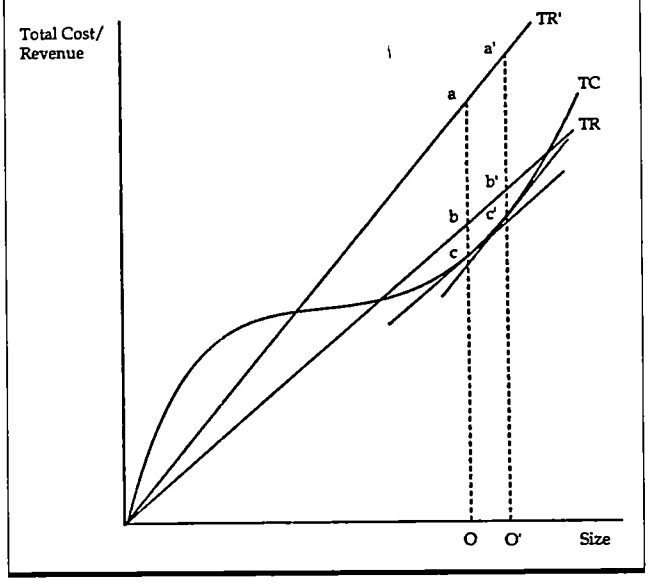
For all states, an optimal size exists defined by the costs of collecting revenue and producing protection and the revenues earned by providing this service to society. Each additional unit of territory acquired by the state produces additional revenue: the state becomes the new local monopoly supplier of protection, and it taxes consumers in that region accordingly.

On the other hand, the costs of governing rise with the size of the political unit, placing an effective cap on the size of nation–states. These resource costs occur primarily in the form of transactions costs of revenue collection (see Levi 1988; North 1981). Over some limited range, the state may enjoy increasing returns to scale in revenue collection; but soon, the addition of more territory begins to strain the administrative abilities of the state, leading to diminishing returns.

When combined with the costs of producing protection, the state's total cost curve typically resembles that in Figure 2. Economies of scale in protection, in conjunction with initial increasing returns in revenue collection, suggest that the slope of the total cost-curve declines for some substantial distance, flattens

Figure 2

The Optimal Size of Political Units



as the marginal costs of revenue collection begin to rise more rapidly, and eventually increases as the costs of revenue collection accelerate and the economies of scale in protection are exhausted.

If revenues increase monotonically, there is a single optimal size of the political unit where marginal revenue equals the marginal costs of collection and production. Geometrically, this occurs where a line tangent to the cost curve is equal to the slope of the revenue line, as at size *O*. At *O*, the economic profit to the state is measured by the line segment bc. No profit-maximizing state has any incentive to expand beyond the point where marginal cost is equated with marginal revenue.

Rents earned by the state, however, cause the total revenue line to rotate counterclockwise from the origin (TR′ > TR). Although the total cost curve may also increase as state rent seeking stimulates higher transactions costs of revenue collection, the curve must rise at a lagging rate. Higher state rents do not increase the costs of producing protection from external threats per se, and this is likely to be the major component of the cost curve. And even if the transactions costs of revenue collection increase, because important social groups demand compensation or public unrest must be suppressed, collective action problems thwart a fully countervailing societal response. Assuming for expositional clarity that total costs remain constant, state rent seeking raises total revenue and expands the optimal size of the political unit from *O* to *O*′. Intuitively, with rents, each unit of territory produces greater revenue for the state. The greater the revenue, the greater the equilibrium costs that can be borne to capture that revenue. Thus, a state with an increased rent-seeking capacity has an incentive to expand until marginal revenue and mar-

ginal cost are once again equalized at a new, larger size.

While this expansion (from O to O') benefits the state, it harms society. With TR′ representing the revenue of a successful rent-seeking state, the line segment b′c′ will be the state's economic profit and a′b′ its rent, with a′b′ being redistributed away from society to state officials. Not only is society exploited by state rent seeking, but it is doubly hurt by the additional expansion rent seeking induces. In the absence of expansion, the state's rents would be ab; with expansion, these rents increase to a′b′. Simple observation suffices to show that a′b′ will always be longer than ab.

Both the original citizens or consumers of state-produced services and the individuals newly incorporated into the territorial unit pay the new higher, expansion-induced price for protection. In other words, greater rents are extracted from both the original and augmented populations, although the exact rate at which these groups will be taxed is determined by their relative costs of controlling the state. The previously foreign population does not bear the burden of the higher rents alone; all consumers in that particular state face a higher price for protection. Imperialism is not simply a means for extracting wealth from "foreign" territories: it is a tool used by the state for exploiting its own society as well.

While this argument has been developed only in terms of a single state service (protection), any rents earned on any service provided by the state increase the optimal size of the political unit. Even if rents are earned from providing, say, the physical infrastructure, the net benefits to the state of providing that good still increase and provide an incentive for further expansion. Fully generalized to all areas of state service, the state optimizes where the marginal revenue (including rents) equals the marginal costs of providing all services and collecting all revenues. Given that the demand for protection is likely to be more inelastic than the demand for other state services, however, the highest rents are likely to be earned in this industry. Nor is there any apparent reason why societal constraints on the state should differ significantly across areas of service.[16] Hence, it is appropriate to focus on the production of protection. Nonetheless, it is important to note that in a fully generalized theoretical framework, any rents earned by the state are sufficient to generate an imperialist bias.

It is virtually impossible to measure state rents directly. Perhaps in past centuries, when the public fisc and the private purse of the ruler were one and the same, it might have been possible to observe the rents earned by the state by measuring the comparative opulence of the royal court. If the general argument developed here is correct, however, then surely one of the primary tasks of modern state budgeting techniques is to obscure the difference between normal and supernormal state profits. It also follows that in the countries where they are most easily observed, rents will be relatively low—even zero, at the extreme.[17] Given these measurement difficulties, no direct test of the theory is possible. Rather, it can be assessed—and the presence of state rents revealed—only by examining the theory's behavioral implications. Accordingly, I shall examine several hypotheses derived from the theory. In doing so, I also provide an explanation for one of the longest-standing puzzles of international relations.

THE PROPENSITY FOR WAR

In a recent review of the literature, Levy writes that (1) "the evidence shows that the proportional frequency of war involvement of democratic states has not been greater than that for nondemocratic states"; (2) "democracies may be slightly less likely than nondemocratic states to initiate wars, but the evidence is not yet conclusive on this question"; and (3) "although democracies have fought wars as frequently as have nondemocratic states, they almost never fight each other. . . . *This absence of war between democratic states comes as close as anything we have to an empirical law in international relations*" (1989, 270; my emphasis). In Russett's view, this final result "is one of the strongest nontrivial or nontautological generalizations that can be made about international relations" (1990, 123).[18]

For illustration, all interstate wars from 1816 to 1988 involving democratic states are listed in the Appendix.[19] Of these 30 conflicts, only 2 involve democratic states fighting each other: World War II, where Finland fought alongside the Axis (an easily explained exception), and the Spanish–American War of 1898.

This pattern of pacifism only among democratic states is inconsistent with most prevailing theories of international politics (Doyle 1983a, 218–25; Levy 1989, 270). Realism, which focuses on the *universal* effects of international anarchy, the security dilemma, and the balance of power, cannot account for the relative pacifism of democracies. This is even more true of its contemporary variant, neorealism, which explicitly abstracts from the domestic characteristics of nation–states. Given the correlation between democratic political structures and capitalism, Marxist–Leninist theories predict a higher incidence of war between democracies. Conversely, liberal economic theory, which highlights the pacifying effects of commerce, predicts a lower *overall* incidence of democratic war involvement, not just a lower probability of war among democracies.

The most persuasive account of the relative pacifism of democracies was first put forth by Immanuel Kant in 1795, when there were less than a handful of democracies in existence, and has been recently summarized and extended by Doyle (1983a, 1983b, 1986; see also Russett 1990, 124–32). There are three steps in Kant's argument. He first posits that republican forms of government, the inevitable result of political evolution, will replace monarchical (or autocratic)

caprice with populist caution. Kant "argues that once the aggressive interests of absolutist monarchies are tamed and once the habit of respect for individual rights is engrained by republican government, wars would appear as the disaster to the people's welfare than he and other liberals thought them to be" (Doyle 1983a, 229).

While republican rule guarantees caution, international law guarantees mutual respect among liberal states—a step that separates Kant from other liberal thinkers:

> As republics emerge . . . and as culture progresses, an understanding of the legitimate rights of all citizens and of all republics comes into play; and this, now that caution characterizes policy, sets up the *moral foundations for the liberal peace*. . . . In short, domestically just republics, which rest on consent, presume foreign republics to be also consensual, just, and therefore deserving of accommodation" (Doyle 1983a, 230; my emphasis).[20]

Finally, drawing upon liberal economic theory, Kant concludes that cosmopolitan law, especially the "spirit of commerce," provides a material incentive for states to promote peace and avert war—a third step that reinforces the basis for mutual respect.

Doyle (1983b) extends Kant by identifying and examining the moral imperative that liberalism creates for democracies in their relations with nondemocratic states.[21] "If the legitimacy of state action rests on the fact that it respects and effectively represents morally autonomous individuals," he argues, "then states that coerce their citizens or foreign residents lack moral legitimacy" (p. 325). It follows, then, that "the liberal dictum in favor of nonintervention does not hold. Respecting a nonliberal state's state rights to noninterference requires ignoring the violations of rights they inflict on their own populations" (p. 330). Thus, the moral foundations of the liberal peace are absent in relations with autocratic states, allowing war to be used as an instrument of statecraft and, at the extreme, necessitating active intervention by liberal states in the internal affairs of autocracies.

Kant's theory and Doyle's extension constitute a normative philosophy for the conduct of foreign affairs but not a positive theory of international relations. This philosophy rests, fundamentally, upon a moral imperative of *restraint*, in which democratic states must forgo potentially welfare-improving actions that would be damaging to their liberal brethren, and *necessary action*, in which a democracy might actually reduce its own material welfare in order to overthrow an autocracy and free a repressed people.[22] While it forms a sophisticated and coherent worldview, liberalism nonetheless contains a curious combination of motivations—a combination that can be reconciled only if we assume that individuals are in fact essentially moral actors willing to forgo material gains for normative ends. But if we accept this assumption, then the exploitative behavior of autocratic rulers is theoretically unmotivated: if individuals are moral and will act morally given the opportunity, why should autocrats, who are unconstrained by society, act immorally? And if power corrupts the ruler, why should collectivities be immune—especially in their relations with other states? Kant's suggestion that autocratic rulers might "resolve on war as a pleasure party for the most trivial reasons" (quoted in Doyle 1983a, 229) does not resolve this contradiction. Kant's liberalism ultimately falls short of being a positive *theory* of democratic pacifism because it lacks what today we would call a fully developed and consistent set of micro motives.[23]

The theory I have summarized above offers an alternative explanation for the relative pacifism of democratic states that nonetheless builds upon, and subsumes, many of Kant's essential insights. First, as I have demonstrated, democratic states will tend to be less expansionist than autocratic states. The larger the rent-earning ability of the state, the greater the optimal size of the territorial unit, and the greater the incentives for the state to try to reach this optimal size. To the extent that war is a necessary byproduct of expansion, it follows (ceteris paribus) that autocratic states will be more war-prone. Where, for Kant, republican institutions restrain the capriciousness of the ruler, democracy, in this approach, constrains the ability of the state to extract monopoly rents at society's expense.

Second, the theory suggests that democracies will often be the object of expansion by autocratic states. Democracies pose two threats to the rent-seeking ability of autocracies. As I have noted, democracies, by their very existence, serve as magnets that pull individuals out of autocratic polities; in the absence of a low-rent democratic haven, exit is less likely to occur. By eliminating democracies, autocratic states can reduce the gains from, and incentives for, emigration. In addition, through their political openness and richer informational environments, democratic states reduce the costs to autocratic societies of monitoring state behavior; by observing democracies, citizens in autocracies are more likely to become aware of the magnitude and consequences of state rent seeking.[24] Again, by eliminating democracies abroad, autocratic states solidify and reinforce their rent-earning abilities. For both of these reasons, democracies, perhaps unwittingly, may become targets of the expansionary activity of autocracies—thus expanding their overall war involvement despite their own pacific nature.[25]

Finally, democratic states may engage in expansion and even intervene in other countries, as Doyle claims, but only under restricted conditions. Democracies will expand *only* when the initial costs of conquest and ongoing costs of rule are less than the discounted present value of future economic profits. Under these conditions, expansion is socially optimal regardless of regime type. Nineteenth-century European imperialism may provide examples of such socially efficient expansion: in many areas of the periphery the cost of war against more "primitive" peoples was relatively low and the potential gain from the erection of a "modern" state with its greater extractive capacity disproportionately large. With the rise of "European-style" states in the periphery dur-

ing the twentieth century, however, the cost of imperialist expansion has substantially increased, apparently foreclosing this option.

By the same principle, democracies may also preemptively intervene in the domestic affairs of an autocracy to construct democratic political structures as long as the costs of the intervention are less than the expected costs of a war stimulated by state rent seeking. In this view, the proactive policy of democracies rests not on a moral imperative but on a rational calculus of preemption.

Together, these propositions imply that democracies are, on average, no more or less war prone than other states. On the other hand, democracies are less likely to fight each other, for only in this area is the absence of an imperialist bias manifest. Indeed, the almost complete lack of war between democracies suggests just how important state rent seeking may be as a source of international conflict.

THE PROPENSITY FOR VICTORY

Democracies are not only less likely to wage war with each other, they are also significantly more likely to win the wars they fight against autocracies. Liberalism offers a possible explanation for this correlation: democracies tend to turn international conflicts into ideological crusades and demand total victory.[26] Yet, as I have noted, the micro motives for democratic crusading are, at best, unclear. Moreover, within this framework, ideological fervor need not translate into victory; for the fear that the winner will transform the loser's social structure may only spur the failing side onto greater efforts. The drive for victory need not yield the desired result.

The theory summarized in Part I suggests a second explanation. To the extent that democratic states earn fewer rents, it follows that they tend (1) to create fewer economic distortions, possess greater national wealth, and devote more resources to security; (2) to enjoy greater societal support for their policies and therefore a greater extractive capacity; and (3) to form overwhelming countercoalitions against expansionist autocracies. I shall develop each of these propositions in turn.

State rents, like the rents earned by private actors, distort patterns of production and consumption, divert resources into directly unproductive activity, create social deadweight losses, and thereby reduce the total product of goods and services within an economy. While the successful rent earner is better off, the economy as a whole suffers. At the very least, growth rates lag behind their potential. North (1981) has examined the effects of state rent seeking on property rights and, in turn, growth; but the argument is more general.[27] Any state rents, including monopoly rents earned through the exchange of state-produced services such as protection, distort the economy and, over time, lower national wealth relative to its potential.[28]

The wealthier the country (ceteris paribus), the more absolute resources it devotes to producing security. For any given set of costs and preferences, wealthier countries produce and consume greater amounts of all goods and services, including protection; under normal conditions, an increase in wealth affects only the level, not the mix, of goods in the economy. It follows, then, that wealthier countries produce and consume greater amounts of security (i.e., provide greater absolute resources to the state for producing protection). In contests where sheer resources matter, the cumulative effects of lower state rents may prove decisive.

Democratic states should also possess greater societal support for their policies, suggesting that they will enjoy a greater extractive capacity for any given level of national wealth. All societies make trade-offs between consuming what are mostly private goods and services and consuming state-produced protection; while both constitute current consumption, the latter serves as insurance that societies will be able to enjoy their present and future holdings of wealth. During hostilities, when the external threat to national wealth, territorial integrity, and the present form of rule is most acute, individuals and, in turn, society will tend to purchase greater quantities of protection (pay higher taxes), thereby transferring greater resources to the state for the waging of war.

In view of their exploitative nature, however, autocratic states pose a greater threat than do nonautocratic states to the current and future ability of democratic societies to produce and consume wealth; as a result, democracies should demand greater protection against these threats and contribute proportionately more to ensure victory in war. Within the theory developed here, individuals are indifferent between being ruled by two equally democratic regimes; each provides similar levels of protection close to the normal profit price (p in Figure 1). As a result, there is no incentive for citizens of one democracy to purchase protection against another equally democratic state unless there is some uncertainty about the likely behavior of the democratic conqueror. Autocratic states, on the other hand, provide protection only at a higher price, with the difference between the normal profit and higher prices (p and r in Figure 1) being captured by the state as rents. In this case, citizens of democracies do possess incentives to purchase protection against this threat to their current and future wealth; indeed, the greater the expected rent seeking from an autocratic conqueror, the more protection citizens will demand, and pay for, from their present democratic state.

Conversely, autocratic societies may actually benefit from defeat—if the victorious democratic states remake the autocracies in their own images. As a result, societies in autocratic polities should be willing to contribute proportionately *less* than democratic societies. While autocratic states will seek to offset this tendency by vilifying the enemy (instilling fear that defeat will mean national destruction, rape, and slavery), a fifth column remains a real possibility. Autocracies may also increase the degree of coercion

used to extract resources; but this implies higher transactions costs for revenue collection, which will further disadvantage them relative to democracies. At the very least, the lack of societal support places a real constraint on their extractive capabilities. Lamborn's (1983, 1991) study of the great powers during the late nineteenth century supports this expectation, as does the tendency of states to expand the franchise or otherwise liberalize politically during or immediately after major wars.[29]

Finally, to the extent that states balance threats, rather than power (as Walt [1987] has argued and as is consistent with the logic I have developed), democratic states should form overwhelming countercoalitions against autocratic states. Not only are autocracies more likely to seek territorial expansion, they are more likely to target democracies (to reduce exit options). In addition, autocratic expansion poses a greater threat to democracies because of the larger rents the state is likely to extract if successful. The greater the threat (ceteris paribus), the greater the balancing reaction by other states.

Given its imperialist bias and likely behavior if successful, the threat posed by an autocracy is proportionately greater than the sum of its aggregate resources, the traditional measure of national power. It follows that the countercoalition that forms against any autocracy should be disproportionately large or overwhelming. If autocracies have greater incentives to expand, democracies have greater incentives to resist. As a result, this coalition should also be disproportionately composed of democractic states.[30] The overlarge "democratic" coalition should *deter* autocratic expansion (by raising the costs of conquest) and, if deterrence fails, be more likely to win. Combined with the greater wealth and extractive capabilities of the individual states, this suggests that the democratic coalition should be virtually invincible. The overlarge coalitions formed during World Wars I and II and, more strikingly, the Cold War (often understood as anomalies in realist theories of international relations; see Waltz 1979) bear out this expectation.

Evidence

Many of the concepts central to the theory summarized here are difficult to operationalize or lie beyond current data-gathering techniques. Extractive capability, for example, is unmeasurable: military spending as a proportion of gross national product (a commonly used indicator) may be distorted and inflated by state rents. Nonetheless, the propositions I have developed suggest that democratic states should tend to win wars—a derived hypothesis that provides an indirect test of the theory.

The historical record is striking. Of the 30 wars listed in the Appendix, 3 (the Korean, Israeli–Egyptian, and Israeli–Syrian Wars) must be excluded from this analysis for want of a clear victor. I also exclude the Spanish–American War, fought between two democracies, but include World War II despite Finland's exceptional position. Of the 26 wars fought since 1816 between democracies and autocracies, the former have won 21 (81%) and lost 5 (19%). In other words, democratic states, either singly or in combination with other states, have won four times as many wars as autocratic states. Excluding the First and Second Balkan Wars, where Greece was the sole democracy on the winning side, does not appreciably change these results: the democracies still win 19 (79%) of 24 wars.

TABLE 1

Regime Type and Victory in War (Individual Participants)

SUCCESS IN WAR	NATURE OF REGIME		TOTAL
	AUTOCRATIC	DEMOCRATIC	
Loser	42	9	51
Winner	32	38	70
Total	74	47	121

Note: All wars in the Appendix are included except the Spanish–American War of 1898 and wars in which no clear winner emerged. Gamma = .694; chi squared = 16.673; df = 1; p = .000046.

Scoring each participant individually yields a strong and significant correlation between democratic victory and autocratic defeat. Table 1 breaks down all 121 participants in the 26 wars according to regime type and outcome. This construction biases the results against the hypothesis by coding as winners the not-inconsequential number of autocracies who fought as members of victorious democratic coalitions. Nonetheless, the degree of association is strong, indicating that even with this bias democratic states are significantly more likely to win—and autocratic states more likely to lose—than the converse.

Rather than relying upon a simple dichotomy, it is possible to examine the average degree of democracy in the sets of winners and losers of these 26 wars. Using an 11-point scale of democracy (0–10), the mean of the 70 winners is 5.60 and the mean of the 51 losers is 2.55.[31] The probability that these figures would emerge by chance is less than .001 (t = 4.43; df = 119).

Finally, the relationship between democracy and victory is quite robust. Table 2 presents a logit analysis performed with the 121 war participants as the units of observation. Along with democracy, the analysis included military personnel, a measure of military strength, iron and steel production, a proxy for industrial capacity, and a dummy variable indicating whether or not the country initiated the war.[32] Common sense and, for war initiation, previous research (Bueno de Mesquita 1981, 22), suggest that all of these relationships should be positively related to victory.

Democracy is consistently positive and significant, offering strong support for the argument developed here. Military personnel and iron and steel production, on the other hand, are insignificant at standard

TABLE 2

Logit Analysis of Victory in War

VARIABLE	EQUATION 1	EQUATION 2
Constant	.4214	−.7527*
	(.4461)	(.3346)
Democracy (0–10)	.1933*	.2524***
	(.0763)	(.0597)
Military personnel (millions)	.3701	.4968
	(.5325)	(.4188)
Iron and steel production (millions of tons)	.0024	−.0334
	(.0542)	(.0201)
Initiator[a]	−1.3982**	—
	(.5126)	
Log likelihood	−48.05	−71.63
n	87	121
Percent correctly predicted	72.41	67.77

Note: Standard errors are in parentheses. Dependent variable is war outcome (loser = 0; winner = 1).
[a]0 = *no*; 1 = *yes*.
*$p < .02$.
**$p < .01$.
***$p < .001$.

levels in both equations, suggesting that in this set of wars neither military nor economic strength is associated with victory. When included, war initiation is significant but in the wrong direction; surprisingly, in wars between democracies and autocracies, noninitiators win more frequently.

The failure of these alternative explanations to predict victory or loss correctly does not imply that common sense or previous research is wrong. Rather, it highlights the exceptional nature of war between democracies and autocracies. In these conflicts, military strength, industrial capacity, and the ability to choose to wage war appear to be far less important determinants of victory than governmental form.

CONCLUSION

Regime type does matter in international politics. Democracies are less likely to fight wars with each other. They are also more likely to prevail in wars with autocratic states. This syndrome of powerful pacifism accords, in part, with "Kantian" liberalism; but because of inconsistent behavioral assumptions, this normative frame cannot be said to constitute a positive theory of international relations.

I have offered an alternative explanation drawn from the microeconomic theory of the state. Specifically, state rent-seeking creates an imperialist bias in a country's foreign policy. This bias is smallest in democracies, where the costs to society of controlling the state are relatively low, and greatest in autocracies, where the costs are higher. As a result, autocracies will be more expansionist and, in turn, war prone. To the extent that democracies do wage occasional wars of expansion, intervene in the domestic affairs of autocracies, and are targets of autocratic expansion, there should be no significant overall difference in their frequency of war involvement. Only in their relations with each other does the relative pacifism of democracies appear. In addition, democracies (constrained by their societies from earning rents) create fewer economic distortions and possess greater national wealth, enjoy greater societal support for their policies, and tend to form overwhelming counter-coalitions against expansionist autocracies. Thus, democracies will be more likely to win wars.

If democracies are powerful pacifists, why do autocracies persist within the international system? It follows from the arguments I have outlined that democracy is an evolutionarily superior and stable form of rule. If so, why has democracy not displaced autocracy?

On the one hand, democracy *has* expanded. The number of democratic countries has grown from a mere handful in the eighteenth century to over 60 in the early 1980s.[33] The recent transformations in Eastern Europe suggest the promise of further liberalizations elsewhere, although in my opinion the tide could easily be reversed. But given the relative infrequency of war, there is no reason to presume that political change will be rapid; the evolutionary pace of the global system may well be glacial.

On the other hand, democracies only *tend* to win wars; historically, for every four they win, they lose one. Nor is there any consistent trend in favor of the democracies: autocracies were most successful in the 1960s, winning half of the wars in which they were engaged, but entirely unsuccessful in the 1970s and 1980s (see Appendix). Any evolution toward greater democracy is likely to be characterized by fits and starts. Four steps forward, one step back.

APPENDIX: ALL WARS INVOLVING DEMOCRATIC STATES, 1816–1988

In the following list, boldface in the righthand column indicate democratic states. Bulgaria, Romania, Italy/Sardinia, and France which fought on both sides of World War II are listed here on their initial sides.

WAR	PARTICIPANTS (WINNERS FIRST)
1. Mexican–American 1846–48	**United States** vs. Mexico
2. Roman Republic 1849	The two Sicilies, **France**, and Austria–Hungary vs. the Papal States
3. Crimean 1853–56	**United Kingdom**, Italy/Sardinia, France, and Turkey/Ottoman Empire vs. USSR/Russia
4. Anglo–Persian 1856–57	**United Kingdom** vs. Iran/Persia
5. Sino–French 1884–85	**France** vs. China
6. Greco–Turkish 1897	Turkey/Ottoman Empire vs. **Greece**
7. Spanish–American 1898	**United States** vs. **Spain**
8. Boxer Rebellion 1900	Japan, **United Kingdom**, USSR/Russia, **France** and **United States** vs. China
9. Spanish–Moroccan 1909–10	**Spain** vs. Morocco
10. First Balkan 1912–13	**Greece**, Yugoslavia/Serbia, and Bulgaria vs. Turkey/Ottoman Empire
11. Second Balkan 1913	Turkey/Ottoman Empire, **Greece**, Yugoslavia/Serbia, and Romania vs. Bulgaria
12. First World War 1914–18	Japan, Belgium, **United States**, Yugloslavia/Serbia, **United Kingdom**, **Portugal**, Romania, **France**, **Greece**, Italy/Sardinia, and USSR/Russia vs. Turkey/Ottoman Empire, Austria–Hungary, Germany/Prussia, and Bulgaria
13. Hungarian–Allies 1919	Romania and **Czechoslovakia** vs. Hungary
14. Russo–Polish 1919–20	**Poland** vs. USSR/Russia
15. Russo–Finnish 1939–40	USSR/Russia vs. **Finland**
16. Second World War 1939–45	**United Kingdom**, **Australia**, **Canada**, Ethiopia, Poland, **United States**, USSR/Russia, **Belgium**, Brazil, China, Yugoslavia/Serbia, **Netherlands**, **New Zealand**, **France**, **South Africa**, Greece, **Norway**, and Mongolia, vs. Japan, Italy/Sardinia, Germany/Prussia, Romania, Bulgaria, Hungary, and **Finland**
17. Palestine 1948	**Israel** vs. Jordan, Iraq, Syria, Egypt/UAR, and Lebanon
18. Korean 1950–53 (no clear victor)	**Greece**, **United Kingdom**, **Canada**, Thailand, **Belgium**, **Turkey/Ottoman Empire**, **United States**, **Australia**, **Netherlands**, Ethiopia, Columbia, **Philippines**, **France**, and South Korea vs. North Korea and China
19. Sinai 1956	**United Kingdom**, **France**, and **Israel** vs. Egypt/UAR
20. Sino–Indian 1962	China vs. **India**
21. Second Kashmir 1965	Pakistan vs. **India**
22. Vietnamese 1965–75	North Vietnam vs. Thailand, South Vietnam, **United States**, Kampuchea/Cambodia, **Republic of Korea**, **Australia**, and **Philippines**
23. Six Day 1967	**Israel** vs. Egypt/UAR, Jordan, and Syria
24. Football 1969	**El Salvador** vs. Honduras
25. Israeli–Egyptian 1969–70 (no clear victor)	**Israel** vs. Egypt/UAR
26. Bangladesh 1971	**India** vs. Pakistan
27. Yom Kippur 1973	**Israel** vs. Egypt/UAR, Iraq, Syria, Jordan, and Saudi Arabia
28. Turko–Cypriot 1974	**Turkey/Ottoman Empire** vs. Cyprus
29. Falklands 1982	**United Kingdom** vs. Argentina
30. Israeli–Syrian (Lebanon) 1982 (no clear victor)	**Israel** vs. Syria

Notes

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1. For an extreme view, see Crozier, Huntington, and Watanuki (1975). Russett argues that cyclical majorities may be the greatest problem facing the democratic formulation of foreign policy (1990, 115–7).

2. This theory has also been referred to as the neoclassical theory of the state (North 1981) and the predatory theory (Levi 1983, 1988). Both of these appellations have misleading

normative implications. I prefer the more neutral term used here. See also Auster and Silver 1979. Within this approach there is some disagreement about the objective that states pursue. North and Levi, for instance, both assume that states maximize revenue, while I assume that states seek profits. For an elaboration of this debate and defense of the profit-maximizing assumption, see Lake 1990.

3. In producing protection and collecting revenues, the state consumes resources—everything from paper clips and stationary to stealth bombers and nuclear weapons. States also consume labor. As noted, the real cost of these resources is determined by their fair market value or (in cases where the state is a dominant consumer) the price the state could obtain if it exercised its monopsonist power—a factor much more important for weapons than, say, for office supplies.

4. The public nature of the good protection complicates the definition of price and demand. By price, I mean the rate at which the state extracts wealth (for practical purposes, the tax rate [t] multiplied by the inverse of the probability (p) of successful evasion (free riding); that is, price = $t(1 - p)$. By *positive demand* I mean that individuals prefer some level of protection at some positive cost to themselves (i.e., price) to no protection at zero cost. Strictly speaking, the demand for public goods is summarized in an evaluation schedule, which differs from a traditional demand curve only in that individual preferences are added vertically, rather than horizontally, in order to reflect nonrivalry in consumption. Following common practice, I nonetheless refer to this evaluation schedule as a demand curve. On the treatment of public goods within a Marshallian economic framework, see the seminal work by Buchanan (1968). My approach differs from that of Buchanan (who adopts an essentially pluralist approach to politics) by including a theory of the state as a monopoly provider of public goods. This has important implications for the point of equilibrium production. See n. 6.

5. Plotting the probability of success in armed conflict against the quantity of resources mobilized for war, Emmerson (1983, 432) argues that protection forms an *S*-shaped curve and that once the probability of success reaches 1.0, the country has entered a "zone of redundancy" in which it has "overmobilized." This clearly implies that the demand for protection can be sated. As I have argued however, the security dilemma (and the action–reaction syndrome implied therein) is likely to prevent the probability of success from reaching its upper bounds. Emmerson further argues that charismatic leaders are most likely to arise when the probability of success is in its middling range and that once a state enters the zone of redundancy, it and its leadership will decay. Interestingly, Emmerson's thesis is contradicted by his own evidence. The ancient states of Balti (the focus of his essay) began to decline in 1846 when they were absorbed by the maharaja of Kashmir and, behind him, the British Government of India (p. 438). This absorption, of course, implies that the Balti's probability of military success against these superior powers was significantly less than 1.0.

6. In pure competition, the firm also produces where marginal cost (MC) equals marginal revenue (MR). Facing a horizontal demand curve, however, marginal revenue equals price and, in turn, demand (D); and the competitive firm, in theory, produces where marginal cost equals demand, that is, where MR = D = MC = MR. In Buchanan's (1968) essentially pluralist view, this is also the Pareto optimal equilibrium for the production of public goods and the point against which all distortions must be measured. Adding a concept of the state as a monopoly provider of public goods significantly alters this equilibrium, however. With a downward-sloping monopoly firm demand curve, supplying protection where marginal cost equals demand would result in a significantly larger quantity; but all units to the right of q in Figure 1 would be produced at a loss (MC > MR). What is efficient for society is inefficient for the state, and vice versa. I shall argue that society can control, in part, the quantity of monopoly rents earned by the state; likewise, society can control in part the level of production, with the resulting equilibrium arising somewhere between MR = MC (the state's preferred level) and MC = D (society's preferred level). For expositional clarity, I shall assume henceforth that production occurs where MR = MC and that political conflict occurs primarily over the level of monopoly rents earned by the state. Relaxing this assumption, however, only reinforces my arguments on democracy and the syndrome of powerful pacifism.

7. At the minimum, the protection service must generate normal profits for state officials, defined as the reservation wage; if the state fails to earn at least normal profits, the individuals who comprise it will leave and assume other employment.

8. This conclusion follows even without the assumption advanced in n. 6 as long as society faces positive costs for monitoring state behavior and engaging in exit and voice.

9. On principal–agent problems, see Fama 1980; Fama and Jensen 1983a, 1983b; Jensen and Meckling 1976; and Ross 1973.

10. A free press is an exception to this rule. News organizations specialize in collecting information (especially on state activities) and selling it to the public.

11. See Tiebout 1956. This is analogous to North's (1981) concept of external competition, where other states exist to act as alternative suppliers of protection. On asset mobility, see Bates and Lien 1985. The phrase *low rent* refers not to the overall tax rate but only to the level of supernormal profits earned by the state on the services it provides. Different states are likely to provide different mixes of services with corresponding tax levies. Following Tiebout, emigrants are likely to select the polity that offers the mix of services they want at the price they are willing to pay. The analysis here focuses only on state rents, which distort the implications of the now-standard Tiebout analysis.

12. Although the direct costs for voting are small to nil, there are certainly opportunity costs for activities forgone and possibly some indirect costs (e.g., transportation to the polling station).

13. This is not to deny that some groups within autocratic polities can participate at relatively low cost and thus have significant influence over the state. In these circumstances, it is expected that the powerful social groups would share in the rents earned by the state. The argument here hinges on the average cost for society as a whole. See also n. 15.

14. Expansionary policy is to be taken in opposition to unilateralism and cooperation. See Lake 1990, 1991. This analysis contrasts with other explanations of so-called overexpansion or with a country's expansion beyond the point where the marginal costs equal the marginal gains to society. Rather than being driven by perverse incentives within the international system, cognitive bias, dominant social groups, or log rolls between concentrated social interests (see Snyder 1991), the explanation developed here focuses on the monopoly structure of the protection industry and the costs to society of regulating state behavior.

15. This argument applies only for rents earned and retained by the state. If the state is merely a conduit for redistributing wealth between social groups, no imperialist bias will emerge. Thus, I differ crucially from much of the extant literature on rent seeking, which tends to focus on the actions of *social* groups (e.g., Olson 1982). While social rent seeking will reduce national wealth as well, I have yet to see a convincing argument that democracies are more prone to social rent seeking than autocracies. Interestingly, even though Olson adopts a pluralist conception of the state, he argues that his theory is supported by evidence from both developed democracies and nondemocratic, non-Westernized polities (1982, 146–80). Although I do not find it persuasive, Ekelund and Tollison (1981) do argue that democracies will actually experience less societal rent seeking than autocracies. My intuition is that democratic and autocratic societies extract similar levels of rents but distribute them over greater and smaller sets of groups, respectively. I contend here, however, that rents earned by democratic *states* are significantly less than the rents extracted by autocratic *states*.

16. Monitoring costs will be higher, however, where the

state can persuade society that "national security" considerations require a higher degree of secrecy.

17. Just as the absence of supernormal profits by entrepreneurs in perfectly competitive economic markets does not vitiate the assumption of profit maximization and, in fact, follows from this assumption, the absence of rents does not undermine the assumption of state profit maximization. Rather, the receipt of only normal profits in ordinary economic enterprises and the state reflects the presence of acute competition and the constraints of society, respectively.

18. For studies supporting these conclusions, see Chan 1984; Ember, Ember, and Russett 1991; Maoz and Abdolali 1989; Rummel 1983; Small and Singer 1976; and Weede 1984.

19. Wars and participants are from Small and Singer 1982, updated through 1988 from Singer 1991. Regime type is from Gurr 1990. Democracy (DEMOC, variable 6.2) is an additive 11-point scale (0–10) based on the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. Countries that score 6 or more on this scale are classified here as democracies; countries that score 5 or less, as autocracies. There are relatively few countries that score in the middle of this range. Although countries that score 5 on this scale are not, in my view, normally considered to be democratic, lowering the threshold a point or two would not significantly alter this list of wars or the analysis.

20. In a personal communication, Michael Doyle has suggested that it is this mutual republican recognition that separates modern democracies from the ancient Greek democracies, which appear not to have been substantially more pacific than their autocratic contemporaries (see Russett and Antholis 1991). My explanation would focus on the relatively narrow franchise in these so-called democracies.

21. Doyle distinguishes between strong and weak autocrats, suggesting that democracies will not war with the former (a point consistent with my analysis below) but will fail to take advantage of mutually advantageous agreements (such as arms control) or opportunities to exploit such states. I question the extent to which this "stylized fact" holds, as democracies often do ally with one autocrat against another even in times of peace. The United States, for instance, played the "China card" in the 1970s. Nonetheless, to the extent that this tendency does exist, it cannot be reconciled with the theory summarized here.

22. Although only implied in their writings, this emphasis on forgoing material gains and incurring material losses is essential for both Kant and Doyle; in its absence, democratic pacifism is not moral but is economically efficient—a position that both authors claim to supersede.

23. Several scholars have recently attempted to formalize a model of foreign policy decisionmaking that focuses on the greater domestic constraints democratic societies exert on their leaders. These "neo-Kantians," however, suffer from the same problem as their intellectual progenitor. Morgan and Campbell base their analysis on "two key assumptions: (1) that a democratic political structure imposes constraints on the decision-making process by restricting the key decision makers' freedom of choice; and (2) that with regard to decisions for war, these constraints push toward peace (i.e., heads of state would be more likely to opt for war than those they govern) (1991, 189–90)." Bueno de Mesquita and Lalman (1992) find empirical support for Morgan and Campbell's first assertion but do not examine the second, more fundamental proposition. Like Kant, these authors do not deduce motivations or explain why leaders are more war-prone and the populace more pacific. While it might be understood as an alternative explanation of democratic pacifism, I see this work as complementary to the analysis offered here.

24. This suggests why "insulation" is an essential element of the totalitarian model. See Friedrich, Curtis, and Benjamin 1969.

25. This argument also implies that autocracies should initiate wars with democracies. Given the complexities of military strategy and the often-considerable gains from surprise attack and preemption, however, who actually fires the first shot or crosses the border (the basis of most standardized codings of war "initiation") is a poor indicator of who actually provoked, or "caused," the war. In addition, since history is typically written by the victors (and, as I shall show, democracies win a disproportionate number of wars), even more subjective estimates of war initiation may well be biased. As a result, I do not place much faith in patterns of war initiation as a test of the theory presented here. Following the coding rules used in Table 1, however, the correlation (gamma) between democracy and war initiation is correctly signed but very weak (−.04) and insignificant. To the extent that this result is valid, it must count against the theory.

26. Contradicting the "crusading" hypothesis, Russett finds that the surge in public support for the president following an aggressive foreign policy action (often referred to as the "rally-round-the-flag" phenomenon) is typically short-lived, with a half-life of no more than a month or two (1990, 94).

27. See especially Olson 1991, which extends to the state Olson's earlier work on social coalitions. Echoing North and the analysis here, he finds that democracies should experience lower state rents and higher growth.

28. This is a long-run, dynamic argument; it does not imply that all democracies are wealthier than all autocracies. The entire literature on the microeconomic theory of the state directly contradicts that on the so-called developmental state. The latter approach asserts that strong, autonomous states have to deter social rent seeking and stimulate growth under conditions of late industrialization (see, e.g., Evans 1979). As I suggest in n. 15, I do not expect there to be a systematic difference between the *level* of rents extracted by social groups in democracies and autocracies. Moreover, the literature on the developmental state is generally mute on the motivations of state officials: Why do political leaders choose the "public" interest over their own private interests? The microeconomic theory of the state suggests that strong, autonomous states will typically exploit, rather than develop, their societies except where they face what North (1981) has termed severe *external constraints* (analogous here to very low costs of exit). Interestingly, most, but not all, of the developmental states have experienced clear threats from other states (e.g., South and North Korea, Taiwan and China). In such polities, especially those producing far within their production possibility frontiers, this analysis predicts that states will opt for increased normal profits from growth over increased rents from redistribution. In the long run, the theory suggests that these developmental autocracies will either ossify and once again become a drag on development (as in the former Soviet Union) or liberalize (as in South Korea).

29 For the domestic consequences of war, see Stein 1978. On political reform in Germany during World War I, see Feldman 1966; and on the Soviet Union during World War II, see McCagg 1978, esp. 18–23.

30. Siverson and Emmons (1991) find that democracies have a greater propensity to ally with one another than with other states.

31. See n. 19 above. As noted, at least one participant in each war is democratic (defined by a minimum score of 6 on an 11-point scale). The mean score of 5.60 suggests that the "average" winner in these 26 wars was autocratic, although there were more democracies (38) than autocracies (32) in the population (see Table 1).

32. Data on military personnel and iron and steel production are from the July 1990 Correlates of War, National Materials Capabilities data set (J. David Singer and Melvin Small, principal investigators). An attempt was made to include data on military expenditures and energy usage from this same data set, but there were too many missing values for the early nineteenth century. Data on war initiation is from Small and Singer 1976. Unfortunately, this set covers only the period through 1965 and does not include several wars later added in Small and Singer 1982; this accounts for the reduction in the number of observations in Table 2, equation 2. The proviso stated in n. 25 holds here, as well. I also assessed the

possible presence of interaction effects among these measures by multiplying democracy with military personnel, iron and steel production, and war initiation. In every case, these interaction terms were insignificant. These results are not reported here.

33. On the diffusion of democracy, see Starr 1991.

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POLITICS AND THE STRUCTURAL DEPENDENCE OF THE STATE IN DEMOCRATIC CAPITALIST NATIONS

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I explore empirically a central claim of the structural dependence thesis, namely, that capitalists' ability to disinvest fundamentally conditions policy choices in democratic capitalist systems. Utilizing time-series data for 16 affluent democracies from 1965 to 1984, I find that, indeed, low rates of business investment are associated with reductions in corporate tax burdens and that these reductions are more pronounced in periods of economic crisis. Moreover, low rates of capital formation engender cuts in personal income taxes during periods of economic stress. However, I also find that the magnitude of responsiveness of taxation to low rates of investment is relatively small and that analyses of the political context of investment and taxation indicate that governments have choices. The responsiveness of corporate tax burdens to capital formation may, under some governments, be part of a policy mix designed to maintain adequate investment and to address the demands of core constituencies.

A central question for students of political–economic interaction in democratic capitalist nations is to what extent politics shapes the policy choices of authoritative actors within the state. For many, this question amounts to whether or not left-wing governments differ from others in their social and economic policies and impacts (e.g., Castles 1982; Hibbs 1987a, 1987b; Stephens 1979). For some, the question also extends to the role of union movements, neocorporatism, and economic structure in shaping redistributive policies and outcomes (Hicks and Swank 1984a; Muller 1989) and economic policies and performances (Hicks 1988; Lange and Garrett 1985). Still others stress the policy impacts of facets of democratic electoral competition, such as the electoral cycle (Nordhaus 1975; Tufte 1978). Yet an important claim of several observers of democratic capitalism, both neopluralist (Lindblom 1977; Stone 1980) and neo-Marxist (Block 1977; Miliband 1969; Offe 1984), is that the social and economic policies of all governments are fundamentally conditioned by their dependence on the willingness of capitalists to continue to invest. This assertion, dubbed the *structural dependence* (of the state on capital) thesis by Przeworski and Wallerstein (1988), is the central focus of the present inquiry.

The structural dependence thesis is by now familiar.[1] Frequently couched in terms of the "privileged position of business" in extracting investment incentives from the state (Lindblom 1977, chap. 13) or of the "structural power" of capital to constrain the range of state action (Block 1977), the thesis embodies a number of important propositions about the relationship between capitalism, politics, and the state in liberal democracies. After a brief review of the thesis and extant evidence on its accuracy as a theory of state behavior in capitalist democracies, I shall present an analysis designed to evaluate one of the thesis's central claims about the role of capital investment as a determinant of governmental policy choices. I shall focus on the responsiveness of corporate income tax policies to the ebbs and flows of business investment in productive activities in a sample of 16 affluent Western democracies from 1965 to 1984. To provide a comparative benchmark and to explore certain questions that emerge from the principal analyses, I shall also extend the inquiry to cover personal income taxation.

THE STRUCTURAL DEPENDENCE OF THE STATE: THEORY AND EVIDENCE

The bedrock of the structural dependence thesis consists of four sets of propositions about the interaction of politics and economics in democratic capitalist systems.[2] Taken individually, most of these claims seem uncontroversial. First, proponents of the thesis argue that economic performance (output, jobs, price stability, and income) is significantly determined by the willingness of the owners of capital to continue to invest in productive activities. As Lindblom argues: "Because public functions [i.e., investment] in the market system rest in the hands of businessmen, it follows that jobs, prices, production, growth, the standard of living, and the economic security of all rest in their hands. . . . Constitutional rules—especially the laws of private property—specify that, although governments can forbid certain kinds of activity, they cannot command business to perform." (1977, 172–73).

Second, structural dependence theorists argue that the role of investment in capitalist democracies is crucial for state policymakers because of the nature of democratic politics and institutions themselves (Block 1977, 15; Lindblom 1977, chap. 13). In the view of these theorists, as well as most students of political behavior in Western democracies, performance of the economy (income growth, employment levels, and

inflation) is crucial for the continued popular approval and reelection of incumbent policymakers (Hibbs 1987a, 1987b; Kiewiet 1985; Lewis-Beck 1986). Moreover, maintenance of high and expanding levels of economic activity is necessary for extraction of the revenues that provide the lifeblood of government personnel and programs.[3] Block sums up the argument succinctly: "Those who manage the state apparatus . . . are dependent on the maintenance of some reasonable level of economic activity. This is true for two reasons. First, the capacity of the state to finance itself through taxation or borrowing depends on the state of the economy. . . . Second, public support for a regime will decline sharply if the regime presides over a serious drop in the level of economic activity" (1977, 15).

Third (following from the first two and particularly important to the present inquiry), proponents assert that if governments attempt to pursue adverse economic regulatory policies, dramatically alter the distribution of income through significant changes in transfer and tax policies, and otherwise pursue policies that may necessarily increase taxes directly or indirectly on business, they potentially undercut the confidence of business in a profitable rate of return on investment and so reduce growth. As Offe has put it: "As long as investment decisions are 'free', that is, as long as they obey the rule of maximum expected profitability, the decisive variable . . . is what Kalecki called 'business confidence'. The ultimate political sanction is non-investment or the threat of it" (1984, 244).

Moreover, as some structural dependence theorists suggest, state policymakers may frequently find it necessary to offer incentives to business to maintain or restore adequate capital investment (e.g., Lindblom 1977, chap. 13). According to the logic of the thesis, support-maximizing and revenue-seeking governments will necessarily regularly provide investment incentives in the face of low rates of investment and be loath to pursue policies detrimental to business confidence to any significant degree.[4]

Finally, the structural dependence thesis is unique in its claims about the mechanisms by which capital obtains desired state policies. Distinct from many theories of the state, the structural dependence thesis embodies the notion that policy-oriented collective actions by businessmen and the organizations that represent them are not necessary to secure investment incentives. As some theorists of structural dependence say quite explicitly, individual businessmen need only calculate rationally that under the current mix of extant or proposed policies the rate of return on investment is insufficient (e.g., Lindblom 1977, 170–75). The ensuing downturn in investment is sufficient to elicit policy response in the absence of conscious attempts to do so through collective action.[5]

Evidence

Thus, there is ample evidence to suggest that economic performance directly affects the public's evaluation of government performance and, in turn, the support and votes that the public provides to incumbents. The proposition that the continuation of business investment in productive capacity is crucial for economic performance is also uncontroversial. However, the notion that the state actually behaves in the manner suggested by the thesis has received remarkably little attention.

Some evidence for the thesis as a theory of state behavior is anecdotal. Efforts at significant redistributions of income and wealth in Allende's Chile, Manley's Jamaica, and even Mitterrand's France are said to have led to serious falls in business confidence and, in turn, economic downturn and policy retrenchment. These analyses of "limiting cases" (e.g., Rueschemeyer and Evans 1985) are instructive but do not tell us much about the relationships between the ebbs and flows of capital formation on the one hand and the provision of investment incentives and other policies designed to enhance business confidence on the other. But it is these more routine political–economic interactions that serve as a crucial test of the generalized form of the structural dependence thesis; that is, if (as the thesis maintains) the crucial linkage is between investment-induced growth and disinvestment-induced decline on the one hand, and popular support, votes, and revenues on the other, vote-maximizing and revenue-seeking incumbents with an electoral horizon of three to five years would certainly display some consistent sensitivity to the basic determinants of growth. Downturns in business investment may call forth immediate, direct investment incentives and other policies that eliminate investment disincentives. To paraphrase Keynes, the logic of the thesis suggests that incumbents may be dead in both the short and long term if they do not regularly bolster the conditions that underpin continued investment.[6]

A handful of recent studies provides limited evidence on the responsiveness of government policies to investment in capitalist democracies. In an effort to assess the structural dependence thesis, a small number of studies explicitly examine the responsiveness of corporate income taxation to investment or general economic downturn. In a study of effective corporate tax rates in the postwar United States, Jacobs (1988) reports that there is no relationship between variations in business investment and corporate tax policy. Complementary results are reported by Quinn and Shapiro (1990) in a study of business influences on United States tax policy: variations in capital investment are either unrelated or negatively related to measures of corporate income taxation and to the distribution of tax burdens between owners of capital and wage earners.

On the other hand, Quinn (1988, 1989) has also reported results consistent with the structural dependence thesis. Examining the relationship between investment incentives and economic conditions in France, Great Britain, Japan, the United States, and West Germany, Quinn (1988) finds that governments increase investment incentives during periods of eco-

nomic downturn and that the sensitivity of corporate tax policies to economic downturn is greater during periods of economic crisis. Similarly, for the United States, Quinn (1989) finds that effective corporate tax rates decline in response to low growth rates and during periods of economic downturn generally. Vogel (1989) reports complementary case study results for the United States that suggest business was able to obtain various concessions from government primarily during the severe downturn in the late 1970s and early 1980s.[7]

Przeworski and Wallerstein (1988) provide the most thorough analysis of the structural dependence thesis to date. However, their central purpose is not to evaluate the thesis as an explanation of the forces that shape state policies but to test the notion that policies that tax the owners of capital to redistribute income must necessarily result in disinvestment. Through a formal analysis, the authors conclude that in the presence of taxes that give preference to investment and penalize consumption out of profits, the thesis is wrong in a static sense: "Any distribution of consumption between wage earners and shareholders is compatible with continual private investment" (p. 24). However, they simultaneously conclude that in a dynamic sense business anticipations of new taxes on their consumption, even with significant investment incentives in place, may well result in disinvestment by capital.[8]

Together, these studies provide very limited information on the accuracy of the structural dependence thesis as a theory of state behavior. As I have mentioned, there is notable inconsistency in their central conclusions about the validity of the thesis. Moreover, with one exception, they are limited to the United States. In addition, Quinn's (1988) comparative study does not directly examine corporate tax responses to variations in capital investment. In sum, the central question of whether or not the state in democratic capitalist political economies systematically responds to low rates of capital investment with investment incentives remains largely unanswered.

Theory, Policy, and Hypotheses

On the basis of the literature and these general theoretical propositions, a number of specific hypotheses about the effect of variations in capital investment on state policy choices can be developed. The logic of the thesis and its interpretation and investigation by scholars suggest that a focus on the responsiveness of corporate tax burdens to business investment is a useful, initial avenue of inquiry for three reasons. First, it effectively indexes both the interplay of corporate tax rates and tax-based investment incentives (e.g., accelerated depreciation allowances, investment tax credits). Indeed, corporate tax rates and tax-based investment incentives have been highlighted by some structural dependence theorists and researchers as constituting two of the principal instruments available to policymakers to bolster low rates of investment. Second, it may assist in answering broader questions posed by the structural dependence thesis. For instance, corporate tax burdens share obvious features of measures of taxes on capital emphasized in studies of redistribution–disinvestment linkages (Przeworski and Wallerstein 1988; Wallerstein 1988). Third, either in terms of rate reductions or investment incentives, it is consistent with both the theory and practice of neo-Keynesian and neoconservative macroeconomic orthodoxy that informed policymakers across my period of investigation.[9]

HYPOTHESIS 1. *Low rates of capital investment will engender reduced taxation of corporate profits.*[10]

Two additional sets of hypotheses stem from theorizing on the determinants of state policy choice in democratic capitalist political economies, from past interpretations and tests of the structural dependence thesis, and from the experiences of democratic capitalist nations in recent years. Each of these departs from a strict understanding of structural dependence that disallows direct or indirect policy impacts of democratic electoral competition and political economic conditions.

Structural Dependence and Party Government. First, extant theory and research on the partisan model of policy choice provide some evidence that the social and economic policies of governments of left-wing and right-wing parties do indeed differ (e.g., Castles 1982; Hibbs 1987a, 1987b; Hicks and Swank 1984a, 1984b; Swank 1988), although both theoretical and empirically based critiques are frequent (Jackman 1986; Pampel and Stryker 1989). Although much of the literature is inconclusive, a number of scholars have hypothesized that left-wing governments may be more likely to favor generally higher levels and rates of taxation on personal income and corporate income in order to promote downward income redistribution to their lower-income constituencies (Cameron 1985; Morrissey and Steinmo 1987; Rose and Karran 1987). Left-wing governments may also be more likely to adopt Keynesian models of the economy that tend to emphasize demand–stimulus responses to low rates of investment and compatibility between high levels of income taxation and redistribution on the one hand and high levels of investment and growth on the other.

HYPOTHESIS 2a. *Corporate tax policies under governments controlled by left-wing parties will be less sensitive to variations in capital investment than such policies under other types of governments.*

HYPOTHESIS 2b. *Corporate tax policies under right-wing governments will be more sensitive to the ebbs and flows of new investment than such policies under other party governments.*

This hypothesis is proposed because of the electoral significance to right-wing parties of business and the owners of capital, because right-wing parties are more likely than others to adopt neoconservative

Table 1

Corporate Income Taxes and Tax Rates in 16 Western Capitalist Democracies

COUNTRY	CORPORATE INCOME TAXATION AS A % OF CORPORATE PROFITS		CORPORATE TAX RATES, 1983 (TOTAL STANDARD RATE)[a]
	1965–69	1980–84	
Australia	17.2	15.9	46.0
Austria	7.9	6.5	60.9/36.9
Belgium	6.8	11.2	45.0
Canada	17.4	13.1	47.0/52.0
Denmark	4.2	7.8	40.0
Finland	8.1	7.6	59.0
France	5.7	10.9	50.0
West Germany	8.7	9.4	63.3/46.7
Ireland	8.2	7.2	50.0
Italy	3.7	6.9	40.5
Netherlands	9.7	11.9	43.0
Norway[b]	5.7	29.2	50.8
Sweden	9.6	9.5	57.4
Switzerland	5.7	8.5	14.2/19.3
United Kingdom[b]	12.3	22.9	52.0
United States	18.5	12.5	50.3
16-nation mean	9.3	11.9	47.1
14-nation mean	—	9.9	—

Sources: Taxation and gross domestic product data are from the OECD's *Revenue Statistics of OECD Nations* (selected years) and *National Accounts of OECD Nations* (selected years). Corporate profit rate data are from Sijbren Cnossen, "Alternative Forms of Corporate Taxation," *Australian Tax Forum* 1 (1984): 253–79.

[a] The double numbers indicate effective rates on retained and distributed profits, respectively.

[b] Note that disproportionately high numbers for Norway and the United Kingdom in the second period are due in large part to North Sea oil revenues. For estimates of the effects of North Sea oil revenues on corporate profits taxes see, e.g., OECD's *Economic Survey: Norway* (selected years).

macroeconomic orthodoxies that emphasize the investment disincentives of taxes, and because right-wing governments generally prefer reductions and low levels of taxes of all sorts.

On the other hand, there is evidence in the experience of Scandinavia (in particular) that actual corporate tax burdens under left-wing governments have been relatively low even in the face of high corporate tax rates. As Schwerin (1984), Esping-Anderson (1985), and others have pointed out, social democratic governments in Sweden, Norway, Finland, and Denmark have granted or maintained ample investment allowances (e.g., special tax-free investment funds) in the taxation of retained earnings. For instance, although Sweden maintained one of the highest corporate tax rates in the early 1980s (57.4% in 1983), overall corporate taxation as a percentage of corporate profits was slightly below the mean for 14 affluent democracies. As illustrated in Table 1, the four Scandinavian political economies (with the exception of Norway in the later period) were at or below the group mean for corporate tax burdens in both the late 1960s and early 1980s despite the fact that Finland, Norway, and Sweden's 1983 corporate tax rates were among the highest.

Contrary to partisan theory, the Scandinavian experience raises the prospect that left-wing governments in general may be sensitive to fluctuations in investment in new productive capacity. One plausible reason for this may be gleaned, in part, from partisan theory itself. Because left-wing governments are hypothesized to pursue redistributive policy goals through significant expansions of social welfare outlays (Hicks and Swank 1984b, 1990; Stephens 1979), high levels of progressive personal income taxation (Cameron 1985), labor market policies, and so on, these governments may be unusually attentive to the investment disincentives of these policies hypothesized by some neoconservative macroeconomic theories.[11] In addition, as I have noted, even the notably strong Keynesian orientation of most of these left-wing governments would allow for the use of investment incentives in response to low rates of capital investment.[12] Finally, within the context of potential growth–equity trade-offs, left-wing governments may forego high tax burdens on reinvested business profits to promote the future welfare of labor. From the perspective of left-wing parties within democratic capitalism, future jobs and income of workers might be promoted by ample current investment incentives and concomitant low corporate tax burdens (see also Przeworski and Wallerstein 1982, 1988).

HYPOTHESIS 2c. *Corporate income taxation under left-wing governments will be more responsive to low capital investment than under other governments.*

Structural Dependence and Economic Crisis. In addition, the logic of the thesis, past studies, and recent experience provide a third set of hypotheses. These

hypotheses revolve around the proposition that government tax policy responses to low rates of capital investment will be more pronounced during periods of economic stagnation. Specifically, the responsiveness of corporate tax policies should be more pronounced during the post-1973 era of stagflation that has plagued democratic capitalist systems. This hypothesis is justified for two reasons. First, sustained periods of slow growth, high unemployment, and price instability should intensify the sensitivity of support-maximizing and revenue-seeking incumbent policymakers to the underpinnings of growth and productivity, particularly capital investment. Second, the post-1973 crisis was accompanied by the ascendance of neoconservative macroeconomic orthodoxies that shifted policymaker attention toward the supply-side determinants of growth, particularly the after-tax rate of return on investment (Bosworth 1984; Przeworski and Wallerstein 1982).

With regard to specific political economic eras, one can suggest that the 1973 OPEC oil price shock serves to demarcate the beginning of the period of ascendance of neoconservative economic orthodoxy, stagnation, and high inflation and interest rates (Bruno and Sachs 1985; Swank 1988). One might also suggest that the second OPEC shock of 1979–80 served to underscore—even intensify—the effects of the first crisis and contribute significantly to the rise of neoconservative ideas and governments (Tsai 1989). On the other hand, the widespread restrictive tilt of macroeconomic policy combined with (automatic) prodeficit pressures of continued stagnation among the affluent capitalist democracies may have raised barriers to systematic cuts in corporate tax burdens in the post-1979 era.

HYPOTHESIS 3a. *Corporate tax policy was more responsive to investment in the post-1973 period than before.*

HYPOTHESIS 3b. *Corporate tax policy was more responsive to trends in capital investment during OPEC 1 than in other years.*

Alternatively, HYPOTHESIS 3c. *Policy was more responsive during OPEC 2 than in other years.*

THE FRAMEWORK FOR ANALYSIS

Quantitative analyses of my central questions are conducted within the framework of a pooled time series design. This choice was made largely because cross-nationally and temporally comparable quantitative indicators for all of the hypothesized explanatory factors are only available for a relatively short time interval (the early 1960s to the mid-1980s) and for a relatively small group of capitalist democracies. Thus, from a conventional perspective on cross-national empirical analyses, the choices were between individual country analyses on approximately 20 data points or cross-sectional analysis of 16–18 data points. Both of these options were rejected in favor of an analysis of pooled cross-sections of time series. This procedure maximizes the available sample size and allows more accurate specifications of the processes determining state policy choices. The actual sample was limited to time series of 20 annual observations from 16 nations.[13]

Model Specification

The generalized model for testing the impacts of movements in business investment on corporate tax burdens may be represented as follows:

$$\text{POLICY}_{it} = B_{1i} + \sum_{k=2}^{k} B_k X_{kit} + e_{it},$$

where i and t are observations on units (nations) and time points (years), respectively, B_{1i} is an equation intercept or intercepts, B_K is a vector of coefficients for the matrix of observations on k exogenous variables (X_K), and e is the error term. Within the X matrix, observations on a variable indexing the movements of business investment in productive capacity and four sets of variables that represent alternative views of the determination of tax policy choices and essential controls are included.

The first set of variables controls for two important potential determinants of corporate tax burdens: the volume of profits itself and the automatic tax effects of investment. With regard to profits, some automatic responsiveness of corporate taxes to variations in the volume of profits is built into tax policy.[14] In addition, policymakers may have made conscious tax policy choices in response to low profits, given the role of profits in generating investment in neo-Keynesian and neoconservative models of investment. Generally, a finding of policy responsiveness to profits might indicate further, indirect support for structural dependence propositions about government sensitivity to investment. Concerning automatic effects of investment on corporate tax burdens, it is important to keep in mind that increases in investment will automatically produce declines in tax burdens (and decreases will produce rises), net of other factors, because of the near-universal use of accelerated depreciation and investment tax credits among our focal countries during the time frame of the present study. Thus, a separate control for this automatic relationship will be used.[15]

With respect to sets of alternative explanatory factors, empirical models incorporate controls for the hypothesized effects of two central aspects of democratic politics. Specifically, following the preceding discussion of left-wing and right-wing party policy preferences, the model allows for the possibility of direct effects of alternations in party government on corporate tax burdens. The model also allows for the prospect that centrist parties (i.e., secular and Christian Democratic centrist parties) are more likely to prefer higher levels of taxation generally than parties of the Right (Cameron 1985). To make actual statistical estimation tractable, the model is initially speci-

fied with measures of left-wing and centrist party control of government, although extended analyses incorporate right-wing party variables. In addition, following some studies on tax policy formation (e.g., Rose and Karran 1987), a control for the impact of the electoral cycle on corporate tax policies will be employed. As proponents of this familiar thesis assert, vote-maximizing, incumbent politicians seeking reelection may seek to decrease unemployment and increase personal income via transfer, tax, and macroeconomic policies as elections grow near (Nordhaus 1975; Tufte 1978).

The third basic set of factors involves controls for variations in macroeconomic performance—economic growth and inflation rates. Generally, these factors account for the roles of the operation of automatic macroeconomic stabilizers embedded in tax policy, as well as the roles of discretionary responses of policymakers to variations in economic performance. Given that investment in new productive capacity is highly correlated with the business cycle (i.e., gross domestic product growth rates), they may also serve to isolate investment–tax policy relationships that are not part of larger countercyclical processes.

Finally, the model incorporates the possibility that international economic forces have significant, direct consequences for tax policies. First, the model accounts for economic openness of the nation at a particular point in time. As Cameron (1978), Katzenstein (1985), and others have argued, high levels of exposure to international competition in goods and services and of flows in labor and capital create pressures for various forms of state intervention to facilitate competitiveness and to compensate groups for adjustments to international competition. In the long term, international economic openness may also contribute to the strength of political organizations (i.e., left-wing parties and union movements) that favor a large state. Together, these forces may create pressures for generally higher levels of taxation, social spending, and state intervention in the economy, as Cameron's (1978) well-known study indicates. On the other hand, openness to the international economy may promote low levels of taxation on business as the state attempts to cultivate international competitiveness, attract new capital investment, and prevent capital flight.

The model also accounts for the possibility of direct effects of energy price shocks associated with actions of OPEC in 1973–74 and 1979–80. As a number of analyses have recently suggested, the OPEC price shocks had important impacts on spending and taxing policy (e.g., Bruno and Sachs 1985; Tsai 1989). These impacts have been direct (through unemployment and price-level effects) and indirect (through shifts in political and intellectual climates and through structural economic changes). Generally, the budgetary impact of these consequences has been to increase spending and deplete revenues through discretionary and automatic policy responses (Chouraqui and Montador 1984), although the post-1973 experience of the OECD nations also suggests that the inflationary aspect of the shocks frequently led to pressures for discretionary fiscal contraction in the form of spending cuts and tax increases, particularly in the post-1979 period (Tsai 1989).

Measurement

The principal indicators for corporate income taxation and the explanatory factors, as well as sources of all data upon which these measures are based, are given in the Appendix. Construction of the empirical indicator for corporate income taxation is relatively straightforward. To adjust for population change, affluence, and profitability and to obviate the problems of heteroscedastic errors typically generated by models where levels of endogenous variables differ notably, total corporate income taxes were expressed as percentages of net operating income (TAXES ON PROFITS).

The operationalization of business investment (real outlays for machinery and equipment) does require slightly more discussion. Ideally, one would employ an indicator of private sector investment in new plant and equipment. However, national account statistics (the principal source of such information) do not provide cross-nationally comparable measures of private sector plant and equipment outlays for the time frame of the present study. Specifically, OECD and UN national account statistics only provide such data for seven nations since the early 1970s. Some alternatives consist of aggregate gross fixed capital formation (GFCF), GFCF net of residential construction, and GFCF net of general government capital outlays. Unfortunately, all these options leave substantial components of direct government and/or aggregate public sector (general government and public enterprise) investment in the resulting indicator.

One category of capital outlays available for most OECD nations since the early 1960s is relatively largely composed of private sector outlays for new productive capacity, namely, investment in machinery and equipment. For those nations for which purely private sector plant and equipment data are available, 86.4% of total investment in machinery and equipment was made by private sector capital.[16] Moreover, total machinery and equipment outlays covary almost perfectly across time with total private sector nonresidential structure, machinery, and equipment outlays. As a result of these considerations, I shall measure private business investment by total machinery and equipment outlays. Consistent with the theory's central focus on changes in productive investment (e.g., disinvestment), as opposed to particular levels of investment, the actual indicator is expressed as the percentage rate of change in real machinery and equipment outlays (%CH INVESTMENT).

To control for the automatic, negative relationship between investment and corporate profits taxation, the level of investment in the preceding year is used. Specifically, this factor is operationalized as total

machinery and equipment outlays expressed as a percentage of gross domestic product (LEV INVESTMENT).

The operationalizations of profits and macroeconomic performance variables are relatively straightforward. In the absence of more detailed, cross-nationally comparable indicators, profits are defined here as net operating income of business. To control for changes in population, economic size, and affluence over time, net operating income is expressed as a percentage of national income (PROFIT/INCOME). Operationalization of both economic growth and inflation follow convention: growth is measured as annual percentage changes in real gross domestic product (GROWTH), while inflation is measured as annual percentage changes in the consumer price index (INFLATION).

Operationalizations of openness and the OPEC oil price shocks are also relatively straightforward. Although one might advance more sophisticated indicators of exposure and vulnerability to the upheavals of international labor, capital, and goods markets, I use the conventional indicator utilized by Cameron (1978) and others to measure domestic political economic exposure to international market forces, namely, import and export flows as percentages of GDP (OPENNESS). The actual indicators for the OPEC oil shocks ultimately consist of dichotomous variables (OPEC1 $\text{DUMMY}_{1974\text{–}79}$ and OPEC2 $\text{DUMMY}_{1980\text{–}84}$) designed to capture the effect of the price shocks and their political and economic consequences on policies in the period after the particular economic shock. These indicators were selected because of high collinearity between more specific measures of oil import costs and macroeconomic performance variables.

With respect to the political factors, a dichotomous variable coded 1.00 for an election year and .00 otherwise (ELECTION) was used to measure the potential policy effects of electoral cycle forces. Consistent with past work on the impacts of partisan control of government (Cameron 1978; Hicks and Swank 1990; Swank 1988), political party control of government was operationalized by computing the annual averages of the proportion of cabinet portfolios held by left-wing (LEFT CABINET), centrist (CENTER CABINET), and right-wing (RIGHT CABINET) parties as defined in Hicks and Swank (1990).

Concerning specification of time lags and leads appropriate to evaluating the focal hypotheses, all variables were lagged one period (or led in the case of election years) except OPEC1 and OPEC2. This was done to obviate simultaneity bias potentially present in some of the coefficients and to allow for both automatic and discretionary processes to begin to exert themselves on policy.[17] With regard to OPEC1 and OPEC2, since price effects and their short-term consequences may appear rather quickly, since I am attempting to measure period effects, since the year specifications (1974–79 and 1980–84) already involve some lag from the oil shocks, and since no compelling reason exists for a specific alternative, I made a contemporaneous specification.

In light of these measurement stipulations and earlier theoretical discussion, the following baseline empirical model may be offered:

$$\text{TAXES ON PROFITS}_{it} = B_{1i} + \sum_{k=2}^{k} B_k X_{kit} + e_{it},$$

where X consists of $\%\text{CH INVESTMENT}_{t-2 \text{ to } t-1}$, $\text{PROFITS/INCOME}_{t-1}$, $\text{LEV INVESTMENT}_{t-1}$, $\text{LEFT CABINET}_{t-1}$, $\text{CENTER CABINET}_{t-1}$, ELECTION_{t+1}, $\text{GROWTH}_{t-2 \text{ to } t-1}$, $\text{INFLATION}_{t-2 \text{ to } t-1}$, OPENNESS_{t-1}, OPEC1 $\text{DUMMY}_{1974\text{–}79}$, and OPEC2 $\text{DUMMY}_{1980\text{–}84}$.[18] This model, as well as variants of it incorporating nonlinear or interactive effects, will be used to evaluate hypotheses 1–3c.

Estimation

The pooling of time series (t = 20) from a number of cross-sectional units (n = 16) presents four formidable estimation difficulties unless the data are unusually well behaved. First, troublesome autocorrelation of errors may be present within cross sections. Second, errors may be heteroscedastic across individual units, groups of cross sections, or blocks of time. Third, errors may be correlated across cross sections at the same point in time. Fourth, the ordinary least squares (OLS) intercepts for the individual cross sections may be notably different from one another. The sum of these problems, if present, constitutes serious violations of OLS assumptions about error structure and will generally produce biased OLS coefficient and standard error estimates (Johnston 1984, chap. 10; Kmenta 1971, chap. 12; Stimson 1985). In the current analyses, the presence of all these problems is possible: the temporally dominant nature of the model (T > N) suggests concern for autoregressive errors, while the nature of the units and endogenous variables indicates potential heteroscedasticity. Moreover, cross-sectionally correlated errors are likely when pooling time series of data from geographic entities (Kmenta 1971, 512). Nation-specific intercepts for tax equations are also likely given the wide variability of political and economic structures that cannot be captured in our models.

To obviate the risks of these potential problems, my models and extensions were estimated using Kmenta's generalized least squares procedure (1971, equations 12.22–40). This method corrects for cross-sectional heteroscedasticity, contemporaneous cross-sectional correlation of errors, and first-order autoregressive errors.[19] To account for the remaining possibility of intercept differences across units, dummy variables (N − 1) were added to the equations. The presence of these variables is likely to bias downward the coefficients of variables whose effects are partially cross-sectional (Judge et al. 1985, chap. 13). The potential bias in all estimates when significant unit effects are present and ignored suggests their inclusion, particularly in the absence of more theoretically informed cross-sectional controls (Stimson 1985). Finally, to maximize the probability of

correctly adjusting estimates for the autoregressive error component, Kmenta's Full Cross-Sectionally Correlated and Autoregressive Model was estimated with individual-nation estimates of first-order autocorrelation (Kmenta 1971, equation 2, 12.25–29). Finally, in order to estimate the conditional effects of business investment on corporate tax policies, interaction analysis was employed. That is, for all focal hypotheses that suggest that effects of business investment on corporate tax burdens vary by the level of another variable, interactions and conditional coefficients and associated significance tests were computed.[20]

FINDINGS

The results of estimation of the baseline corporate income tax model are presented in the first set of columns of Table 2. For all models, the upper portion of the table reports generalized least squares estimates of the parameters for the focal causal factors while the middle portion reports nation-specific intercepts. The reader should recall that these parameters index unit effects relative to the left-out unit (the United States), net of the effects of other exogenous variables. As such, they have little theoretical meaning and are presented for the information of the interested reader. The lower portion of the table reports goodness of fit and residual statistics.

As the first column of Table 2 reveals, support for the principal proposition derived from the structural dependence thesis is generated from the estimation of our baseline model: as the rate of new investment declines corporate tax burdens decline (and as the rate increases, so do the burdens). Moreover, this significant parameter estimate ($b = .0182$, $t = 3.96$) relating the rate of investment in new productive capacity and corporate tax burdens emerges in the presence of controls for alternative political and economic determinants of corporate income taxation and for the automatic effects of investment on corporate tax burdens: the relationship between past levels of investment and corporate tax burdens is negative and significant, as expected. Profits and corporate income taxation are also significantly associated, as anticipated: low profitability is associated with low corporate tax burdens, and high profitability with high burdens. While it is impossible to segregate automatic and discretionary policy effects of variations in profits, the arguably limited automatic linkages of profits and taxation and the role of profits as a determinant of investment in economic theory suggest that an investment-motivated discretionary response to profitability is a possibility. As such, a discretionary profits effect would underscore structural dependence propositions about the sensitivity of policymakers to investment and its determinants.

With respect to the direct effects of political forces, left-wing and centrist party control are both negatively and significantly related to corporate income taxes. While this finding supports the view gleaned from the Scandinavian experience about left-wing parties and corporate tax policy, it also extends it to Centrist parties: net of other forces, centrist parties such as the centrist Christian Democratic parties of the Benelux nations also maintain lower corporate tax burdens. Although correctly signed, the parameter estimate for the election year variable is completely insignificant.

Table 2 also reports findings for effects of macroeconomic performance and international economic factors. In the case of growth and inflation, findings are consistent with theory on countercyclical movements of taxes: corporate tax burdens are positively and significantly related to rates of change in real gross domestic product and in price levels. With respect to international economic factors, findings for openness indicate that exposure to international markets, net of other forces, results in pressures for low corporate taxes. On the other hand, the direct effects of the OPEC oil shocks are positive: net of other determinants of corporate tax policy, corporate tax burdens were higher in both OPEC 1 ($b = 1.8990$) and OPEC 2 ($b = 3.2087$). These findings may be interpreted as reflecting the overall upward movement in taxes in the last several decades, as well as pressures on taxes generated by restrictive fiscal policy and stagnation-induced budget deficits, particularly in the latter period.

Overall, results from the baseline model provide clear support for the notion that low rates of investment in new productive capacity systematically give rise to reductions in corporate tax burdens. However, two qualifications of this initial conclusion must be made. First, the magnitude of policy response to investment is arguably not large. For instance, a decline from 10% to 0% in the rate of new investment is associated with a .2 reduction of corporate income taxes as a percentage of net operating income. Given that the average of corporate taxes as a percentage of net operating income is roughly 10% across our focal period (see Table 1), this relative change is not great. Secondly, results reported in Table 2 suggest that a number of other political and economic forces exert substantial influence in corporate tax policymaking. For instance, the advent of a left-wing government (LEFT CABINET from .00 to 100) would have a larger impact on corporate taxes than a 10% fall in the rate of new investment. In sum, particularly strong inferences from structural dependence theory about, for example, the overriding role of investment and the trivial import of democratic elections could not be made on the basis of our otherwise clear evidence for the thesis.[21]

Results for tests of the hypotheses proposing that policy responsiveness to investment will be conditioned by the partisan composition of government are presented in the second and third pairs of columns of Table 2. These columns provide information on the interaction itself and the conditional coefficients for the policy impact of investment at party government levels of 40% and 100% of cabinet portfolios. As the second pair reveals, the interaction between invest-

Table 2

Capital Investment, Politics, and Corporate Income Taxation, 1965–84: Direct and Partisan-mediated Investment Impacts on Corporate Profits Taxes in 16 Nations

VARIABLE	BASELINE MODEL UNSTAND. GLS EST.	BASELINE MODEL SE	INVESTMENT AND TAXES UNDER RIGHT-WING GOVERNMENTS UNSTAND. GLS EST.	INVESTMENT AND TAXES UNDER RIGHT-WING GOVERNMENTS SE	INVESTMENT AND TAXES UNDER LEFT-WING GOVERNMENTS UNSTAND. GLS EST.	INVESTMENT AND TAXES UNDER LEFT-WING GOVERNMENTS SE
%CH INVESTMENT$_{t-2\ to\ t-1}$	.0182*	.0046	.0138*	.0050	.0063	.0054
PROFIT/INCOME$_{t-1}$	.0088*	.0023	.0847*	.0023	.0764*	.0232
LEV INVESTMENT$_{t-1}$	−.1482*	.0527	−.1553*	.0484	−.2349*	.0526
RIGHT CABINET$_{t-1}$	—	—	.0044*	.0020	—	—
RIGHT * %CH INVEST	—	—	.0002*	.0001	—	—
%CH INVEST at						
RIGHT$_{40}$	—	—	.0216*	.0047	—	—
RIGHT$_{100}$	—	—	.0334*	.0091	—	—
LEFT CABINET$_{t-1}$	−.0060*	.0019	—	—	−.0064*	.0019
LEFT * %CH INVEST	—	—	—	—	.0004*	.0001
%CH INVEST at						
LEFT$_{40}$	—	—	—	—	.0245*	.0044
LEFT$_{100}$	—	—	—	—	.0518*	.0068
CENTER CABINET$_{t-1}$	−.0066*	.0026	—	—	−.0054*	.0025
ELECTION$_{t+1}$	−.0095	.0463	−.0292	.0430	−.0502	.0478
GROWTH$_{t-2\ to\ t-1}$	.0440*	.0137	.0494*	.0127	.0483*	.0138
INFLATION$_{t-2\ to\ 1-1}$	.0735*	.0176	.0814*	.0175	.0880*	.0172
OPENNESS$_{T-1}$	−.0456*	.0086	−.0444*	.0084	−.0452*	.0082
OPEC1 DUMMY$_{1974-79}$	1.8990*	.1850	1.7957*	.1812	1.8207*	.1814
OPEC2 DUMMY$_{1980-84}$	3.2087*	.2362	3.1691*	.2280	3.0647*	.2296
AUSTRALIA	1.0525	1.9863	.0944	1.9303	1.1980	11.9866
AUSTRIA	−5.4872*	1.8784	−5.6604*	1.7767	−5.3859*	1.8844
BELGIUM	−1.2807	2.1718	−1.6246	2.0863	−1.4956	2.1422
CANADA	.6152	2.1541	.6984	2.0653	.2090	2.1394
DENMARK	−7.2370*	1.9827	−7.4554*	1.8929	−7.4697*	1.9856
FINLAND	−5.6503*	1.9397	−5.8613*	1.8577	−5.7197*	1.9541
FRANCE	−5.8573*	2.0791	−5.9983*	2.0011	−5.7630*	2.0562
W. GERMANY	−4.6478*	1.8154	−4.8042*	1.7370	−4.6605*	1.8234
IRELAND	−4.3929*	1.9896	−4.5623*	1.9457	−4.2732*	1.9710
ITALY	−11.0490*	2.0899	−11.2740*	2.0059	−11.0550*	2.0894
NETHERLANDS	−.0779	2.0224	−.3508	1.9228	−.1226	2.0260
NORWAY	4.2550	3.8603	3.3989	3.7459	4.8384	3.5753
SWEDEN	−2.8630	1.8674	−3.1361*	1.7862	−3.1816*	1.8806
SWITZERLAND	−4.9155*	2.0294	−5.1480*	1.9326	−4.9451*	2.0361
U. K.	3.5529	2.5355	3.3573	2.5027	3.2473	2.5194
Constant	12.9230*	2.0175	12.5960*	1.9169	13.9830*	2.0349
Degrees of freedom	293		293		292	
Buse R^2	.8108		.8431		.8510	
Durbin–Watson	1.9633		1.9702		1.9498	
Rho	.0134		.0114		.0190	

Note: These pooled time-series estimates are obtained by estimating Kmenta's Full Cross-sectionally Correlated and Autoregressive (AR1) Model with unit effects added (Kmenta 1971, equations 12.33–40).
*$p \leq .05$.

ment and right-wing party government control is significant: as right-wing party control increases, the effect of investment on corporate taxation increases. This finding is placed in concrete relief by the two conditional coefficients. At the level of 40% right-wing party cabinet portfolios, the coefficient relating investment to taxation is .0216, while at the 100% level, it is .0334.

The third pair of columns of Table 2 reports the results for the left-wing party–investment interaction. As suggested by my alternative hypothesis (2c) for the mediating effect of left-wing parties, left-wing parties appear to be notably sensitive to trends in investment. The interaction is positive and significant, indicating that as left-wing party government control increases, the policy effects of investment are

Table 3

Capital Investment, Economic Crisis, and Corporate Income Taxation, 1965–84: Investment Impacts on Corporate Profits Taxes during Economic Crisis in 16 Nations

	INVESTMENT AND TAXES					
	DURING OPEC		DURING OPEC1		DURING OPEC2	
VARIABLES	GLS EST.	SE	GLS EST.	SE	GLS EST.	SE
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	.0201*	.0068	.0131*	.0050	.0185*	.0054
PROFIT/INCOME$_{t-1}$	.0767*	.0243	.0847*	.0242	.0749*	.0246
LEV INVESTMENT$_{t-1}$	−.2215*	.0563	−.1611*	.0529	−.1525*	.0567
LEFT CABINET$_{t-1}$	−.0072*	.0017	−.0059*	.0019	−.0064*	.0019
CENTER CABINET$_{t-1}$	−.0088*	.0025	−.0071*	.0026	−.0083*	.0028
ELECTION$_{t+1}$	−.0143	.0517	−.0179	.0451	−.0474	.0495
GROWTH$_{t-2 \text{ to } t-1}$	.0213	.0150	.0419*	.0139	.0496*	.0144
INFLATION$_{t-2 \text{ to } t-1}$	.0574*	.0188	.0756*	.0180	.0751*	.0183
OPENNESS$_{t-1}$	−.0098	.0078	−.0451*	.0085	−.0477*	.0090
OPEC DUMMY$_{1974-84}$	1.8464*	.2107	—	—	—	—
OPEC * %CH INVEST	.0048	.0078	—	—	—	—
opec1 DUMMY$_{1974-79}$	—	—	1.8155*	.1929	1.8893*	.1938
OPEC1 * %CH INVEST	—	—	.0185*	.0071	—	—
%CH INVEST in OPEC1	—	—	.0316*	.0066	—	—
OPEC2 DUMMY$_{1980-84}$	—	—	3.1256*	.2411	3.1694*	.2470
OPEC2 * %CH INVEST	—	—	—	—	−.0102	.0089
AUSTRALIA	.3908	1.8707	1.1234	2.0178	1.0979	2.0043
AUSTRIA	−7.1037*	1.7370	−5.3862*	1.9001	−5.2741*	1.8923
BELGIUM	−4.5414*	1.9980	−1.2077	2.1876	−.9050	2.2094
CANADA	−.6259	1.9493	.3569	2.0794	.8194	2.1223
DENMARK	−8.9999*	1.8730	−7.1973*	2.0064	−7.0542*	2.0054
FINLAND	−6.8459*	1.8009	−5.5325*	1.9566	−5.4071*	1.9549
FRANCE	−6.5831*	1.9900	−5.7273*	2.0991	−5.7559*	2.1117
W. GERMANY	−5.8207*	1.6622	−4.5757*	1.8363	−4.4873*	1.8331
IRELAND	−7.1176*	1.8507	−4.3539*	2.0075	−4.1954*	2.0178
ITALY	−11.5380*	2.0325	−10.8800*	2.1319	−10.5000*	2.0914
NETHERLANDS	−2.9405	1.8625	−.0366	2.0401	.2229	2.0581
NORWAY	1.2137	3.7500	4.3866	3.8502	4.3851	3.9570
SWEDEN	−4.2709*	1.7375	−2.8543	1.8932	−2.7561	1.8834
SWITZERLAND	−6.6321*	1.8822	−4.8203*	2.0515	−4.6815*	2.0378
U. K.	1.9714	2.4574	3.5954	2.5630	3.5107	2.5466
Constant	14.0090*	1.8515	13.0870*	2.0338	13.270*	2.0398
Degrees of freedom	293		292		292	
Buse R^2	.7760		.8104		.8097	
Durbin–Watson	1.9730		1.9620		1.9401	
Rho	.0128		.0139		.0238	

Note: These pooled time-series estimates are obtained by estimating Kmenta's Full Cross-sectionally Correlated and Autoregressive (AR1) Model with units effects added (Kmenta 1971, equations 12.33–40).
*$p \leq .05$

magnified. The conditional coefficients actually indicate that the effect of investment in new productive capacity on corporate tax burdens is higher during a left-wing government that controls all cabinet portfolios (b = .0518) than for a comparable right-wing government (b = .0334).[22] As such, this finding, combined with evidence on the direct effects of left-wing parties from the baseline model, adds weight to the propositions that left-wing governments (for the reasons I have discussed) are likely to maintain low corporate tax burdens and be particularly sensitive to trends in new capital investment when formulating corporate tax policy.

Table 3 reports the results of tests of the hypotheses predicting that corporate tax policy responsiveness to rates of new investment will be more pronounced during periods of economic crisis. As reported in the first pair of columns, the interaction between the before-or-after OPEC 1 variable (OPEC DUMMY$_{1974-84}$) and capital investment, while correctly signed, is not significant. This finding indicates no overall difference in the policy impacts of capital investment across the two periods. However, interactions between OPEC subperiods and the capital investment variable indicate that we should not reject the economic crisis hypothesis too quickly. As the second column pair of Table 3 reveals, there is a significant interaction between OPEC1 DUMMY$_{1974-79}$

and capital investment; that is, policy responsiveness is greater in the years 1974–79 than in other periods. However, the interaction between OPEC2 $\text{DUMMY}_{1980-84}$ and capital investment is insignificant, indicating that policy responsiveness to investment in the years 1979–84 did not differ significantly from other periods.

Together, these findings suggest that the effects of political economic changes occurring in the wake of the first OPEC oil shock intensified policy sensitivity to capital investment but that this heightened responsiveness was not sustained after 1979. Generally, this pattern of findings bolsters support for the proposition that the restrictive turn in macroeconomic policy among most capitalist democracies after the late 1970s, together with the burgeoning budget deficits in many nations, set limits to significant reductions in tax burdens during these years, net of other forces.[23]

Capital Investment, Politics, and Personal Income Taxation

A good deal of the debate that has emerged since the late 1960s over the disincentive effects of taxation on growth has hinged on discussions of the impact of high levels and rates of personal income taxation on saving incentives and investment (e.g., Bosworth 1984; Feldstein 1987). In addition, even in the era of pronounced neo-Keynesian orientation of policymakers, cuts in personal income taxes to bolster aggregate demand in response to lagging rates of investment would be fully sanctioned. Thus, to expand the empirical investigation and to provide a benchmark for judging the generalizability of the findings in my analysis of corporate taxes, I shall explore hypotheses directly analogous to those generated for corporate income taxes in the case of personal income taxes.[24]

The focus on personal income taxes will, moreover, enable us more fully to explore one of the most novel findings in the preceding analyses of structural dependence propositions—left-wing parties are particularly sensitive to trends in investment in new productive capacity. As I have noted, one explanation for this relationship involves, in part, the notion that left-wing parties maintain high levels of progressive personal income taxes (and social welfare effort) and thus are particularly sensitive to the perceived need to bolster investment through corporate tax-based incentives. This explanation might be strengthened by examining the impact of left-wing governments on personal income taxes within the same framework. Although evidence of partisan effects on welfare effort and economic policies and outcomes is substantial, theoretical and empirical controversies remain. In addition, evidence of systematic partisan effects on income taxation is mixed; few studies have focused systematically on partisan effects on personal income taxes across a large number of countries.

In order to explore these questions, models directly analogous to those already estimated were formulated for personal income taxes. Other than a different dependent variable (TAXES ON INCOME-personal income taxes as a percentage of national income), there are only two minor changes in the baseline model. First, rather than a control for profits, the model includes a control for per capita real national income (REAL INCOME/POP_{t-1}), a major determinant of cyclical variation in personal income taxes, and a source of support for high tax burdens (Cameron 1978). Second, the control for the level of investment is dropped. Otherwise, to facilitate comparison to the corporate tax models, all specifications, measurements, and estimation procedures are identical to those for corporate tax equations.

The results for the estimation of the baseline model are presented in the first pair of columns in Table 4. As the table reveals, there is no direct effect of the rate of investment on personal income taxes across our entire period. While correctly signed (b = .0002), the coefficient is tiny in magnitude and completely insignificant. The results reported from the estimation of the baseline model are, however, more supportive of our expectations about the effects of left-wing parties: left-wing party government control is positively and significantly correlated with personal income taxes, net of other forces shaping personal income tax policy. Concretely, the advent of a left-wing government (.00–100% of cabinet portfolios) would increase personal income taxes by .5% of national income. Overall, these findings add weight to the proposition that left-wing parties maintain low corporate taxes and display relatively substantial sensitivity to low rates of investment in the form of adaptive corporate tax policies as part of a coherent policy mix. This mix of policies is ostensibly oriented to producing more income equality while simultaneously generating adequate investment in new productive capacity.[25]

With regard to the control variables in this baseline estimation, policy effects of other exogenous variables are, for the most part, consistent with theory. Real per capita income (REAL INCOME/POP_{t-1}), centrist party government control (CENTER CABINET_{t-1}), economic openness (OPEN_{t-1}), and the OPEC oil shocks (OPEC1 and OPEC2 dummies) are all positively and significantly associated with personal income taxation. Inflation is also positively and significantly associated with personal income taxes, indicating that inflation resulted in bracket-creep increases in these taxes and perhaps led to more restrictive fiscal policies. The only unanticipated findings pertain to election years (ELECTION_{t+1}) and economic growth rates ($\text{GROWTH}_{t-2 \text{ to } t-1}$): the election years variable is completely insignificant, while the growth variable is, somewhat anomalously, negative and significant. This latter finding (that low rates of growth are associated with high personal income taxes and high rates with low taxes) is probably an artifact of the control for real income. The two variables are highly correlated temporally within countries, and variations in per capita real income probably tap the countercyclical and discretionary growth responses of personal income taxes better than growth rates in gross domestic product.

Table 4

Capital Investment, Economic Crisis, and Personal Income Taxation in 16 Nations, 1965–84: Investment Impacts on Personal Income Taxes (Baseline and Economic Crisis Models)

			INVESTMENT AND TAXES			
	BASELINE MODEL		DURING OPEC1		DURING OPEC2	
VARIABLE	GLS EST.	SE	GLS EST.	SE	GLS EST.	SE
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	.0002	.0019	−.0034	.0023	.0003	.0021
REAL INCOME/POP$_{t-1}$	.1544*	.0197	.1546*	.0190	.1550*	.0195
LEFT CABINET$_{t-1}$	.0050*	.0008	.0052*	.0008	.0052*	.0008
CENTER CABINET$_{t-1}$	.0036*	.0007	.0035*	.0007	.0036*	.0007
ELECTION$_{t+1}$	.0185	.0226	.0282	.0214	.0167	.0231
GROWTH$_{t-2 \text{ to } t-1}$	−.0264*	.0058	−.0285*	.0055	−.0268*	.0058
INFLATION$_{t-2 \text{ to } t-1}$	.0039*	.0064	.0341*	.0063	.0340*	.0064
OPENNESS$_{t-1}$	.0149*	.0054	.0487*	.0051	.0493*	.0054
OPEC1 DUMMY$_{1974–79}$	1.0550*	.1414	1.0077*	.1401	1.0589*	.1410
OPEC1 * %CH INVEST	—	—	.0087*	.0033	—	—
%CH INVEST in OPEC1	—	—	.0053*	.0028	—	—
OPEC2 DUMMY$_{1980–84}$	1.1358*	.1991	1.1526*	.1977	1.1613*	.1993
OPEC2 * %CH INVEST	—	—	—	—	.0003	.0021
AUSTRALIA	1.4561*	.7481	1.4848*	.7130	1.4543*	.7312
AUSTRIA	−3.2164*	.4231	−3.2580*	.4062	−3.2330*	.4234
BELGIUM	−1.8354	1.0797	−1.7014	1.0786	−1.8241*	1.0749
CANADA	−.9953*	.4062	−.9216*	.3690	−.9798*	.3970
DENMARK	9.1723*	2.3532	9.3357*	2.3505	9.2095*	2.3459
FINLAND	4.5962*	.8115	4.6666*	.8038	4.6159*	.8103
FRANCE	−6.2023*	.4959	−6.2568*	.4853	−6.2216*	.4978
W. GERMANY	−.7786*	.3684	−.8315*	.3725	−.7893*	.3712
IRELAND	−4.8088*	.8383	−4.7196*	.8222	−4.8027*	.8346
ITALY	−5.8701*	.7432	−5.8502*	.7275	−5.8626*	.7430
NETHERLANDS	−2.9299*	.9783	−2.9753*	.9406	−2.9441*	.9748
NORWAY	1.2753	1.5288	1.0068	1.5168	1.1862	1.5435
SWEDEN	8.7008*	.3288	8.6857*	.3169	8.6986*	.3282
SWITZERLAND	−3.1609*	.4595	−3.1502*	.4504	−3.1609*	.4589
U. K.	−.4143	.3266	−.3393	.5455	−.3986*	.5638
Constant	7.7314*	.3266	7.7845*	.31168	7.7253*	.3280
Degrees of freedom	294		293		293	
Buse R^2	.9834		.9868		.9846	
Durbin—Watson	1.9730		1.9619		1.9757	
Rho	.0128		.0181		.0114	

Note: These pooled time-series estimates are obtained by estimating Kmenta's Full Cross-sectionally Correlated and Autoregressive (AR1) Model with unit effects added (Kmenta 1971, equations 12.33–40).
*$p \leq .05$.

Moving to the second and third pairs of columns of Table 4, results address the prospect that personal income tax policy may have been more responsive to low rates of investment under specific political economic conditions. To conserve space, only results for the specific post-1973 subperiods are presented. Interactions for capital investment on the one hand, and the entire post-1973 period, as well as left- and right-wing party government control on the other, are all insignificant.

However, as the table reveals, a significant effect of investment on personal income taxes is uncovered for the years 1974–79 (see %CH INVESTMENT in OPEC1). Indeed, this effect parallels the one for corporate income taxes, although (unlike the corporate income tax case) the policy effect of investment is only significant during the years of economic downturn immediately following the first OPEC oil shock. And, similar to my interpretation in the corporate income tax models, one might suggest that while recession and the advent of stagflation intensified policymaker sensitivity to the underpinnings of growth in the post-1973 era, the restrictive orientation of macroeconomic policy and burgeoning budget deficits after the second OPEC shock undercut the ideological and material bases for large tax reductions in response to lagging investment. Generally, the impact of investment in the 1974–79 period seems fairly small: a drop from 10% to 0% in the rate of capital investment results in a decline in personal income taxes of .05% of national income.

In sum, my brief exploration of the determinants of personal income tax policy has added support to the structural dependence thesis in the sense that during

economic crisis, policymakers respond to low capital investment with reductions in personal income taxes, net of other political and economic forces shaping these tax policies. However, outside the years of the first OPEC shock, policymakers did not alter overall personal income tax burdens in response to inadequate investment. My inquiry into the determinants of personal income taxes has also shed some light on the behavior of left-wing governments: left-wing governments do indeed have positive effects on the level of personal income taxation. Together with findings from my corporate tax equations and discussion of patterns of taxation in particular countries, this result suggests that the notion that left-wing governments in capitalist democracies simultaneously pursue redistributive goals through personal income taxes (and welfare effort) and investment goals through low corporate taxes is plausible.

CONCLUSIONS

Overall, the results indicate that the generalized version of the structural dependence thesis as a theory of state behavior has merit. From my analyses, one might conclude that the state in capitalist democracies does regularly offer investment incentives and, under certain conditions, removes perceived investment disincentives in the face of inadequate capital investment. Specifically, low rates of capital investment result in reductions in corporate tax burdens, and this policy responsiveness to investment trends is more pronounced during periods of economic crisis. In addition, during periods of economic stress, low rates of investment in new productive capacity are associated with reductions in personal income taxes. In sum, these findings provide a fairly solid empirical base for a component of the structural dependence thesis that has received remarkably little empirical attention.

However, the preceding analysis also leads to qualification of a more simplistic reading of the structural dependence thesis. First, the analysis indicates that the magnitudes of the policy changes in response to low rates of capital investment are relatively small. Furthermore, my analysis of the broader political and economic forces shaping tax policy choices suggest that facets of democratic politics, features of domestic economic performance not recognized or stressed in structural dependence theory, and international economic pressures also influence taxation. Indeed, the analysis has strengthened the case for those who argue that the distinctive policy preferences of parties and patterns of party control of government are important influences on state policy choices (Castles 1982; Hibbs 1987a, 1987b; Stephens 1979). It has also strengthened the case for those who view interdependence and vulnerability to international markets as fundamental determinants of patterns of domestic policy (Cameron 1978; Katzenstein 1985).

The findings also speak to the broader question of how governments in democratic capitalism may adapt to the need to maintain adequate investment in new productive capacity and to meet pressures for policy outputs that are expressed through democratic politics. As we have seen, most, if not all, governments have regularly offered investment incentives to maintain adequate capital investment. Yet governments have choices. Left-wing governments, in particular, may confront especially important policy decisions, in that these governments have recently faced, in varying degrees, increasing challenges from the emergence of chronic economic performance problems, tax–welfare backlash movements, neoconservative macroeconomic ideas, and budget deficits. All of these forces called into question the political ability and economic capacity of left-wing governments to perform well on both the equality and growth dimensions of system performance.

As the results indicate, one strategy adopted by these governments generally is to provide relatively large reductions in the tax burdens on business to restore and maintain adequate investment in new productive capacity. From the perspective of left-wing governments within democratic capitalism, this strategy may appear to be a necessary and effective one. From the long-term point of view, maintaining ample investment incentives for business can help to secure future jobs and income for labor. From a near-term vantage point, it may obviate some of the pressure for significant retrenchments in welfare effort, progressive income taxation, and other policies. A remaining question (beyond the scope of this study) is whether a policy mix that emphasizes relatively low corporate tax burdens and generally responsive corporate tax policy within the context of high levels of personal income taxation (and welfare effort) actually works to achieve its desired policy goals.

APPENDIX

OPERATIONALIZATIONS OF THE PRINCIPAL VARIABLES INCLUDED IN ANALYSES

TAXES ON PROFITS$_t$	total taxes on corporate profits as a percentage of net operating income, time t
TAXES ON INCOME$_t$	total taxes on personal income as a percentage of national income, time t
%CH INVESTMENT$_{t-2 \text{ to } t-1}$	percentage change from t − 2 to t − 1 in real investment in machinery and equipment
LEV INVESTMENT$_{t-1}$	investment in machinery and equipment as a percentage of gross domestic product, time t − 1
PROFIT/INCOME$_{t-1}$	net operating income as a percentage of national income, time t − 1

REAL INCOME/POP$_{t-1}$	real income per capita, time t − 1
LEFT CABINET$_{t-1}$	annual average of left-wing party cabinet portfolios as a percentage of all portfolios, time t − 1
CENTER CABINET$_{t-1}$	annual average cabinet portfolios of secular and Christian Democratic centrist parties as a percentage of all portfolios, time t − 1
RIGHT CABINET$_{t-1}$	annual average of right-wing party cabinet portfolios as a percentage of all portfolios, time t − 1
ELECTION$_{t+1}$	dummy variable for year of national legislative election, time t + 1
GROWTH$_{t-2 \text{ to } t-1}$	percentage change from t − 2 to t − 1 in real GDP
INFLATION$_{t-2 \text{ to } t-1}$	percentage change from t − 2 to t − 1 in the Consumer Price Index
OPENNESS$_{t-1}$	imports plus exports as a percentage of GDP, time t − 1
OPEC1 DUMMY$_{1974-79}$	dummy variable coded 1.00 for 1974–79; otherwise 0.00
OPEC2 DUMMY$_{1980-84}$	dummy variable coded 1.00 for 1980–1984; otherwise 0.00
AUSTRALIA . . . U.K.	15 dummy variables for countries/units (N = 16).

DATA SOURCES

Political Data

Left-wing, centrist, and right-wing party cabinet portfolios are from Eric Browne and John Dreijmanis, *Government Coalitions in Western Democracies*, (New York: Longman, 1982) and *Keesings Contemporary Archives* (selected years). Classifications of parties are based on the schema outlined in Francis Castles and Peter Mair, "Left–Right Political Scales: Some Expert Judgments," *European Journal of Political Research* 12 (1984): 73–88. For parties not covered by Castles and Mair or for ambiguous cases, supplementary information is from the *Political Handbook of the World* (selected years) and a variety of country-specific sources. Data on elections are from *International Almanac of Electoral History* (Mackie and Rose 1974); updates in *European Journal of Political Research* (Mackie and Rose selected years).

Economic Data

Data on gross domestic product, real gross domestic product, national income, net operating income, imports, exports, and capital formation are from the OECD's *National Account Statistics* (various years). Data on inflation and interest rates are from the International Monetary Fund's *International Financial Statistics* (various years). Population and labor force data are from the OECD's *Labour Force Statistics* (various years).

Policy Data

Data on personal and corporate income taxes are from the OECD's *Revenue Statistics of OECD Countries* (various years).

Notes

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1. For further expositions and critiques of the thesis, see Elkin 1985; Freeman 1989; Marsh 1983; Przeworski and Wallerstein 1988; Vogel 1983, 1989; and Ward 1987.

2. I do not purport to test all the relationships entailed by the structural dependence thesis. I do not test whether capital investment is crucial for economic growth, whether economic growth is essential for government popularity, or whether various policies are detrimental to economic performance. I simply ask whether the state behaves in the manner suggested by the thesis (as if these relationships were true or were perceived to be true) and responds to low rates of investment with policies that are favorable to the owners of capital.

3. Przeworski and Wallerstein have argued that rather than electoral support per se, some incumbent parties (e.g., left-wing parties) are motivated primarily by the desire to maximize the future welfare of a constituency other than business (e.g., labor) (1988; see also Przeworski 1990, esp. 92–96). However, to the extent that these governments believe particular policies to be a source of low investment, policy change in the favor of owners of capital will be pursued, much as under reelection-driven governments.

4. Governments may also be responsive to variations in capital investment even in the absence of adverse policy impacts; that is, as long as policymakers perceive that policies contribute to low investment, their choices may be shaped by investment considerations. Alternatively, it is possible that policymakers may simply reject the notion that certain policies necessarily engender investment disincentives. For instance, pre-1973 governments of strong Keynesian disposition (e.g., British Labour and the Swedish Social Democratic governments of the 1960s) generally adopted the view that high investment and economic growth rates were compatible with substantial income redistribution (Przeworski and Wallerstein 1982).

5. With regard to collective action, one might note that business mobilizations to act collectively in electoral and lobbying arenas may accompany declining investment and periods of economic stress and, in turn, increase policymaker responsiveness to trends in business investment (Edsall 1984; Vogel 1989).

6. This argument assumes that investment is perceived by policymakers to be relevant to relatively short-term economic performance. While Przeworski and Wallerstein question this notion (1988, 20), there is some evidence for it. For instance, the Report of the Council of Economic Advisors and other economic planning documents in the case of the United States suggest business investment is closely monitored as a determinant of short-term economic performance and that policies to counter investment downturns are readily forthcoming (Swank 1989).

7. With respect to the welfare state, Hicks and Swank (1984b) and Pampel and Stryker (1989) explicitly test the hypothesis, inferred from structural dependence theory, that low rates of capital investment bring forth retrenchments in welfare policies because of business's preference for low levels of social welfare effort. Hicks and Swank report results consistent with the hypothesis; Pampel and Stryker do not.

8. Wallerstein (1988) extends the original analysis of Przeworski and Wallerstein (1988) to include the possibility that disinvestment may occur through international movement of capital. The results of this analysis support the earlier conclusions.

9. Although neo-Keynesian thought, which predominately influenced policymakers at least through the mid-1970s, emphasizes anticipated aggregate demand as a principal source of new investment, neo-Keynesian orthodoxy also recognizes the importance of internal finance or profits and supply-side factors (Bosworth 1984; Fazzari and Mott 1986–87). Neoconservative macroeconomic theories, ascendant in the 1970s, underscore the role of taxation by emphasizing the after-tax rate of return on investment as a key determinant of new investment in productive capacity (e.g., Fazzari and Mott 1986–87; Sahling and Akhtar 1984–85).

10. Reductions in corporate profit taxes may come through either rate reductions or tax-based investment incentives, and these two avenues have both been used across time in capitalist democracies to change corporate tax burdens (Pechman 1988). Unfortunately, cross-nationally comparable individual annual measures of corporate tax rates or investment incentives only exist for a subset of focal nations and years.

11. While recent research on the work, saving, and investment disincentives of social welfare and personal income tax policies reveals only moderate-to-weak disincentive effects of policy (e.g., Page 1983; Pechman 1990), left-wing governments faced frequent challenges to redistributive policies. Beginning in the late 1960s, tax–welfare backlash movements and parties in many countries highlighted purported disincentive effects of high levels of taxation. These trends were dramatically reinforced in the early 1970s and beyond by poor economic performance and the concomitant ascendance of neoconservative macroeconomic theories.

12. See n. 9

13. The nations are Australia, Austria, Belgium, Canada, Denmark, Finland, France, West Germany, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, the United Kingdom, and the United States. Japan and New Zealand, two nations often included with this group, were omitted because of the absence of data on business investment in Japan before the early 1970s and large amounts of missing data for New Zealand. Iceland, Israel, Luxembourg, and several less-industrialized capitalist democracies were omitted because of nation-specific peculiarities (e.g., small size) and a paucity of relevant data.

14. Corporate tax rates generally consist of flat rates on corporate income. However, allowances and exemptions on invested retained earnings (in nearly all of the countries), lower rates on distributed profits in some nations (e.g., Austria), and low rates on initial corporate earnings in some nations (e.g., Canada) assure some cyclical association between profits and corporate tax burdens.

15. A separate control for this automatic effect is important to isolating discretionary policy response to investment. In addition, students of tax policy may also be quick to note that recent changes (i.e., post-1984) in corporate tax policy have been in the direction of eliminating investment tax credits and allowances and reducing overall corporate tax rates.

16. For the 1970s and after, this group included Australia, Canada, the Netherlands, Norway, Sweden, the United Kingdom, and the United States. Beginning in 1989, the OECD (in its *Economic Outlook*) began providing estimates of the volume indexes of private investment from the early 1970s to the present for most OECD nations.

17. The lag for %CH INVESTMENT, when the original variable is defined as change from $t - 1$ to t, captures changes between $t - 2$ and $t - 1$. Because many crucial tax policy choices are often made by executives in most of our nations between March and September of $t - 1$ and by legislatures between July and December of $t - 1$, this lag should tap variation in the key monitoring period of policymakers (typically, the four quarters before policy choice). Also, given the autoregressive nature of the investment variable, it should be related to past trends and short-term forecasts used by policymakers. Accordingly (and also in light of controls for automatic effects), I interpret the reported relationships between investment and taxation as reflecting discretionary policy responses. That is, such relationships may, in part, stem from legislative changes in corporate tax policy (e.g., in the mix and magnitude of investment incentives and in corporate tax rates) and discretionary action of tax authorities in enforcement and interpretation of tax codes that stem from changes in investment.

18. One might also specify the models with a lagged endogenous variable to estimate inertia and the partial adjustment of policy to investment (Johnston 1984). All focal models I discuss were reestimated with such lagged endogenous variables. These reanalyses reveal no differing conclusions for any hypotheses. (Lagged endogenous variable equation results are available on request.)

19. The first observation in generalized least squares estimation is kept by using the standard procedure offered by Poirier (1978). Examinations of nation-specific first-order rhos and residual variances suggested moderate-to-high first-order autocorrelation and low-to-moderate heteroscedasticity and contemporaneous correlation across all models presented below. To obviate concerns for any of these problems, the generalized least squares estimator was used for all models.

20. It may be helpful at this point to recall the computational procedures and interpretation of interaction terms. Briefly, the interaction of, let's say, X_1 and X_2, when the explanandum is Y will tell us whether the effect of X_2 on Y varies by levels of X_1 (or vice versa). The individual coefficient for X_2 is an index of the effect of X_2 when X_1 has a value of .00. The interaction term itself when multiplied by a given value of one of the two variables (say, X_1) and added to the coefficient of the other variable (say, X_2) becomes the slope for the effect of the second variable (X_2) at that given value of the first variable (X_1). The significance test for the interaction simply tells us whether differences in the slope of the second variable (X_2) at different levels of the first (X_1) are significantly different from zero. The significance test for the X_2 is a test of the significance of X_2 when X_1 is .00. Significance tests for effects of X_2 at any level of X_1 are readily computed. To compute the requisite standard error for the coefficient of X_2 at any given level of X_1, Friedrich (1982) points out that s_{b2-X1} is given by the square root of $[\text{VAR}(b_2) + X_1^2\text{VAR}(b_3) + 2X_1\text{COVAR}(b_2b_3]$, when b_1 and b_2 are the estimated coefficients for X_1 and X_2, respectively, and b_3 is the coefficient for the interaction $X_1 * X_2$.

21. With respect to the stability of the investment effects, one might inquire whether investment effects on corporate tax burdens are generated largely by experiences of one particular nation. Such an analysis was performed, in which the baseline model of the first pair of columns of Table 2 was reestimated 16 times, each time deleting the series for one nation. The coefficient for investment remained positive and highly significant in all cases. (Results of these 15-nation estimates are available on request.)

22. The insignificance of the capital investment variable itself in the left-wing interaction model indicates (as noted in my discussion of interaction methodology) that the effect of investment outside left-wing government, on average, is insignificant. This finding, coupled with our finding of increasingly large policy impacts of investment under right-wing governments, indicates that the policy impacts of investment under centrist governments may actually be low or nonexistent. Analysis of the tax policy effects of investment under centrist governments actually revealed that the significant, positive effect of investment on corporate tax burdens disappears in governments with more than 50% of cabinet portfolios held by centrist parties. Overall, this indicates that

centrist parties, unlike left-wing and right-wing parties, do not systematically prefer the use of business tax relief in the face of low rates of investment, perhaps favoring fiscal and monetary policy alternatives. However, to answer this question fully and to rule out the possibility that the investment coefficient in the left-wing interaction model may have fallen below significance because of multicollinearity, a careful cross-national study of the tax policy preferences and behaviors of centrist parties over time would be required

23. The experiences of individual nations indicate that policymakers frequently reduced tax burdens during the 1974–79 period. For instance, new investment incentives (e.g., more generous depreciation schedules and investment tax credits) or lower corporate rates were enacted in the United Kingdom (1974, 1975), the United States (1975, 1979), and France (1975, 1977, 1978). While these policies continued in the early 1980s, some nations altered course and revoked some of the recently enacted business investment incentives and corporate tax rate reductions (e.g., the United States in 1982, the United Kingdom in 1984).

24. To explore fully whether or not policymakers responded to lagging investment by reducing tax rates on those strata most likely to save, I could have used measures of tax rates by income group and examined their responses to investment. Unfortunately, cross-nationally comparable time-series data on effective income taxes by income group (e.g., deciles, quintiles) is unavailable for a large number of the focal nations.

25. Both cross-national and U.S. research has shown that the bulk of redistribution achieved through direct fiscal policies comes through income transfers (e.g., Hicks and Swank 1984a; Pechman 1990). However, personal income taxes and income transfers for our sample are highly correlated ($r = .77$, $p < .001$). Moreover, in parallel work, I have also found that left-wing party government control and left-wing party oppositional strength are related to income transfers (Hicks and Swank 1984b, 1990).

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"SOPHISTICATED" VOTING IN THE 1988 PRESIDENTIAL PRIMARIES

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Voters in multicandidate contests may confront circumstances under which it is in their interest to vote for a second- or even lower-ranked candidate. The U.S. electoral system, typically offering a choice between only two major contenders, rarely presents opportunities for this "sophisticated" voting. In presidential primaries, however, many plausible candidates may compete. We investigate the presence of sophisticated voting in the 1988 presidential primaries, using data from the National Election Study's Super Tuesday survey. We examine patterns of voting types based on ordinal measures of preferences among candidates and assessments of their chances of winning their party's nomination and estimate several models of choice, testing the multicandidate calculus of voting. Among both Republicans and Democrats, respondents' choices were consistent with the calculus of voting and thus with sophisticated voting.

Voters make decisions in a world of imperfect information, but the lack of information is particularly acute in nomination contests. Voters cannot rely upon their partisan loyalties to reduce information costs, and they often must choose among candidates with similar policy positions. Moreover, unlike most general elections, in which the effective choice is between two major party candidates, nomination contests often involve three or more candidates. The choice among candidates is particularly complicated in U.S. presidential nominations because, unlike most party nominations, choices are not made in a single primary election. Instead, the nomination contest lasts for several months; and the information available to the primary or caucus voter varies greatly over time.

As presidential nominations are usually multicandidate contests, voters may confront situations in which they have reasons not to vote for their most preferred candidate. At the outset of the campaign, voters may have very little information about the candidates' prospects. After the Iowa caucuses and the New Hampshire primary, some candidates are winnowed out; others continue with substantially reduced prospects; and still others have greatly enhanced chances of victory. New information about the candidates' prospects may lead some voters to alter their choices. While some may support their most preferred candidates regardless of their prospects of victory, others may also weigh those prospects. Those who vote for their most preferred candidate without taking that candidate's chances into account cast a *sincere vote*. Those who weigh both their preferences and the candidates' chances cast a *strategic, or sophisticated, vote*, perhaps voting for their next-most-preferred—or an even lower rated—candidate.

Many popular accounts of presidential nomination contests describe the electorate as ill informed, capricious, and vulnerable to media manipulation. Some academic accounts are equally unflattering (e.g., Keeter and Zukin, 1983). Strategic or sophisticated voting (or any other form of rational voting) seem at odds with such accounts.

Our analysis suggests that voters make relatively good use of the available information. Voters appear to have relatively stable evaluations of the candidates, and they appear to be attuned to their fortunes in a reasonably informed and sensible way. Most voters do support the candidate they most prefer; but many appear to be guided in part by information about the candidates' prospects of winning their party's nomination, or *viability*.[1] That voters often act strategically tends to support rational choice models of behavior, particularly the "calculus of voting" (Downs, 1957; Riker and Ordeshook, 1968, 1973) that combines utility and probability terms. Such behavior is inconsistent with Ferejohn and Fiorina's minimax-regret model (1974, 1975), which predicts that rational voters never engage in strategic voting.

The 1988 presidential contest provides an excellent opportunity to test for the presence of sophisticated voting. Most importantly, it is the first contest since the post-1968 reforms in which neither party had an incumbent president seeking his party's nomination. As a result, both contests began with many major contenders. Moreover, the National Election Study's (NES's) Super Tuesday survey, conducted by the Center for Political Studies of the University of Michigan, provides appropriate data for studying individual-level choices.

This study was conducted using telephone polling from 17 January through 8 March (Super Tuesday) in the 16 states holding primaries on Super Tuesday. A total of 2,117 respondents were interviewed, and the continuous monitoring design provides a random sample for individual weeks or other time periods.[2] The survey asked respondents which Democratic and which Republican candidate they would vote for—our dependent measure, *vote preference*.[3] Two other

questions provide the information needed to ascertain whether the vote preference was strategic, sincere, or of some other type. First, respondents were asked to evaluate all candidates they had heard of on a feeling thermometer (0–100 degrees in ascending favorability). Second, respondents were asked to rate each of these candidates' chances of winning the Democratic or Republican presidential nomination on another hundred-point scale.

The Super Tuesday Study suggests that voters may have been behaving in a sophisticated manner. Among Republicans, George Bush held a 37-point lead in vote preferences over Robert Dole in the weeks before the Iowa caucuses (see Table 1).[4] After the Iowa caucuses (8 February), in which Dole finished first and Bush finished third, Dole jumped ahead by 8 points. But on 16 February, Bush won the New Hampshire primary, with Dole finishing second. Bush held a 29-point lead among Republicans interviewed after the New Hampshire primary. But during these time periods candidate evaluations, as measured by the feeling thermometers, changed very little. There were, however, substantial changes in their perceived viabilities. The relative stability of evaluations suggests that the dramatic changes in voter preferences were due to the equally dramatic changes in viabilities, a pattern consistent with the theory of strategic voting.

The table also provides evidence of sophisticated voting among Democrats. Before the Iowa caucuses, Dukakis held a 10-point lead over Gephardt in both voting preferences and viability, along with a slightly smaller lead in candidate evaluations. After Gephardt's first-place showing in Iowa, with Dukakis finishing third, Gephardt emerged with an 11-point lead in voting preferences, a 4-point edge in evaluations, and a 7-point advantage in perceived viability, although Dukakis led in Gallup polls conducted in New Hampshire (see Abramson, Aldrich, and Rohde, 1991, table 1.2). After Dukakis' New Hampshire win (with Gephardt finishing second), Dukakis regained his lead on all three measures; but his gains in vote preference and perceived viability were much greater than his gains on the candidate thermometers.

This aggregate evidence suggests that voters may be responding to changing information, and may condition their choice of candidates both on their evaluations of the candidates and on their perceptions of the candidates' chances for success. Before analyzing these data further, we will briefly review the multicandidate calculus of voting, the theory of sophisticated voting.

SOPHISTICATED VOTING AND THE THEORY OF THE CALCULUS OF VOTING

The logic of not "wasting" a vote on a hopeless candidate or party has long been discussed in popular and academic accounts of elections; but this logic applies only when there are more than two choices. In most U.S. general elections, third-party candidates are so weak that few voters even consider voting for them. But when third-party candidates generate support, as in the 1980 presidential election, arguments about wasting votes are likely to be raised and acted upon (see Abramson, Aldrich, and Rohde, 1983, 174–76).

The wasted vote logic is compatible with the theory of the calculus of voting, which is based on expected utility maximization. Originally developed by Downs (1957) and Riker and Ordeshook (1968, 1973) to model turnout in two-candidate races, the calculus was extended to multicandidate contests and conditions for sophisticated voting were derived by McKelvey and Ordeshook (1972). Black (1978) and Cain (1978) tested versions of this calculus using Canadian and British survey data, respectively, and found strong evidence that some voters cast sophisticated votes.

The essential logic is simple. Suppose there are three candidates—A, B, and C—whom a voter prefers in that order. Suppose that A has very little chance of winning, but that the race between B and C is close. One form of sophisticated voting occurs if the voter casts a ballot for B to avoid wasting a vote on A's hopeless candidacy. The keys to the calculus of voting are the relative intensity of preferences (utility comparisons) for the three candidates and the voter's perception that his or her vote can help make a candidate win or lose. The keys, that is, are the *expected utilities* associated with voting for A, B, or C or with abstaining.

As can be demonstrated (see Abramson et al. 1990; Aldrich 1991), the rational voter's choice depends on the comparative *utilities* associated with the candidates and the relative *probabilities* of outcomes. Thus, the likelihood of voting for the second-choice candidate increases as the voter evaluates A and B more similarly and as B is increasingly preferred to C. It also increases as the chances that one's vote would help B win grow and as the chances of A's winning decline. For the sophisticated voter, the appropriate action depends on the relative comparisons of utilities and probabilities.

Because the probability of a single vote affecting the outcome of an election is very small, some argue that very few voters will consider the candidates' chances (see Barry 1970; Ferejohn and Fiorina 1974, 1975). Admittedly, in the real world, very few voters think in terms of making or breaking a tie. Like all theories, the calculus of voting is a simplification of reality that seeks to capture the most salient features of actual situations. Many voters may see some candidates as having real chances of winning and others as likely losers, and they may weigh these perceptions against the relative attractiveness of the candidates. This might be especially true in presidential primaries; for each vote has a small, but real, chance of increasing a candidate's delegate total. Moreover, especially in the early stages of the primary season, momentumlike

TABLE 1.

Vote Preferences, Candidate Evaluations, and Perceived Nomination Prospects of Candidates by Party and Date of Survey

CANDIDATES AS PERCEIVED BY SUPPORTERS OF THEIR OWN PARTY	TIME PERIODS: PRE-IOWA CAUCUSES	IOWA CAUCUSES TO NEW HAMPSHIRE PRIMARY	NEW HAMPSHIRE PRIMARY TO SUPER TUESDAY PRIMARIES
Percentage vote preference			
Bush	60.0	39.4	56.7
Dole	23.5	47.4	28.2
Difference	36.5	−8.0	28.5
	(230)	(137)	(305)
Average thermometer score			
Bush	69.5	66.2	70.5
	(223)	(131)	(297)
Dole	66.2	68.5	66.7
	(183)	(124)	(264)
Difference	3.4	−2.3	3.8
Average chances of nomination			
Bush	76.6	67.6	75.0
	(223)	(130)	(297)
Dole	64.0	71.7	67.5
	(183)	(125)	(265)
Difference	12.6	−4.2	7.5
Percentage vote preference			
Dukakis	18.9	20.2	34.6
Gephardt	9.0	31.2	15.4
Difference	9.9	−11.0	19.1
	(212)	(109)	(324)
Average thermometer score			
Dukakis	60.5	58.9	61.3
	(120)	(69)	(225)
Gephardt	54.5	63.2	56.1
	(105)	(74)	(214)
Difference	6.1	−4.3	5.3
Average chances of nomination			
Dukakis	57.2	59.8	68.3
	(120)	(70)	(224)
Gephardt	47.1	66.7	57.8
	(107)	(74)	(213)
Difference	10.2	−6.9	10.5

Note: Number of cases in parentheses.

dynamics may magnify the impact of each additional delegate (see Aldrich 1980a, 1980b; Bartels 1988).

Sophisticated voters may wind up voting for their first choice. A sophisticated vote is one based on weighing both preferences and probabilities of success. A sincere vote is based upon preferences alone and leads to voting for the most preferred candidate, regardless of circumstances. When a voter's sophisticated and sincere choices coincide, the choice is called "straightforward" (Farquharson 1969). That most citizens vote for their first-ranked alternative, therefore, does not demonstrate that they are voting sincerely. Many may be sophisticated voters facing circumstances that yield a straightforward choice.

THE CAMPAIGN SETTING

The 1988 presidential nominations were contested vigorously by six major Republican candidates and

seven Democrats. As in all contests since 1976, the Iowa caucuses and the New Hampshire primary were the two most important early contests. Three weeks after the New Hampshire primary, 20 states, concentrated in the South, held primaries or caucuses on the same day. Super Tuesday provided by far the largest number of delegates to be won on a single date and became the third important campaign event. Bush had been the Republican front-runner from the beginning, with Dole seen as his major challenger. After the Iowa caucuses, Dole led Bush in polls conducted in New Hampshire (see Abramson, Aldrich, and Rohde 1991, table 1.2), much as he did in the Super Tuesday survey (see Table 1). But after his New Hampshire victory, Bush was again the front-runner. Alexander Haig had withdrawn four days before the New Hampshire primary, and Pierre ("Pete") du Pont IV withdrew two days after that primary. Jack F. Kemp, Pat Robertson, and Dole had all been seriously weakened.

The Democratic race was more complicated. No candidate could be considered a clear front-runner, although both Gephardt and Dukakis were major candidates. Among the seven Democrats, only Bruce Babbitt (who quit two days after the New Hampshire primary) withdrew before Super Tuesday. But Iowa and New Hampshire had dramatically altered the prospects of several candidates. Gary Hart's chances were drastically reduced, and Paul Simon announced that he would sit out Super Tuesday. Gephardt came in second in New Hampshire; but he trailed Dukakis by 16 percentage points, diminishing his prospects. On the other hand, Jesse Jackson and Albert Gore, Jr. had yet to make a major effort; and Super Tuesday would be their first major test.

These events apparently led voters to reevaluate the prospects of the competing candidates, and these changed perceptions may have affected voting preferences. However, supporters of Robertson and Jackson might be less affected than the supporters of other candidates. From the outset Robertson and Jackson were given little chance of winning, but their supporters were strongly committed to them. Gore's supporters might believe that his prospects were yet to be tested. One might expect, therefore, high levels of sincere voting for Robertson, Jackson, and perhaps Gore. Such expectations, however, could also be consistent with sophisticated voting, yielding straightforward choices based on strong preferences.

Our preliminary analysis of the Super Tuesday Study suggests that there was sophisticated voting; but a better test requires examining individual-level preferences, evaluations, and assessments of the candidates' chances of winning. As noted, we have used the hundred-point feeling thermometers to create both ordinal preference rankings of the candidates and interval scores. For the latter, the hundred points is an arbitrary absolute scale. These scores can be viewed as measuring the utility the respondent attaches to electing each candidate. Theoretically important are the intervals between the various candidates. Without loss, we can assign a score of one to the highest-ranking and zero to the lowest-ranking candidate (say, candidate n). What is critical, then, is where the middle-ranked candidates fall. For example, the score for the second-ranked candidate can be normalized as follows:

$$u_2 = (U_2 - U_n)/(U_1 - U_n), \quad (1)$$

where U_j indicates the original thermometer score and u_2 indicates the "normalized" evaluation score for the second-ranked candidate.[5] Hence, for all respondents, the normalized scores are one for the highest-ranked candidate, u_2 for the second-ranked candidate, and zero for the lowest-ranked candidate. Thus, u_2 will be between zero (if candidate 2 is tied in score for lowest-ranking) and one (if two are tied for top-ranking). Normalized scores can be derived for all other middle-ranked candidates in similar fashion.

Assessments of candidates' chances of winning nomination were also measured by hundred point scales, and these responses were also normalized. If n candidates are rated, let p_j denote the "normalized" probability score and P_j denote the original score for candidate j. Then, "normalized" viability scores can be found by

$$p_j = P_j/(P_1 + \ldots + P_n). \quad (2)$$

These variables provide the basic data for estimation.

PREFERENCES, EVALUATIONS, AND VIABILITIES

We begin by examining the relationship between voter preferences and candidate evaluations and between voting preferences and viability ratings. Among the 649 Republicans with a vote preference who rated a candidate, 87% had a vote preference for the candidate they rated highest on the thermometer scale. Among the 609 Democrats with a vote preference and who rated at least one candidate on the feeling thermometers, 85% reported a vote preference for the candidate they rated highest on the thermometer scale (for detailed results, see Abramson et al. 1990, table 2). As high as these percentages are, they are lower than the 95% who report voting for their highest-ranked major party candidate in NES general election surveys (see Abramson, Aldrich, and Rohde 1983, table 8.7). Ten percent of the Republicans and 9% of the Democrats registered a vote preference for their second-ranked candidate. These figures do not demonstrate that the vast majority of respondents are sincere. Many may have strategically straightforward choices.

Among the 647 Republicans with a vote preference who ranked a candidate on the hundred point viability scale, 79% reported a vote preference for the candidate they thought most likely to win the party's nomination; among the 608 comparable Democrats, 72% reported a vote preference for the candidate they thought most viable. (For detailed results, see

TABLE 2.

Classification of Voting Types Based on Rank Orders of Evaluations and Perceived Viability

EVALUATION RATING OF RESPONDENT'S CHOSEN CANDIDATE	RANKING OF NOMINATION VIABILITY OF RESPONDENT'S CHOSEN CANDIDATE		
	HIGHER	SAME	LOWER
HIGHER	straightforward by evaluations and viability	straightforward by evaluations	apparently sincere
SAME	apparently sophisticated by viability	straightforward by equality	irrational by viability
LOWER	apparently sophisticated	irrational by evaluations	irrational by evaluations and viability

Note: One-paired comparison. Four other categories are (1) rating exactly two candidates on feeling thermometer, with vote preference for the higher-rated candidate, classified as *rational but uninteresting*, since sincere and sophisticated choices are indistinguishable; (2) rating only one candidate on the feeling thermometer, with vote preference for that candidate, classified as *rational but uninteresting*; (3) having vote preference for candidate rated lowest overall on feeling thermometer, classified as *irrational*, and (4) having vote preference for candidate not rated on feeling thermometer, classified separately (i.e., neither rational nor irrational).

Abramson et al. 1990, table 3. In each party, 14% had a voting preference for the candidate they believed was the second-most-likely to win the party's nomination.) Once again, this does not necessarily indicate that most respondents were purely sincere. They, too, might have straightforward choices. Indeed, these data suggest that many supported the candidate they liked best and believed had the best chances of success.

To some extent voters may overestimate the chances of the candidates they like, and some may like candidates they believe will be successful. Bartels (1988) examined the relationship between expectations and preferences, finding that both Mondale's and Hart's thermometer evaluations in the 1984 primary season were a function, in part, of respondents' perceptions of their chances of winning the nomination and also that these perceived chances were, in part, a function of respondents' evaluations on the feeling thermometer. He also found similar relationships in data from the 1980 nomination contests (Bartels 1985). Paolino (1989) reported similar findings with 1988 data. Our own analyses, reported below, suggest that candidate evaluations affect assessments of viability. Thus, it appears that respondents partially project their evaluations of candidates onto their perceptions of candidate viability, seeing candidates they evaluate highly as more likely to be nominated. According to our estimates, respondents do not appear to rationalize their evaluations of the candidates.

Because respondents are likely to overestimate the chances of their preferred candidates, the number of seemingly straightforward voters may be inflated. We believe this would be true mainly for the most highly evaluated candidate. Viabilities would not, it seems likely, be projected consistently across the sample for candidates rated second-highest. Thus, the extent of apparent straightforwardness, that is, voting for one's first choice who also is considered most viable, might be higher in these data than it "should" be; but voting for second-choice candidates should be largely unaffected. We shall return to this point later.

ANALYSIS OF ORDINAL VOTER TYPES

The critical question is how voting preferences are affected by the combination of preferences and expectations about viability. We shall examine these relationships in an ordinal framework before turning to model estimation. The logic can be seen in Table 2, which presents how a respondent can compare two candidates' ordinal evaluations and viabilities. Our full classification is based upon the comparison between the candidate the respondent prefers to support and every other candidate. The resulting typology is reported in Table 2.

The cell in the upper right-hand corner of Table 2 corresponds to a respondent whose vote preference is for the candidate ranked higher on the feeling thermometer but thought to have lower viability. This configuration is the classic case of sincere voting, that is, voting for the less viable but more preferred alternative. We call this type of voter *apparently sincere*, because actual sincerity depends upon the order of magnitude involved. The upper left-hand cell typifies the *straightforward* voter. For this voter, there is no trade-off between evaluations and viability, since the highest-rated candidate is also the most viable one. The bottom left-hand cell corresponds to *apparently sophisticated* voting, because the individual supports a more viable candidate over one who is better liked. The bottom right-hand cell is labeled *irrational*, since the voter supports a candidate who is less well liked and less viable than another. A complete typology of response types includes nine additional cases. Five types involve ties and are displayed in Table 2. Four additional categories are listed below the table.

This typology of voting types is developed by examining all pairs of candidate comparisons, using only ordinal information. (See Abramson et al. 1990,

TABLE 3.

Distribution of Voting Types by Political Party (%)

VOTING TYPE	POLITICAL PARTY	
	REPUBLICANS	DEMOCRATS
Straightforward	52.8	43.8
Apparently sophisticated	13.9	13.1
Apparently sincere	10.3	14.3
Rational but uninteresting	6.5	10.5
"Irrational"	10.0	11.7
Vote preference for unrated candidate	4.6	6.8
Total Percent	100.1%	100.2%
Number of cases	680	650

app. 1, for a detailed explanation.) The candidate the respondent preferred to vote for was compared to every other contender for the party's nomination in turn. If the preferred candidate was rated higher than every other candidate on both evaluations and viability, the respondent was classified as straightforward; and so on.

The basic distribution of these typologies is reported in Table 3. In both parties, straightforward voting was the most common classification. About one in seven was classified as apparently sophisticated, while a few more Democrats and a few less Republicans were apparently sincere. Between 10 and 12% were classified as irrational and 6 to 10% as rational but uninteresting. The remaining 5 to 7% preferred a candidate whom they did not rate on the feeling thermometer.

Table 3 suggests that most of the action is concentrated in the three most interesting cases for our analysis: the straightforward, apparently sincere, and apparently sophisticated categories.[6] Counting rational but uninteresting cases, over 8 in 10 supported a candidate in a fashion consistent with rational choice. Most respondents did not to have to face a tough choice between a candidate they liked most and another they believed more likely to succeed. When there was such a trade-off, respondents were somewhat more likely to support the candidate they thought would win.

We can use these classifications to examine some political questions. Do these patterns change over time, due either to the changing circumstances of the campaign or simply to the proximity of the interview to Super Tuesday? Is there more apparently sophisticated reasoning among some subgroups than others? The relative sizes of the categories of *uninteresting, irrational,* and *preferring an unrated candidate* might be affected by the level of information held, assimilated, and used by a respondent. Thus, we might expect the frequency of these categories to decrease over time and/or as the political and intellectual skills and attributes of the respondents increase. The *uninteresting* category, for example, may become less common as the primary day nears, simply because more respondents have more information and therefore rate more candidates. The use of more complex information in decisionmaking may increase with either time or awareness. For example, the incidence of apparently sophisticated choices might increase, relative to apparent sincerity.

Our analysis reveals that there were relatively few differences among voting types between Republicans and Democrats (see Table 3) by the time period of the survey, level of education, interest in the campaign, or extent to which the respondent followed the campaign in the newspapers (see Abramson et al. 1990, tables 5–8).[7] To be sure, increasing information and proximity to Super Tuesday do lead to fuller and richer evaluations; and, as expected the proportion in the *rational but uninteresting* and *vote preference for unrated candidate* categories decline as political involvement increases. There is little in these data to suggest, however, that voting straightforwardly, strategically, or sincerely varies substantially. In short, fuller information leads to rating more candidates but does not seem to affect greatly how these evaluations and assessments of viability are employed in making decisions.

Vote preferences, by contrast, are strongly related to voting types, as the results in Table 4 reveal. It reports the distribution of voting types among Republicans who preferred Bush, Dole, and Robertson. Those who preferred Robertson were far more likely to be classified as apparently sincere than were those who preferred Bush or Dole. Conversely, very few of Robertson's supporters were apparently sophisticated voters. Those who preferred Bush present the opposite profile. He was the straightforward choice for three out of five of his supporters. Most of the remainder came to prefer him as the apparently sophisticated choice, while very few were apparently sincere. Supporters of Dole fell somewhere between those preferring to vote for Bush or Robertson, but they were more similar to Bush supporters.

The table shows a similar pattern among supporters of the five leading Democrats. Those who preferred Jackson were much like Republicans who preferred Robertson—disproportionately likely to be apparently sincere and very unlikely to be apparently sophisticated. The reason is the same in both cases—

TABLE 4.

Distribution of Voting Types by Vote Preference (%)

	REPUBLICANS			DEMOCRATS				
VOTING TYPES	BUSH	DOLE	ROBERTSON	DUKAKIS	GEPHARDT	GORE	HART	JACKSON
Straightforward	60.3	51.9	32.8	54.6	51.0	50.5	36.8	33.3
Apparently sophisticated	19.2	15.9	1.6	23.9	18.3	7.1	10.4	2.9
Apparently sincere	2.2	12.0	32.8	5.1	5.8	17.2	14.9	21.7
Rational but uninteresting	9.7	1.0	9.4	3.9	3.9	8.1	21.8	21.0
"Irrational"	5.1	12.5	20.3	5.7	9.6	12.1	10.4	15.9
Vote preference for unrated candidate	3.5	6.7	3.1	6.8	11.5	5.0	5.8	5.1
Total Percent	100.0%	100.0%	100.0%	100.0%	100.1%	100.0%	100.1%	99.9%
Number of cases	370	208	64	176	104	99	87	138

few thought their preferred candidate had a good chance of winning. Those who preferred Dukakis were much like those who preferred Bush, with many falling into the straightforward and into the apparently sophisticated categories. Few were categorized as apparently sincere. Gephardt supporters were almost as likely as Dukakis supporters to be classified as straightforward and as sophisticated voters. Half of Gore's supporters were straightforward, as well; but they were less likely to be apparently sophisticated and more likely to be apparently sincere.

We have reported elsewhere the distribution of voting types by candidate preferences in the three major time periods of the campaign (see Abramson et al. 1990, table 2A5). We expect, for example, that after his loss in Iowa, fewer Bush supporters should be apparently sophisticated and more might be apparently sincere or straightforward by preferences alone. We expect these patterns to be reversed after his New Hampshire victory. Those preferring Dole should reveal the opposite patterns of changes. Patterns of voting types among Democrats preferring Dukakis should be similar to those among Republicans preferring Bush, while Democrats preferring Gephardt should be similar to those preferring Dole. There should be relatively little change in voting types among those Robertson or Jackson supporters, since they tended to prefer their candidate regardless of his chances.

The over-time changes among those preferring to vote for Bush and Dole are uniformly consistent with these expectations; and many of these comparisons, especially in the *straightforward by evaluations* subcategory, are large. Among Democrats, these patterns are mostly consistent with these expectations, as well, although the magnitudes are often somewhat smaller.

These analyses suggest two major conclusions: (1) the distribution of voting types varies little with measures of political involvement; and (2) straightforward, apparently sincere, and apparently sophisticated voting types do vary substantially by respondents' preferred candidates. Moreover, these types vary over time in ways that are consistent with the realities of the campaign. Thus, this analysis suggests that respondents are attuned to the chances of the candidates and incorporate such considerations in their candidate preferences.

ESTIMATION OF MODELS BASED ON THE MULTICANDIDATE CALCULUS OF VOTING

The multicandidate calculus of voting assumes that all voters are "sophisticated," because it assumes that all choose on the basis of expected utility maximization. The central terms in the determination of voters' choices, therefore, involve their candidate preferences (or utilities) and their assessments of the candidates' viabilities. We shall test two statistical models based on the calculus of voting.

We begin by examining viability, given candidate preference orders—replicating analyses conducted by Black (1978), who used data from the 1968 and 1972 Canadian federal elections. He employed ordinal preference rankings from survey responses and measures of probability terms based on actual election returns. His tests were based on the assumption of the calculus of voting that voters who shared similar preference orderings would be more or less likely to vote for their most- or next-most-preferred party, given differing probabilities of casting decisive votes.

Here, we estimated similar models. The dependent variable is dichotomous, indicating whether respondents preferred to vote for their first (scored one) or second (scored zero) most highly ranked candidate, as measured by the feeling thermometers. As we reported above, about 85% preferred to vote for their first-ranked candidate. Ten percent preferred to vote for their second-ranked candidate, a result similar to the percentage that reported voting for their second-ranked party in Cain's (1978) study of the 1970 British general election. Too few preferred any lower-rated candidate to make estimation feasible.

The independent variables are comparisons of the

TABLE 5.

Probit Estimates of the Probability of Preferring to Vote for the First- or Second-highest-ranked Candidate, Given Ordinal Preferences and Differing Assessments of Candidate Viability

INDEPENDENT VARIABLES	MLE		
	REPUBLICANS	DEMOCRATS	BOTH PARTIES
P_{12}	2.285*	3.123*	2.715*
	(.600)	(.610)	(.421)
P_{13}	1.707*	1.290*	1.413*
	(.546)	(.529)	(.370)
Constant	1.040*	1.194*	1.121*
	(.0915)	(.0884)	(.0623)
-2xLLF (χ^2_2)	44.68*	69.40*	111.99*
Number of cases	503	483	986

Note: MLE denotes maximum likelihood estimate. P_{xy} denotes the difference in normalized viability scores given to candidates x and y. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .05$ (one-tailed test of coefficient estimates).

assessments of viabilities. (Differences in the normalized viability measures [see equation 2] will be used and referred to in Tables 5 and 6 as P_{12} for the difference in viability between the *first-* and *second-*rated candidates, and so on.) The large number of paired comparisons of viabilities, due to the large number of candidates, prohibit including all possible paired comparisons.[8] Therefore, we have included comparisons among the candidates rated highest and next highest on the feeling thermometers and the candidate the respondent assesses as most viable of the remaining, lower-rated contenders. Since the hypothesis of sophisticated voting is that voters consider supporting their second-choice candidate to avoid selection of some even lower-rated candidate, this procedure should select the lower-rated candidate whom voters see as the most significant "threat." As a result, there are three viability comparisons, P_{12}, P_{13}, and P_{23}. Parameter estimates of the first two variables should be positive, while the third should be negative. Note that by definition, $P_{23} = P_{13} - P_{12}$. Therefore, only the later two variables are included in the estimation.[9]

The results of probit estimates of this model are reported in Table 5 for Republicans, Democrats, and the two parties combined. The overall fit of these models is strong and statistically significant. The viability difference variables are significant and correctly signed in all three cases. The coefficient estimates are relatively large, and their impact can be consequential. For example, compare two voters with normalized viability estimates of their three candidates of .5, .3, and .2 and of .3, .5, and .2, respectively. Based on the Republican estimates, the first voter would be expected to vote for the most preferred candidate with a probability of .98, while the second voter's likelihood of voting for the most preferred candidate drops to .77. The comparable Democratic probability estimates are .99 and .76, respectively. In short, the estimates clearly support the theory; the similarity in coefficient estimates between the two parties is striking; and the impact of the coefficients is substantial.

A second set of probit models more fully exploits the measures available. The heart of expected utility is the multiplication of utility and probability; and we construct such models here by multiplying the difference in viability of two candidates by the difference in utility evaluations (see equation 1). Cain (1978) estimated similar models using data from the Butler and Stokes survey of the 1970 British general election. The dependent variable is the same as in Table 5, and the same sets of three candidates are employed. The independent variables are denoted as PB_{jk} in the tables, representing $(P_j - P_k)(U_j - U_k)$ for candidates j and k, and the normalized measures of both viability and utility are used (see equations 1 and 2). As before, we expect coefficient estimates for the PB_{12} and PB_{13} to be positive, since greater expected utility for the most preferred candidate compared to the other two should make supporting the most preferred candidate more likely. Conversely, we expect the parameter estimate for the PB_{23} variable to be negative, controlling for the other two variables. As the expected utility for the second candidate is increasingly larger compared to the third candidate, the likelihood of voting for the second-most-preferred candidate should increase. In this case, PB_{23} is not, by definition, equal to the difference in the other two variables; therefore, all three variables can be included.

The probit estimates for these models are reported in Table 6. The overall goodness-of-fit statistics are again large and significant for all three estimations (Republicans, Democrats, and both parties combined). All parameter estimates have the correct sign, and those involving the most preferred candidate are statistically significant in each model. The PB_{23} coefficient is large and statistically significant for Democrats and for both parties, although it is small and insignificant (albeit correctly signed) for Republicans. As in the first model, the impact of the independent

TABLE 6.

Probit Estimates of the Probability of Preferring to Vote for the First- or Second-highest-ranked Candidate, Given Normalized, Multiplicative Utility and Viability Measures

INDEPENDENT VARIABLES	MLE		
	REPUBLICANS	DEMOCRATS	BOTH PARTIES
PB_{12}	5.807*	5.335*	5.347*
	(2.479)	(1.746)	(1.383)
PB_{13}	2.188*	3.650*	2.932*
	(1.028)	(.911)	(.647)
PB_{23}	−.567	−3.205*	−1.988*
	(1.289)	(1.182)	(.828)
Constant	1.029*	1.151*	1.087*
	(.0900)	(.0885)	(.0618)
−2xLLF (χ^2_3)	33.03*	71.10*	100.47*
Number of cases	495	475	970

Note: MLE denotes maximum likelihood estimate. PB_{xy} denotes the difference in normalized viability scores times the difference in normalized thermometer evaluation scores given to candidates x and y. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .05$ (one-tailed test of coefficient estimates).

variables can be substantial. For example, if we repeat our example, but with normalized utilities of (1, .7, 0) for the three candidates, the estimates reported in Table 6 yield likelihoods of voting for the highest-rated candidate of .98 and .99 for Republicans and Democrats, respectively, when the three viabilities are (.5, .3, .2). These likelihoods decline to .78 and .70, respectively, with viabilities of (.3, .5, .2). If the utility for the second-rated candidate is .3, the estimated likelihoods are .99 and .999, respectively, for the first set of viabilities and .69 and .68 for Republicans and Democrats for the second set of viabilities. These examples are not extreme, yet the model estimates predict substantial differences in the probability of voting for the first- versus second-rated candidate.[10]

SOME ADDITIONAL EVIDENCE

Differences among Supporters of Different Candidates

We shall explore two further questions that, while not direct tests of the calculus of voting, are important to this study. One set of model estimates examines the possibility that supporters of different candidates employed different decision rules. In particular, the data in Table 4 suggested that Robertson and Jackson supporters might have been sincere, ignoring the viability assessments. Alternatively, they may have been sophisticated voters who so greatly favored their first choice over the alternatives that they had straightforward sophisticated strategies. The second question concerns estimating the extent and consequences of any rationalization of thermometer or viability ratings.

The first question requires us to examine candidate choice by name rather than their position in preference order. Moreover, testing whether supporters of some candidates employed viability assessments in their decisionmaking but others did not requires us to examine the impact of viability and evaluations separately, rather than multiplicatively as in our tests of the calculus of voting. While not a direct test of the calculus of voting, therefore, the results will determine the robustness of our estimates across differing types of candidates and campaigns.

The dependent variable is the candidate the respondents preferred. This is a purely categorical variable in both parties, examining Bush, Dole, and Robertson among Republicans and Dukakis, Gephardt, Gore, and Jackson among Democrats.[11] Therefore, multinomial logit was used for estimation. Since scale effects could be material, the direct, rather than normalized, measures of evaluations and viabilities are used. Thus, the independent variables are the individual candidate feeling thermometer scores and the individual assessments of each candidate's viability. With k candidates, multinomial logit estimates $k - 1$ equations, with the remaining candidate serving as the baseline. Coefficients, therefore, indicate the effect of a variable on increasing the likelihood that the respondent will support the indicated candidate over the baseline candidate. The probability of preferring the baseline candidates (here Bush and Dukakis) is one minus the probability of supporting all other candidates.

The estimates are reported in Tables 7 and 8. The goodness-of-fit statistic for each party is very large. There is also general support for the major hypothesis that vote preference depends on evaluations and

TABLE 7.

Multinomial Logit Estimates of Republican Vote Preference, Given Candidate Evaluations and Assessments of Candidates' Viability

	DOLE	ROBERTSON
Bush		
Evaluation	−.0979*	−.1113*
	(.0133)	(.0189)
Viability	−.0658*	−.0603*
	(.0131)	(.0207)
Dole		
Evaluation	.1068*	−.0509*
	(.0145)	(.0214)
Viability	.0514*	.0470*
	(.0129)	(.0237)
Robertson		
Evaluation	−.00376	.1608*
	(.00747)	(.0256)
Viability	−.000976	.0276
	(.00826)	(.0184)
Constant	.4825	−1.9007
	(.9135)	(1.7344)
$-2xLLF$ (χ^2_{12})	503.33*	
Number of cases	482	

Note: Bush serves as baseline candidate. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .025$ (one-tailed test), $\leq .05$ (two-tailed test).

viability. There are some small differences between the two parties.

The Bush–Dole comparisons are just as expected. The higher the evaluations and the viability assessments of Bush, the more likely the respondent to report a vote preference for Bush; and the higher the scores on these two variables for Dole, the more likely the respondent to support Dole. Evaluations of all three candidates were significantly related to reporting a vote preference for Robertson, although Dole evaluations are most weakly so, as expected. Viability assessments of Robertson were positively, but only weakly and not significantly, related to his support. This suggests sincerity among his supporters; but those with a Robertson vote preference did weigh his opponents' viabilities, Bush negatively and Dole positively. Thus, Robertson supporters were not purely sincere, but weighed his opponents' prospects, consistent with the calculus of voting.

Estimates of the model of Democrats' vote preferences are clear; and there is general support for the hypothesis that supporters of all candidates followed the logic of the calculus of voting, weighing both preferences and viability. The probability of expressing a vote preference for Gephardt and for Gore, compared to Dukakis, fit the expected pattern very closely. The higher the evaluation or assessment of viability for Dukakis, the less likely respondents were to express a vote preference for Gephardt or for Gore. Conversely, the more positively Gephardt or Gore were evaluated and the candidate's viability assessed, the more likely the respondent was to have a vote preference for Gephardt or Gore, respectively. The positive signs on evaluations of Gore and Gephardt in each other's equations suggest that their supporters saw them as near substitutes, with Dukakis as the main alternative.

The comparison between Jackson and Dukakis is generally consistent with the expected utility logic. Evaluations of Jackson and Dukakis perform as expected. The more viable Jackson was seen to be, the more likely respondents were to express a vote preference for him. While the opposite is true for assessments of Dukakis' viability, as expected, the parameter estimate is not significant at the conventional .05 level, although it is nearly so (and is significant at the .1 level). Thus, the conclusion is that the Jackson supporters appeared to reason via expected utility but that the strength of that model, especially in terms of the viability coefficients, is somewhat less. In this sense, there is a reasonable parallel between Robertson and Jackson supporters, where viability assessments are consequential but somewhat more muted than among supporters of other candidates.

Estimation of Projection and Its Consequences

It is often argued that voters might engage in projection, such as in "wish fulfillment," believing that their preferred candidate is more likely to win than others do. Alternatively, there may be "bandwagon" effects, with some voters evaluating winning candidates highly only because they are winning. Bartels (1985, 1988) and Brady and Johnston (1987) provide estimates that indicate that voters in presidential primaries do make such projections. Here, we estimate a simple simultaneous equation model to test for the presence of such relationships between the candidate evaluation and viability measures and examine the consequences of these estimates for our findings.

If either sort of projection occurs, models that fail to take projection into account will yield biased (and inconsistent) estimates. In our case, the results would likely *overestimate* the extent of straightforward voting and *underestimate* the extent of sincere and sophisticated voting. More voters, that is, would report that their highest-rated candidate is the most viable candidate than would be the case with no projection. Thus, fewer would appear to be casting sophisticated or sincere votes. While projection, therefore, should lead our estimates to be conservative, it is important to estimate the extent of projection and its consequences.

We developed a simultaneous equation model with two equations, one for candidate evaluations and the other for viability ratings, and estimated the model by three-stage least squares. The feeling thermometer equation includes two variables in addition to viability assessments. One is a summary measure of the respondent's assessments of the candidates' traits, scored so that a positive coefficient is expected.[12] The second is the distance between the respondent's self-placement and the placement of the candidate on

TABLE 8.

Multinomial Logit Estimates of Democratic Vote Preference, Given Candidate Evaluations and Assessments of Candidates' Viability

	GEPHARDT	GORE	JACKSON
Dukakis			
Evaluation	−.1405*	−.1479*	−.1275*
	(.0295)	(.0275)	(.0267)
Viability	−.0475*	−.0563*	−.0355
	(.0221)	(.0231)	(.0221)
Gephardt			
Evaluation	.0975*	.0364	−.0374**
	(.0251)	(.0221)	(.0209)
Viability	.0844*	.00310	.0272
	(.0230)	(.0172)	(.0172)
Gore			
Evaluation	.0738*	.1329	.0192
	(.0293)	(.0269)	(.0203)
Viability	−.0183	.0398*	.00632
	(.0167)	(.0186)	(.0159)
Jackson			
Evaluation	−.0521*	.00380	.0806*
	(.0177)	(.0157)	(.0188)
Viability	.0282	.00543	.0339*
	(.0174)	(.0149)	(.0143)
Constant	−1.3426	.0781	2.8613
	(1.9148)	(1.7651)	(1.7222)
-2xLLF (χ^2_{24})		270.33*	
Number of cases		192	

Note: Dukakis serves as baseline candidate. LLF denotes Log of Likelihood Function. Standard errors are in parentheses.
* $p \leq .025$ (one-tailed test), $\leq .05$ (two-tailed test).
** $.025 < p < .05$ (one-tailed test).

the seven-point ideology scale, with a negative coefficient expected. We also included a dummy variable for blacks' evaluations of Jackson.[13] These variables are assumed to have no effect on viability ratings.

The viability equation includes the thermometer score and a variable measuring the candidate's standing. This measure was based on actual poll results at the time of the interview and is, therefore, literally exogenous.[14] We also included combined candidate standing with our measure of the respondent's political involvement, on the hypothesis that the more involved are more likely to have more accurate assessments of candidate standing. Positive signs are expected for both of these variables.

We estimated these models for each party and for the three candidates included in our tests of the calculus of voting. The critical questions focus on the impact of the endogenous variables on each other. We also report a system R-squared for a measure of overall fit of the two-equation model. The correlation of error terms between the two equations (labeled the cross-r) provides a general indication of the potential for remaining endogenous relationships unaccounted for in the model.

The estimates of these models are reported in Table 9. The fit of these models is generally adequate, and the cross-equation error term correlations are gratifyingly low. Moreover, the signs of exogenous variables are generally correct and often significant (with the second and third Democratic candidate equations being the principal exceptions).

The critical question concerns the impact of the endogenous variables. The results are fairly clear. Viability assessments have a small—and often insignificant—impact on candidate evaluations, while those evaluations have a consistently large and significant impact on the viability measures. To be sure, viability is significant in the second and third Republican candidate equation; but the coefficient estimates are small (less than one-half the size of the smallest coefficient estimate for the thermometer evaluations). The one large coefficient (in the third Democratic equation) has a very large standard error, indicating unreliability. Moreover, the magnitude of the coefficients typically increases going from highest, to second-highest, to lowest-rated candidate—precisely the opposite of bandwagon reasoning.

Candidate evaluations, however, have large and significant effects on viability judgments. One might have expected, however, that "wish fulfillment" would have been concentrated in assessing the chances of the most highly rated candidate. The estimates for Republicans decline only slightly, moving from highest- to lowest-rated candidate; and they

TABLE 9.

Estimates of Reciprocal Relationship between Candidate Evaluations and Viability Assessments, by Party

CANDIDATE RANKING	REPUBLICANS			DEMOCRATS		
	FIRST	SECOND	THIRD	FIRST	SECOND	THIRD
Evaluation equation						
Viability	−.073	.080*	.189*	.084	−.029	−.835
	(.065)	(.044)	(.071)	(.127)	(.228)	(.575)
Traits	14.990*	14.033*	15.871*	19.979*	15.284*	27.503*
	(1.41)	(1.27)	(1.70)	(2.09)	(2.65)	(8.38)
Ideology	−.985*	−1.473*	−2.406*	−0.485	−1.586*	−2.522
	(.518)	(.492)	(.606)	(.633)	(.696)	(1.58)
Jack. dum.	—	—	—	2.614	3.789	—
				(2.46)	(7.34)	
Constant	34.513*	19.123*	−5.789*	11.292	14.788*	10.132
	(5.11)	(4.56)	(4.54)	(6.66)	(6.20)	(10.5)
Viability equation						
Evaluation	.889*	.650*	.627*	.395*	.661*	.878*
	(.143)	(.115)	(.079)	(.127)	(.128)	(.113)
Standing	6.080*	8.244*	4.757*	−.206	.059	−.057
	(.814)	(.805)	(.854)	(.182)	(.181)	(.164)
Involv*stand	.018	−.055	.395*	.112*	.025	.071*
	(.165)	(.172)	(.185)	(.050)	(.049)	(.046)
Constant	−29.398*	−20.345*	−4.647	28.770*	8.833	9.415*
	(12.5)	(8.04)	(4.10)	(9.74)	(7.71)	(5.17)
Sys. R^2	.233	.350	.368	.177	.202	.151
Cross-r	−.246	−.363	−.495	.059	−.238	.304
	501	450	449	444	427	381

Note: See text for definition of variables. Standard errors are in parentheses.
* $p \leq .05$ (one-tailed test, constant two-tailed test).

move in exactly the opposite direction for Democrats. While this pattern is more reminiscent of response set bias than projection, it nonetheless indicates that we must examine choice in light of projection.

Since projection seems apparent only with viability measures, we computed a set of estimates of viability responses "purged" of the estimated projection of evaluations. These "purged" estimates were determined by computing the predicted viability ratings from the three-stage least squares estimates and subtracting off the projection (i.e., the thermometer score times its estimated coefficient). We used these to compute new ordinal classifications of voter types (comparable to Table 3) and reestimated the tests of the calculus of voting (comparable to Tables 5 and 6). In some respects, estimates of choice controlling for projection provide a test of a more dynamic model, in the sense that they attempt to capture the processes through which the voters reach their decisions.

With the Democratic estimates of wish fulfillment yielding coefficients that actually increased with decreasing ranking of the candidates, the proportion of straightforward voters increased slightly (from 43.8% to 48.3%), the proportion of sincere voters was halved (from 13.1% to 7.2%), and the proportion of sophisticated voters was exactly the same (13.1%). Among Republicans, the heavier discounting of the "purged" viability of the first-rated candidate, due to the large impact of evaluations on those viability assessments, led to a dramatic decrease in the proportion of straightforward voters (down from 52.8% to only 6.0%) and an equally dramatic increase in apparently sincere voting (up from 10.3% to 55.9%) and little change in apparently sophisticated voting (12.7% vs. an original 13.9%). Roughly speaking, the difference is that in the original measures, Bush was often the first-ranked candidate in thermometer and viability scores (i.e., the most preferred and most viable candidate). If we eliminate the effect of evaluations on viability, Bush (or whoever is rated most highly) is no longer the most viable candidate, so that voting for the most preferred candidate is now apparently sincere rather than straightforward.

It is perhaps more important to consider the impact of the "purged" viabilities on estimates of the calculus of voting, since the categorization does not take magnitudes into account and is not a direct test of sophisticated voting. Our results are presented in Table 10. Since the effect of projection is concentrated on the viabilities, we reestimated the models reported in Table 5. These include only probability terms and are therefore more affected by projection than the model estimates in Table 6.[15] It is true that the fit of the models and the estimate of each coefficient are

TABLE 10.

"Purged" Measures of Candidate Viabilities

INDEPENDENT VARIABLES	MLE		
	REPUBLICANS	DEMOCRATS	BOTH PARTIES
P_{12}	1.139* (.271)	1.034* (.231)	.662* (.139)
P_{13}	.745* (.236)	.299 (.224)	.214* (.130)
Constant	1.846* (.170)	.982* (.090)	1.308* (.0585)
−2xLLF (χ^2_2)	29.35*	44.25*	44.79*
Number of cases	503	483	986

Note: Replication of models reported in Table 5. P_{xy} denotes the difference in normalized viability scores given to candidates x and y. LLF denotes Log of Likelihood Function.
* $p \leq .05$ (one-tailed test of coefficient estimates).

smaller than in Table 5. Moreover, P_{13} in the Democratic case is much smaller and no longer statistically significant. Beyond that, however, there is remarkable similarity between these estimates and those based on the original measures. We therefore conclude that while there was a significant projection of evaluations onto viability assessments, corrections for projection seem to overcompensate (Bush *was* the front-runner in 1988). Even so, the corrections only attenuate, but do not reverse, any of our conclusions.

CONCLUSION

Citizens decide how to cast their presidential primary votes in sensible, even rational, ways. Over 80% of the respondents in each party had a vote preference consistent with the assumption of rationality, whether that was a straightforward, apparently sincere, apparently sophisticated, or rational-but-uninteresting type (see esp. Table 4).

In the post-1968 presidential nomination system, candidate fortunes have risen and fallen, sometimes with startling rapidity. Data from the 1988 NES Super Tuesday survey indicate that citizens responded to electoral events (here the Iowa caucuses and New Hampshire primary) by revising their perceptions of candidate viability in ways that corresponded to these events. While their evaluations of the candidates changed, these changes were far more muted. Vote preference changed dramatically, more in line with changing perceptions of viability than candidate evaluations. Moreover, the changes in voter types over the various campaign periods among the supporters of the various candidates further support this conclusion.

Our results further suggest that the changes in vote preferences were due to the rational weighing of the comparative chances of the candidates, along with the less variable (at least in the aggregate) comparative evaluations of the candidates. We tested the multicandidate calculus of voting in three ways. Each test led to the conclusion that both Republicans and Democrats had vote preferences consistent with sophisticated voting. Moreover, the direct tests of the calculus of voting (reported in Tables 5 and 6) showed that the specific parameter estimates were remarkably similar for both parties.

In our final models, in which the hypotheses of sophisticated versus sincere voting could be examined, we found that supporters of most candidates were estimated to have weighed both candidate evaluations and viability assessments. Robertson support was not significantly related to assessments of his chances; but it was significantly related to assessments of the chances of his major opponents. This does not fit neatly into purely sophisticated or purely sincere voting models, but it does provide evidence of strategic reasoning in line with the calculus of voting. Jackson supporters, by comparison, appeared to weigh both his chances and (although with less statistical certainty) those of the comparison candidate, Dukakis. As with Robertson, then, those with a vote preference for Jackson were consistent with the calculus of voting; but (also as with Robertson's supporters) the impact of viability assessments were more muted. Overall, then, most respondents followed the reasoning assumed in the multicandidate calculus of voting.

Respondents do seem to engage in projection, tending to rate more highly the chances of the candidate they like the best. But our estimates indicate that they also rate more highly the chances of their second- and third-ranked candidates. Even when projection effects are taken into account, the overall number of sophisticated voters was affected very little. Like voters in other democracies, U.S. citizens appear to avoid wasting their votes, when they are confronted with a multicandidate campaign in which the wasted vote logic applies.

We have demonstrated that citizens made sophisticated choices in only a single set of primaries—one

that may have provided especially clear cues about the candidates' prospects, particularly among Republican contenders. Further research is clearly necessary both to specify the conditions under which sophisticated voting occurs and to understand better the dynamics of voters' choices up to the moment of their actual voting decisions.

Despite the need for additional research, our findings yield important insights about the democratic process. Amid a nomination process often referred to as chaotic and capricious, our evidence nonetheless suggests that primary voters are not simply fickle or that they jump on bandwagons because they like to support winners. Instead, our evidence suggests that overall, they make reasoned and sensible choices, seeking to support the office seeker who best combines feasibility as a candidate with attractiveness as a potential nominee.

Notes

The data for our analyses were made available by the Inter-University Consortium for Political and Social Research. This research was conducted in part while Aldrich was a Fellow at the Center for Advanced Study in the Behavioral Sciences. He would like to thank them and the National Science Foundation for its support, through grant no. BNS 87-00864. These institutions bear no responsibility for our analyses and interpretations. We would like to thank John Brehm for his counsel and John Geer for his comments.

1. Voters may also consider the candidates' chances of winning the general election. We investigated this possibility in Abramson et al. 1990. The evidence suggests that there is little difference between using measures of viability and using measures of electability.

2. In addition, 1,688 of these respondents were interviewed by telephone after 8 March. For a full description of this survey, see Center for Political Studies 1988.

3. Regardless of whether they planned to vote or not, respondents were asked which primary they would vote in if they did vote. Respondents who said they would vote in the Democratic primary were asked which Democrat they would vote for. Those who said they would vote in the Republican primary were asked which Republican they would vote for. Those who were not sure or did not know which primary they would vote in were asked how they would vote in both primaries. We call this measure *vote preference*, since it was asked even for those who did not intend to vote on 8 March.

4. We use the date of completion of the interview, which may differ from the date the survey was begun, to determine the time period in which an interview was conducted. In this and subsequent tables, we classify voters as Republicans or Democrats based on the primary in which they intended to vote or in which they said they would vote if they voted. We do not base this decision on candidate preference except for those unsure about which primary they would (or might) vote in.

5. We also estimated all of our statistical equations using the original thermometer scores in place of the normalized scores. There were no substantive differences in the estimates.

6. We present these results for all of the subcategories in Abramson et al. 1990, table 5. For example, among those classified as straightforward, we show the proportions who are classified as straightforward by evaluations and viability, by evaluations only, and by viability assessments only. For the distribution of subcategories for Table 4, see Abramson et al. 1990, table 9.

7. We include respondents in our analyses who reported that they did not intend to vote on 8 March and those who, when reinterviewed after that date, reported that they did not participate. There is very little difference between respondents who did, and those who did not, intend to vote and also very little difference between those who reported voting and those who reported not voting (Abramson et al. 1990, app. 2).

8. In addition to multicollinearity among the independent variables, the relatively small numbers of Republicans and of Democrats who preferred their second-ranked candidate limits the ability to absorb many degrees of freedom and produce meaningful estimates (further complicated by the asymptotic properties of probit and logit). Moreover, missing data loom large, especially for viability (and, in later models, utility) measures for minor candidates. See Abramson et al. 1990 for procedures and comparisons of models similar to those reported here and models containing larger numbers of comparisons.

9. Estimation of models that include P_{23} (and thus exclude either P_{12} or P_{13}) yield the hypothesized negative (and statistically significant) coefficient.

10. Since both of these models combine utility and viability measures, it could be argued that only one of them is driving the results. Perhaps, for example, all that matters is utility, not viability (i.e., respondents prefer to support candidates based only on utility considerations, or they back winners regardless of preferences). We tested models using just utility differentials and using just viability differentials. The results were dramatic and clear cut: goodness-of-fit statistics of the alternatives were far smaller than for the model reported in Table 6. In particular, the goodness-of-fit measures for normalized-utility-only models were 12.43 and 2.37 for Republicans and Democrats, respectively, while the comparable models for viability-assessments-only were 8.80 and 1.81, respectively. As these numbers suggest, the appropriate hypothesis tests are easily significant, indicating that for both parties the combined models are far superior to either of the individual models.

11. Too few respondents supported any other candidate except Hart to make analysis feasible. There was substantial support for Hart, but it collapsed after the Iowa caucuses and New Hampshire primary. While his collapsing support is consistent with the calculus of voting logic, including him would increase missing data substantially.

12. The candidate character variable is an average of responses to the eight measures of traits, such as "moral" and "decent," asked about each candidate in the Super Tuesday survey. The average was computed for all responses given, provided that at least six questions were answered. A factor analysis of these measures resulted in each candidate's traits loading on a unique dimension with similar magnitudes for each of the trait responses.

13. This appears only in the equations for the first- and second-highest-rated candidate, because no black included in the estimations rated Jackson third-highest. We considered including such a variable for white fundamentalists' evaluations of Robertson, but there were too few cases for reliable estimates.

14. Respondents were assigned to one of five time periods based on the date their interview was completed. The candidate standing variable for each candidate was an average of opinion poll results and any primary and caucus results in the relevant period. The periods and results used are

1. 17 January–2 February
   Des Moines Register Iowa poll (24 January)
   NYT/CBS national poll (26 January)
   Roper poll (South, 31 January)
2. 3–8 February
   Roper (South, 31 January)
   NYT/CBS national poll (2 February)
   Gallup poll (Iowa, 4 February)
3. 9–16 February
   Roper (South, 31 January)
   NYT/CBS national poll (2 February)
   Iowa caucuses

4. 17–22 February
   Roper (South, 31 January)
   NYT/CBS national poll (22 February)
   Iowa caucuses
   New Hampshire primary
5. 23 February–8 March
   NYT/CBS national poll (22 February, and South 4 March)

These objective measures of standing were drawn from the *New York Times*, except for the Gallup Poll which was taken from the *Gallup Opinion Report*, and applied to all candidates except Gore. Gore's standing was based only on the national and southern polls, because he did not compete in the Iowa caucuses or the New Hampshire primary.

15. We also reestimated those models; and the effect of projection and its correction via "purging" are less in those models than in those reported in the text. These estimates are available on request.

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POLITICAL PARTIES AND ELECTORAL MOBILIZATION: POLITICAL STRUCTURE, SOCIAL STRUCTURE, AND THE PARTY CANVASS

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As agents of electoral mobilization, political parties occupy an important role in the social flow of political communication. We address several questions regarding party mobilization efforts. Whom do the parties seek to mobilize? What are the individual and aggregate characteristics and criteria that shape party mobilization efforts? What are the intended and unintended consequences of partisan mobilization, both for individual voters and for the electorate more generally? In answering these questions we make several arguments. First, party efforts at electoral mobilization inevitably depend upon a process of social diffusion and informal persuasion, so that the party canvass serves as a catalyst aimed at stimulating a cascading mobilization process. Second, party mobilization is best seen as being environmentally contingent upon institutional arrangements, locally defined strategic constraints, and partisan divisions within particular electorates. Finally, the efforts of party organizations generate a layer of political structure within the electorate that sometimes competes with social structure and often exists independently from it.

One of the major good works of political parties is to engage citizens in the political process. Parties play an important role in democratic politics when they mobilize the electorate to turn out and vote, thereby involving citizens in democratic governance. These efforts on the part of parties are not unbiased attempts aimed at encouraging diffuse system support: they carry an explicitly partisan message. But in the necessarily free market of democratic electoral competition such self-interest is a virtue because it serves to educate and inform the electorate in a politically meaningful manner. Modern treatments of U.S. political parties often focus upon parties as formal secondary organizations that publish platforms, endorse and support candidates, and provide officeholders with a framework for coalition formation (Mayhew 1986). At the same time, it is important to remember that as agents of electoral mobilization, parties occupy an important role in the social flow of political communication. Thus, inevitably, party politics and partisan efforts at electoral mobilization include a heavy dose of social influence. When a party worker knocks on a citizen's door, calls a citizen on the telephone, or affronts him with a yard sign, an effort is being made by one individual to provide information that will influence the behavior of another individual.

Moreover, the motivation for such contact resides in a set of implicit assumptions regarding how voters make their political choices. Successful efforts at voter mobilization do not enlist party supporters in single-file fashion. Even at the height of the so-called machine era in party politics, jobs, favors, and assistance were not distributed on a tit-for-tat basis (one job for one vote). Such an enterprise would have bankrupted even the wealthiest party organization. Rather, party efforts at electoral mobilization inevitably depend upon a process of social diffusion and informal persuasion. If a party worker can succeed in convincing Sally to vote Democratic, then Sally might be able to convince Bill, and so on. The investment of party resources becomes more potent to the extent that the votes of individuals are interdependent because the initial contact has cascading consequences in the collective deliberations of democracy.

We ask three questions regarding the role of party organizations as agents of mobilization in contemporary U.S. politics.

1. Whom do the parties seek to mobilize, and what are the individual and aggregate characteristics and criteria that shape party mobilization efforts?
2. What are the intended and unintended consequences of these mobilization criteria? Are the parties able to identify and contact appropriate portions of the electorate?
3. What impact do party mobilization efforts have upon citizens who are contacted? Does this impact reach beyond the point of contact to affect the electorate more generally?

Before turning to these questions, we address several antecedent issues related to the environmentally contingent nature of organized partisan efforts at electoral mobilization.

PARTIES AND MOBILIZATION

Any analysis of partisan mobilization in the context of U.S. politics must address a seeming discrepancy: at the same time that political scientists have developed a general skepticism regarding the mobilization potential of party organizations, party activists continue to invest heavily in the effort. Indeed, the reported incidence of party contacts among the electorate is higher now than it was in the early 1950s (Krassa

1985), a period of time during which political parties are generally thought to have been more vibrant.

Skepticism regarding the mobilization potential of party organizations is rooted in several lines of argument. Many scholars and journalists agree that party organizations have weakened dramatically over time. From this perspective, it is difficult to see how party mobilization efforts could be important. Further, viewed from the perspective of national surveys, party contacts are seen as having generally consistent but modest effects on turnout, with little or no effect on the direction of the vote (Kramer 1970–71; Krassa 1988, 1989).

How can the discrepancy be understood? Why would party organizations continue to invest so heavily in undertakings that yield so modest a reward? First, it is important to remember that even modest effects in a statistical sense may be consequential to the outcome of an election, thereby making party efforts worthwhile. Second, the demise of party organizations, particularly at the local level (Gibson et al. 1983, 1985) may have been seriously overstated. Third, most analyses that point toward weak parties are narrowly focused to exclude candidate-centered organizations—perhaps the dominant form of partisan electoral organization in U.S. politics. If we include the mobilization efforts of candidate-centered organizations as party activity—a wholly appropriate practice in the organizationally fractured world of U.S. political parties (Sorauf and Beck 1987, chap. 1)—locally organized party efforts can be quite substantial. Finally (and perhaps most importantly), more recent efforts at understanding party mobilization are typically undertaken from the vantage point of the national sample survey. But is it appropriate to judge the impact of party mobilization efforts based simply on the votes of contacted respondents? Or should we look beyond the reports of individual voters to examine the environment of party mobilization as it affects both the strategy and the consequence of mobilization efforts?

THE ENVIRONMENT OF PARTY MOBILIZATION

The efforts of parties and their candidates to mobilize the electorate are located geographically and socially within the context of particular structural and institutional settings. Political party organizations in Indiana face a different set of opportunities and constraints than party organizations in California or Illinois (Jewell 1984; Jewell and Olson 1982). Some of these constraints are legal and institutional. The advent of the direct primary revolutionized the conduct of party politics; but the revolution occurred along very different lines in different settings, depending upon the particular shape of the reform. These reforms, coupled with later national reforms, produce a synergism with truly profound consequences that are still unfolding (Shafer 1983, 1988). Electoral reforms in many states were designed with the explicit purpose of creating an environment hostile toward organized parties and their efforts, effectively divorcing electoral participation from partisan involvement (Hawley 1973). In other states an environment was created that proved to be much more congenial to organized party politics, maintaining a crucial and visible role for party organizations and partisan attachments (Bledsoe and Welch 1987).

It is not only the various legal and institutional settings of U.S. electoral politics that create the unique environments of party mobilization. As Beck (1974) and others have shown, party mobilization efforts must also be seen within the existing partisan balance of particular locales (also see Hofstetter 1973; Miller 1956; Putnam 1966). It is a common observation that U.S. electoral politics has become nationalized with a corresponding breakdown of regional electoral patterns; but the case for nationalization is too easily overstated, at least as it relates to one-party dominance in state and local politics. In their 1974–80 analysis of party competition within the states, Bibby and his colleagues (1983) classify only 22 states as being two-party-competitive. Moreover, one-party dominance within particular locales is even more widespread. The increased willingness of the U.S. voter to engage in split-ticket voting has meant that local one-party politics can survive even when one-party control runs in a direction opposite that of state and national trends. Indeed, Gibson and his colleagues (1985) show that the organizational vitality of state political party organizations is independent of the organizational vitality of local political party organizations. Hence, it is commonplace for U.S. voters to live, for example, in Democratic cities that lie within Republican states. Configurations such as these will be shown to have important political consequences.

Both environmental features loom especially large with respect to the present analysis: (1) the institutional environment of party activity, particularly as it affects the availability of information that parties might use in their mobilization efforts; and (2) the partisan balance that exists within a particular locale. Both features are part of the inescapable context within which party efforts are formulated, and both have substantial consequences for the strategies of party organizations.

THE RESEARCH SITE: SOUTH BEND, INDIANA

We address the strategies and consequences of partisan mobilization through the analysis of a 1984 election study conducted in the South Bend, Indiana metropolitan area. South Bend proves to be an advantageous study site for several reasons. First, the city possesses a political environment that is relatively congenial to political organization and the conduct of partisan politics (Stabrowski 1984). While

the state of Indiana does not possess a closed primary system, voters go on record as party supporters when they choose a party's primary ballot, and party organizations are able to identify the voters who participate in their primaries.[1] Second, the Democratic party has traditionally possessed an advantage in South Bend politics. Thus, we are able to examine the organizational and institutional implications of majority and minority standing. Third, the study of partisan mobilization is hampered by an exclusive focus on presidential politics; but important gubernatorial and congressional campaigns were also being waged in South Bend during the fall of 1984, and we will consider partisan mobilization with respect to these campaigns as well.

The argument we develop does not depend upon the selection of South Bend as a study site, and a skewed partisan environment does not make South Bend atypical of other U.S. cities. The short answer to those remaining skeptical is that everyone lives in a South Bend. The compelling reality of urban and suburban politics today is that almost all of us live and work as part of political majorities or minorities because few environments maintain a genuine political balance. Our neighborhoods are tilted one way, our workplaces another, and our cities perhaps another. The aggregation of micro environments to the typical levels of party organization is unlikely to create politically competitive units. Indeed, the continuing efforts of political elites are aimed at avoiding just such competition. But just because the partisan bias of an environment is highly skewed, it does not follow that party organization and political mobilization become irrelevant. A major proposition advanced here is that important and systematic political consequences flow from these normal and typically skewed partisan distributions.

The South Bend study included a three-wave panel survey of approximately 15 hundred respondents, where the second and third waves were supplemented with new respondents to offset panel attrition. All interviews were conducted through the use of computer-assisted telephone interviewing at the Center for Survey Research of Indiana University in Bloomington. Our analysis relies most heavily on information taken in the third, postelection wave. Two features of the South Bend study deserve special attention for purposes of this analysis. First, the 15 hundred respondents were chosen to be equally distributed across 16 neighborhoods. Respondents were randomly selected within the neighborhoods, but the neighborhoods were chosen purposefully to maximize social status variation across the neighborhoods and to minimize variation within the neighborhoods. All the neighborhoods were predominantly white in racial composition. We thus have random samples of more than 90 respondents in each neighborhood, which means that the analysis can operate at two levels. Any question included in the survey can be used as an individual measure, or it can be aggregated to provide a neighborhood measure.

Second, the survey information is augmented by public record data regarding voter registration and primary participation for individual citizens. This information is used to determine (1) whether respondents are registered to vote, (2) whether they voted in past elections, and (3) which party's primary ballot they requested when voting in a primary election. Thus, we can consider the citizens' self-reported behavior relative to the official record of the behavior (Katosh and Traugott 1981; Traugott and Katosh 1979), and all this within a particular political and social setting.

In summary, we aim at the analysis of a particular party system, particular party organizations, and a particular institutional setting. What relevance does such an analysis have for U.S. politics more generally? A major weakness of modern research on parties and party systems is that it is typically and implicitly predicated on the unsustainable myth that the relationship between parties and voters can be understood *apart* from a particular setting. Our argument lies in an exactly opposite direction: the relationship between parties and electorates is environmentally contingent and must be understood in that light. Only by appreciating the relationship between parties and voters in South Bend (and Los Angeles and Schenectady and Walla Walla) can we begin the task of making meaningful statements in more general terms (Boudon 1986).

THE STRATEGIC BASIS OF THE PARTY CANVASS

Perhaps the most important form of encounter between parties and citizens (and certainly the most widely studied) is the party canvass—the effort of party workers to contact individual citizens. About one-third of our South Bend respondents report being contacted by at least one of the parties, with 22% reporting contact by each of the parties and 10% reporting contact by both of the parties.[2] If party organizations are successful at targeting their potential supporters for contact, we should expect to see a strong negative relationship between the likelihood of being contacted by the Democrats and by the Republicans. But the South Bend data demonstrate a level of mutual contacting (.10) that is double the proportion that would be expected by chance alone ($.22 \times .22 = .046$), and this raises important issues: Whom *do* each of the parties contact? What criteria *do* the parties use in deciding whom to contact? Are the parties ignorant, or merely naive?

The official list of registered voters within a particular governmental jurisdiction is the most readily available source of information that most parties can use in their efforts to mobilize voters. State-to-state variations in voter registration arrangements are enormous, but in many states the record-keeping operations of the registrar provide a valuable source of information for political party efforts. Registration records identify the citizenry which is currently eligi-

TABLE 1

Ballot Chosen in the Primary Election by Self-Reported Partisanship, 1982–84 (%)

	DEMOCRAT		INDEPENDENT			REPUBLICAN	
PRIMARY	STRONG	WEAK	DEM.	NO PARTY	REP.	WEAK	STRONG
1984							
Democratic	57.6	45.0	46.9	16.2	20.9	10.7	5.6
Republican	.0	.9	1.1	2.9	9.3	16.7	27.0
No vote	42.4	54.1	52.0	81.0	69.8	72.7	67.4
Number of cases	335	220	179	105	215	150	267
1983							
Democratic	53.4	40.4	33.5	20.0	19.1	10.0	7.9
Republican	.0	1.8	1.1	1.9	12.1	12.7	31.1
No vote	46.6	57.7	65.4	78.1	68.8	77.3	61.0
Number of cases	335	220	179	105	215	150	267
1982							
Democratic	40.6	32.7	33.0	12.4	13.0	6.0	3.8
Republican	.3	.4	.6	4.8	9.3	13.3	25.8
No vote	59.1	66.8	66.5	82.9	77.7	80.7	70.4
Number of cases	335	220	179	105	215	150	267

ble to participate in an election. In many states, registration records provide indirect evidence regarding the partisan loyalties of citizens. In some states voters must register with a particular party in order to participate in that party's primary. In other states, such as Indiana, voters do not register with a party, but go on record as party supporters when they request that party's ballot at a primary election.

How useful is this information as a guide to the predicted political behavior of a voter? Participation in St. Joseph County primaries is a less than perfect indicator of partisan loyalty (see also Finkel and Scarrow 1985). More significantly for the conduct of electoral politics, the choice of a party's ballot is a *biased* measure of partisanship because an asymmetrical relationship exists between partisanship and primary ballot choice. The dominance of the Democratic party in local politics creates a situation in which the winner of the Democratic primary is frequently the winner of the general election as well, and thus the stakes are often higher in the Democratic primary (Farquharson 1969; Key 1949). Table 1 combines (1) post-election survey data regarding partisan loyalty with (2) public record data regarding primary participation in the three previous primary elections, and thus we can compare partisan behavioral records across self-reported subjective loyalties. Table 1 shows that many Republican loyalists vote in Democratic primaries, but Democratic loyalists seldom vote in Republican primaries. While this sort of cross-party primary voting is substantial, Table 1 also shows that it has not reached epidemic proportions; thus, knowing which ballot a voter takes at a particular election still provides a useful indicator of partisan preference.[3] At the same time, ballot choice is a biased indicator; and the bias has important consequences for political organization and partisan mobilization.

CRITERIA FOR PARTY CONTACT

To what extent does information regarding primary participation affect the likelihood that a voter will be contacted by a party? We employ public record data regarding cumulative primary participation histories to address this question among our respondents. These cumulative histories provide information on as many as eight previous primaries for individuals who are registered to vote, and this information is used to determine frequency of participation and ballot choice among our respondents.

Among respondents who have never voted in a primary, approximately 25% report being contacted during the campaign. In contrast, the rate of reported contact increases to about 40% among other respondents. Not surprisingly, party organizations focus their contacting efforts on their own primary voters. Of those who only vote in Democratic primaries 32% report being contacted on behalf of Democratic candidates, and only 19% report a Republican contact. Among Republican primary voters, 34% report a Republican contact, and only 15% report a Democratic contact. Significantly, the Democrats demonstrate a rate of contacting citizens who have voted in both parties' primaries (23%) nearly equal to the Republicans rate (29%), even though Table 1 shows that such voters are almost entirely Republican in their professed political sympathies. It would appear that the Democrats are either misperceiving, miscalculating, or unable to repair a situation in which they contact voters who are likely to be Republican loyalists.

What other information might the party organizations use in their contacting efforts? South Bend politics is structured by local geography in some very pronounced ways. For example, the proportion of our survey respondents voting for Reagan within the

16 sampled neighborhoods varies from .18 to .59; and the proportion voting for Mondale varies from .14 to .38.[4] Political strategists within the city are certainly aware of these patterns; thus, it is reasonable to expect that they use zone of residence as an indicator of likely political sympathies. Such a measurement device is especially useful because it allows the party to organize its efforts on a geographic (door-to-door) basis, rather than on the basis of a list of individuals. But how should parties decide *which* neighborhoods might provide a fruitful focus for their activity? Several criteria might be used: (1) they might select neighborhoods where the turnout for the party's primary was heaviest; (2) they might select neighborhoods that are high (or low) in terms of social status; (3) they might have complex, historically idiosyncratic reasons for selecting particular neighborhoods; and so on.

We have experimented with a variety of different aggregate measures that might potentially explain the individual incidence of party contact within our sample. The results of these efforts are shown in the top of Table 2, which displays logit models for the probabilities that a respondent is contacted by each of the parties. Party contact by a particular party is the criterion variable. The explanatory variables are cumulative participation records in Democratic and Republican primaries, and the neighborhood property that was found to be most influential in explaining each party's contacting efforts. The magnitudes of effects predicted by these models are shown in Figure 1. As this figure shows, the probability of Republican contact among our respondents is enhanced both by Republican primary participation and by living in a neighborhood with a higher turnout in the Republican party primary. Several neighborhood properties provided nearly equivalent statistical purchase in explaining Republican contact—the closest competitor being the mean level of educational attainment within the neighborhood. In other words, citizens are more likely to be contacted by the Republican party if they participate in Republican primaries. Independent of their own participation, they are also more likely to be contacted if they live among others who vote in Republican primaries; and this tends to occur in higher-status neighborhoods.

The Democrats exhibit a different pattern of contacting, at least with respect to aggregate neighborhood effects. Figure 1 shows that those who participate in Democratic party primaries are more likely to be contacted by the Democrats; but the only neighborhood property that helps to explain Democratic contact is simply the proportion of neighborhood respondents who report being contacted. Thus, people who live in neighborhoods with high levels of Democratic contact are more likely to be contacted, even after the individual criteria for contacting are taken into account.

Is it tautological to say that people living in high-contact areas are more likely to be contacted? We have removed the respondent's own experience when calculating the proportion of neighborhood respondents who were contacted by the Democrats; thus, the relationship is not tautological. For example, if a respondent lives in a neighborhood where 20% of the people were contacted by the Democrats, this means that 20% of all *other* respondents living in the neighborhood were contacted. Indeed, the individual probability of being contacted by the Republicans is *not* significantly affected by the level of neighborhood contacting after the individual contacting criteria are taken into account.

In one sense, this account of Democratic contacting is not entirely satisfactory, because it does not provide a rationale for the higher incidence of contacting in some neighborhoods. For some reason (perhaps related to traditional areas of Democratic strength; see Stabrowski 1984), the Democratic party tends to concentrate its resources within particular neighborhoods. At the same time, our inability to explain this concentration of effort is revealing in another respect: it shows that *political structure,* as well as social structure, is a significant element in political affairs. Political outcomes cannot be explained wholly in terms of class and status; they must also be explained in terms of party. Indeed, in the sense we are defining it here, political structure provides a separate level of meaning and experience that is imposed upon social structure (Key and Munger 1959).

THE EFFECTIVENESS OF THE PARTY CONTACTING CRITERIA

So far, we have addressed the factors that serve as criteria for party contact—information that the parties use to locate and mobilize their supporters. This information provides only an indirect measure of partisan orientation; but it is the best that the parties can do, because it is, in essence, all the information that is available. In other words, the party is not able to interview all the voters prior to contacting them! If they *were* able to conduct such interviews, they might obtain a more direct measure of partisan orientation that left less room for doubt regarding political intentions.

While such information is unavailable for the entire electorate, it *is* available for the respondents who participated in the survey. Thus, we can assess the success of each party in locating its supporters. The bottom of Table 2 replicates the analysis of the top but includes individual-level controls for self-reported partisanship (party identification) and individual educational attainment, allowing us to address the question, To what extent do the contacting criteria serve as valid indicators of party support? If the introduction of a control for each respondent's partisanship diminishes the explanatory power of the contacting criteria, then the contacting criteria can be assumed to be doing a good job for the party.

The introduction of these controls produces very different consequences for the analysis of Democratic

TABLE 5

Neighborhood Ratio of Democratic Signs and Stickers to Republican Signs and Stickers by Republican and Democratic Contact in the Neighborhood

EXPLANATORY VARIABLES	COEFFICIENTS
Constant	1.46 (2.52)*
Nghbd. proportion contacted by Democrats	8.03 (4.55)**
Nghbd. proportion contacted by Republicans	−9.05 (5.23)**
R^2	.80
Number of cases	16

* $p \leq .05$, two-tailed test.
** $p \leq .01$, two-tailed test.
Note: Least squares models. Coefficient t-values in parentheses.

occur as the result of both party contact and the opposite party's signs and stickers. If Republicans perceive themselves as a minority in South Bend—and particularly if they perceive themselves as a minority within a neighborhood—then they may behave more strategically than Democrats, as the rate of primary crossover voting already indicates (Farquharson 1969). In particular, confronted by excess Democratic yard signs and bumper stickers, they may well contact their known supporters to put up yard signs in response. In short, we may be seeing further evidence of the value that accrues to the political minority as a result of intense party activity (Katz and Eldersveld 1961).

The distribution of yard signs and bumper stickers reflects the complex interplay of partisan support, party strategy, and party competition. While we do not possess the precise menu that yields the aggregate result, we are able to identify many of the crucial ingredients. They are displayed in Table 4, where the ratio of Democratic to Republican signs and stickers is regressed on the levels of Democratic and Republican contacting in the neighborhoods.

Quite clearly, party activity makes a decided difference: 80% of the variation in the ratio can be explained in terms of party effort.

CONSEQUENCES OF PARTY EFFORTS

What are the consequences of party mobilization efforts for the citizens who are contacted? Do organized partisan efforts have results which extend beyond the point of contact? These are crucial questions that speak directly to the role of political parties in democratic politics and to the potential for a politically organized electorate. We address them here in the context of extensive previous work showing (1) generally moderate but consistent effects upon levels of turnout and (2) mixed evidence regarding effects on the direction of the vote (Crotty 1971; Cutright 1963; Cutright and Rossi 1958; Gosnell 1927; Katz and Eldersveld 1961; Kramer 1970–71; Krassa 1988, 1989; Pimlott 1972, 1973; Wolfinger 1963).

As we have seen, South Bend party organizations tend to pursue contacts among citizens who have voted in previous party primaries and thus have a record of party support. This is an important fact to take into account when judging the individual consequences of party contact. South Bend primary voters participated in the subsequent general election at an extraordinarily high rate. More than 98% of respondents who voted in the primary also voted in the general election. In contrast, nonparticipants in the primary were as likely to vote as to stay home at the general election. Thus, if an analysis ignores the high level of participation among primary voters, the importance of party contacting might easily be exaggerated because party contacting is keyed to primary participation.

In keeping with the importance of primary participation, the consequences of party contact are analyzed separately among primary election voters and nonvoters. The effects of party contacts upon turnout, presidential voting, gubernatorial voting, and congressional voting are considered among primary election nonvoters in the top of Table 6. In column 1, nonvoting is the reference condition, and the coefficients predict the probability of voting in the general election. In the remaining columns, voting for the Democratic candidate is the reference condition, and the coefficients predict the probability of voting for the Republican candidate. Thus, we can use the first model to consider the effect of contact upon turnout. And for those who *do* vote in the general election, we can employ the other models to consider the effect of contact on the two-party division of the vote. The top of Table 6 shows, at most, weak effects due to party contacting among nonvoters in the primary election. Both Republican and Democratic contact fail to generate statistically discernible effects at even the 95% level of confidence for any of the voting behaviors, although the respective coefficients all lie in the expected directions. In general, the more impressive effects in these four models are due to self-identified partisanship[7] and, in the case of turnout, to individual and contextual education.

The estimated effects of individual and contextual education on turnout (based on Table 6, column 1), with all other variables held constant, are shown in Figure 3. Respondents with higher levels of schooling are more likely to vote, regardless of the educational level of the surrounding neighborhood population. Conversely, respondents who live among a more highly educated population are more likely vote, regardless of their own educational levels (Tingsten 1963). Recall once again that the top of Table 6 is estimated on the basis of respondents who did not vote in the previous spring's primary election. Political structure has little effect on general election turn-

TABLE 6

Voting Choices in the 1984 General Election

EXPLANATORY VARIABLES	TURNOUT	REPUBLICAN		
		PRESIDENT	GOVERNOR	REPRESENTATIVE
Primary nonvoters				
Constant	−3.16**	−.51	−4.91*	−3.87
Dem. Identifier[a]	−.49**	−2.77**	−1.84**	−2.17**
Rep. identifier[a]	.26	2.10**	1.96**	1.45**
Age	−.02**	.02	.01	.02
Educ.[b]	.09*	.01	.02	−.06
Nghbd. educ.[c]	.24**	.08	.32	.31*
Dem. contact in nghbd.[d]	−.78	−.61	1.69	−.76
Rep. contact in nghbd.[d]	−.89	.36	−.03	.40
Dem. contact[a]	.35	−.73	−.12	−.44
Rep. contact[a]	.23	.19	.27	.33
Number of cases	853	373	374	374
Voters in the primary				
Constant	—	.70	−2.49	−.42
Dem. identifier[a]	—	−1.35**	−2.14**	−1.69**
Rep. identifier[a]	—	4.13**	1.70**	2.23**
Age	—	−.01	.00	.00
Educ.[b]	—	.04	.03	.06
Nghbd. educ.[c]	—	−.06	.14	−.14
Dem. contact in nghbd.[d]	—	.44	2.64	.59
Rep. contact in nghbd.[d]	—	.33	−.60	2.87
Dem. contact[a]	—	−.49	−.85**	−.83**
Rep. contact[a]	—	.46	.62*	.10
Number of cases	—	539	557	552

Note: Logit models.
[a]Dummy coded.
[b]Years of school.
[c]Mean school years.
[d]Proportion contacted.
*$p < .05$, two-tailed test.
**$p < .01$, two-tailed test.

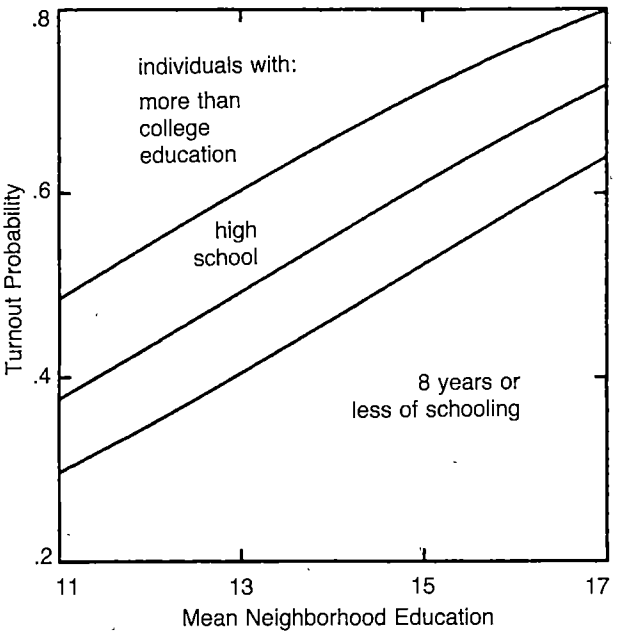
out among these individuals lying outside the boundaries of conventional party involvement. In contrast, social structure has a decided effect upon whether they vote. For the other respondents (those who *did* vote in the primary election), the level of participation in the general election is so high that *both* social structure *and* party contact are rendered irrelevant. This is not to say that partisan contacts and social structural influences are immaterial to their initial involvement, but the focus of this paper is necessarily upon the activation process during the campaign, and primary voters did not need to be activated.

If party contacting is immaterial to general election turnout among primary voters, does it have any effect upon the two-party division of the vote? The bottom of Table 6 shows that, at least in terms of the presidential vote, the contacting coefficients for party contact lie in the substantively appropriate directions but fail to satisfy the 95% level of confidence. A very different situation occurs with respect to the gubernatorial and congressional elections. In three of four instances, the coefficients not only lie in the expected directions but also satisfy at least the 95% level of confidence. Both of these elections produced highly visible campaigns. The incumbent Republican governor took advantage of a significant statewide organization to ensure his reelection; and South Bend's popular and well-known Democratic county prosecutor mounted an aggressive but unsuccessful challenge against the incumbent Republican congressman.[8] These results, which show an effect due to party activity in subnational elections, are in keeping with a stream of research documenting the importance of parties and party activities particularly at the state and local level (Caldeira, Clausen, and Patterson 1990; Caldeira, Patterson, and Markko 1985; Katz and Eldersfeld 1961; Patterson and Caldeira 1983; Wolfinger 1963).

How large are the estimated effects due to party contacts? Table 7 uses coefficient estimates from the bottom of Table 6 to illustrate the effect of party contact upon the predicted probability of a Republican gubernatorial vote, across categories of party identification. The effect of the partisan contact is especially pronounced among independents and Republicans. This is not to argue that contacting is always important, only that it can be. And these results suggest that it is more likely to be important in state and local contests, among the citizens who participated in party primaries.

Figure 3

Turnout Probability in the General Election by Individual and Neighborhood Education, for Nonvoters in the Primary Election



Note: Age is held constant at 45 years; neighborhood proportions contacted by Democrats and Republicans are held constant at .10; partisanship is held constant at *independent*; and Republican and Democratic contact are held constant at *no contact*. Based on estimates from Table 6.

Thus, the irony of party contacting deepens. In terms of turnout, politically marginal citizens (i.e., who failed to vote in the primary election) appear to lie beyond the reach of partisan organization. Conversely, politically engaged citizens (who vote in party primaries) are overwhelmingly likely to vote in the general election *whether they are contacted or not*. The effectiveness of organized partisan effort is most pronounced among citizens who vote in the primaries, but the effect lies in altering the two-party division of the vote in state and local contests. Thus, it would appear that partisan contact is less important as a vehicle to get out the vote than as a means of swaying the preferences of voters who are already likely to vote.

We could end the analysis here with the conclusion that party contacting only has an effect on the direction of the vote among some citizens in some contests. But does such an analysis exhaust the possibilities? Are we looking in the right places to understand the consequences of partisan mobilization? Should we only view the party canvass as a direct instrument of mass mobilization enlisting voters one by one to the party's cause? Or should we also view the canvass as a catalytic agent that stimulates voter mobilization by enlisting additional party workers in the effort?

OTHER FORMS OF ELECTORAL INVOLVEMENT

Voting in an election is only one form of electoral involvement that is susceptible to the influence of party mobilization efforts. A whole range of gladiatorial activities are central to the electoral process and dependent upon party organizational activities. We consider four of these activities here, based upon the following questions asked in the third, postelection wave of the survey.

1. Did you work for any candidate in this election?
2. Did you go to any political meetings, rallies, dinners, or things like that?
3. Did you put up a political yard sign or bumper sticker during the campaign?
4. Did you give any money to a political party or candidate?

Each of these activities considered singly is, of course, a rare event when compared with the frequency of voting. But taken together they point toward involvement that is perhaps surprisingly widespread. A summary index is formed by counting the number of activities in which each respondent reported participating. A score of zero indicates no involvement in any of the activities (71% of the third-wave respondents), and a score of four indicates involvement in each of the four activities (2% of the third-wave respondents). Thus, nearly 30% of the

TABLE 7

Predicted Republican Proportion of Two-Party Vote for Governor in General Election among Primary Voters, by Party Contact and Party Identification

PARTY IDENTIFICATION	CONTACTED BY			
	DEMOCRATS	REPUBLICANS	NEITHER	BOTH
Dem. identifier	.04	.15	.08	.07
Rep. identifier	.65	.89	.81	.77
Independent	.25	.59	.44	.38

Note: All other variables are held constant at intermediate values: age = 45 years, educ. = 12 years, nghbd. educ. = 12 years, nghbd. Dem. contact = .10, nghbd. Rep. contact = .10. Based on estimates from the bottom of Table 6, col. 3.

TABLE 8

Partisan Electoral Involvement As a Function of Various Factors, Including Party Contact

EXPLANATORY VARIABLES	COEFFICIENT	t-VALUE
Constant	−.24	.77
Dem. identifier[a]	.24	4.38**
Rep. identifier[a]	.21	3.42**
Voted in Dem. primary[a]	.07	1.25
Voted in Rep. primary[a]	.30	3.48**
Dem. contact[a]	.24	4.08**
Rep. contact[a]	.19	3.26**
Dem. contact in nghbd.[b]	−.44	1.22
Rep. contact in nghbd.[b]	1.19	2.92**
Indiv. educ.[c]	.04	3.98**
Nghbd. educ.[d]	−.02	.86

Note: Least squares model. N = 1,439; R^2 = .08.
[a]Dummy coded.
[b]Proportion contacted.
[c]Years of school.
[d]Mean school years.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

sample reported engaging in at least one of these activities.

As Table 8 suggests, the structure of political life looms large with respect to these other forms of electoral involvement. In particular, party contacts play an important role; but there is also evidence to suggest that partisan identification, primary participation, and the level of party contacting within neighborhoods (at least, by Republicans) may make a difference, as well.[9] In contrast, while the effect of individual education is substantial, the neighborhood educational level has no effect at all. Unlike the case of general election turnout among nonvoters in the primary, political structure goes much farther than social structure in explaining levels of activation and involvement.

SECOND-ORDER CONSEQUENCES OF THE PARTY CANVASS

The party canvass has substantial consequences for activating the activists, but does it serve to stimulate a more widespread mobilization process? One opportunity to address this question lies in examining the consequences that derive from simple, grassroots partisan activity—displaying a political yard sign or bumper sticker. Does the aggregate distribution of yard signs and bumper stickers have any measurable consequence for the individuals who are exposed to them? At the second wave interview, during the heart of the campaign, we asked our respondents a question regarding which presidential candidate they thought would win the most votes in their neighborhood. We also asked whether they thought it would be a close race, so that a four-point scale can be constructed measuring the respondents' expectations regarding voting behavior among neighborhood residents.

How might partisan activity alter these expectations? In Table 9, column 1, the expectation regarding the neighborhood is regressed on respondents' preelection candidate preferences and the neighborhood distribution of yard signs and bumper stickers; and the results show that the distribution of yard signs and bumper stickers is quite clearly related to neighborhood expectations. The problem is, of course, that while Democratic yard signs and bumper stickers *do* tend to be located in Democratic neighborhoods, they are not the only ways in which residents of a neighborhood form judgments regarding the political tendencies of other residents. We have no method for sorting out the extent to which signs and stickers are responsible for these judgments, but we can examine the extent to which respondents' expectations are misinformed—the extent to which yard signs and bumper stickers have effects that are not attributable to the actual distribution of support.

The respondents' expectations are considered once more in Table 9, column 2; but controls are included for the *actual* distribution of votes in the respondents' neighborhoods. Our strategy is to reconsider yard signs and bumper stickers but to subject their effect upon respondent perceptions to a control for reality. Do visible manifestations of support continue to affect respondent perceptions once we simultaneously take into account the reality upon which the perceptions are based? It must be emphasized that this is a very stringent test. Our new question becomes, Do yard signs and bumper stickers have an effect upon perceptions of support when we control the actual levels of support upon which the perceptions are based? Thus, we are ignoring yard signs and bumper stickers as vehicles for communicating the reality of these support levels and focusing instead upon their potential to produce alternative perceptions of the same underlying reality.

This new test is accomplished by aggregating the reported votes of respondents from the third-wave, postelection survey within each of the neighborhoods. Treating the resulting measures as the actual distributions of support within the neighborhoods, we are able to isolate the independent informational effect due to visible demonstrations of support—the yard signs and bumper stickers. A single measure for the distribution of signs and stickers has been constructed providing the difference in the proportions of neighborhood residents displaying Democratic versus Republican yard signs and stickers (proportion Democratic minus proportion Republican). As the table shows, this variable measuring the distribution of yard signs and bumper stickers produces a statistically discernable effect with an appropriate sign for neighborhood expectations.

Contextual theories of politics often assert that citizens surrounded by a particular preference are more likely to adopt the preference. The empirical support for such an argument is substantial and widespread (e.g., Segal and Meyer 1974). But the

TABLE 9

Expectations of Election Outcomes in Neighborhood by Respondent's Preference and Neighborhood Yard Signs and Bumper Stickers, with and without Controls for Actual Levels of Support in the Neighborhoods

EXPLANATORY VARIABLES	FOR ACTUAL SUPPORT LEVELS IN NEIGHBORHOODS: NO CONTROLS	CONTROLS
Constant	3.41 (32.63)**	2.24 (8.28)**
Mondale supporter[a]	−.41 (4.97)**	−.36 (4.51)**
Reagan supporter[a]	.15 (1.91)	.11 (1.42)
Dem. signs and stickers[b]	−22.54 (14.13)**	—
Rep. signs and stickers[b]	8.48 (10.75)**	—
Nghbd. Mondale vote[b]	—	−1.92 (3.26)**
Nghbd. Reagan vote[b]	—	2.77 (8.34)**
Partisan diff., signs and stickers	—	−4.11 (4.88)**
R^2 =	.29	.36
Number of cases	939	939

Note: Ordinary least squares model. Coefficient t-values are in parentheses.
[a]As is the case with the dependent variable, these dummy variables are measured at the second wave of the survey, prior to the general election. Only the neighborhood variables are taken from the third, postelection wave of the survey. Only respondents participating in both the second and third waves are included in this table.
[b]Neighborhood proportion.
[c]The partisan difference in signs and stickers is measured as the neighborhood proportion with Democratic signs and stickers minus the neighborhood proportion with Republican signs and stickers.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

TABLE 10

Individual Party Identification As a Function of Neighborhood Partisanship and Other Individual-level Factors

EXPLANATORY VARIABLES	FOR PRESIDENTIAL VOTE CHOICE: NO CONTROLS	CONTROLS
Constant	−.68 (1.55)	.09 (.24)
Indiv. educ.[a]	.10 (3.60)**	.08 (3.29)**
Family income[b]	.15 (4.11)**	.09 (3.00)**
Age	.01 (2.08)*	.01 (2.75)**
Ethnic attachment[c]	−.45 (3.99)**	−.30 (3.18)**
Union member in family[c]	−.71 (5.54)**	−.25 (2.28)*
Protestant[c]	.90 (7.95)**	.63 (6.56)**
Nghbd. party ID[d]	.36 (3.89)**	.26 (3.34)**
Mondale voter	—	−1.66 (14.12)**
Reagan voter[c]	—	1.31 (11.47)**
Number of cases	1,326	1,285
R^2	.20	.46

[a]Years of school.
[b]Range, 1–8.
[c]Dummy-coded.
[d]The respondent's own party identification is excluded in the calculation of the neighborhood mean for that respondent.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

enduring critique of such an argument is an alleged failure to document the mechanisms whereby the aggregated preferences of a population are translated into a source of influence for individuals (Hauser 1974). We supply one part of the response to this critique: an important mechanism of contextual influence is party effort. People know their neighbors' politics, in part because of party organization aimed at informing them.

Viewed somewhat differently, party organization is designed to exploit social organization. Table 10, column 1, displays a regression of party identification among the South Bend respondents on a range of different measures: education, income, age, the strength of the respondent's ethnic attachment,[10] union membership, religion, and the partisan composition of the respondent's neighborhood. As this regression shows, individual partisan orientations are subject to the influence of the individual's partisan surroundings, even when we include controls for a wide range of individual level characteristics (Hauser 1974). In particular, the mean partisanship of the neighborhood has important consequences for the partisanship of individuals who live within the neighborhood. Even when we take account of short-term factors affecting partisanship (Franklin and Jackson 1983) by controlling for presidential vote choice in Table 10, column 2, the effect of the partisan context on individual loyalties is still substantial. In short, individual partisanship is contingent upon partisan surroundings. Party organization is not the only mechanism that ties individual preference together with the politics of the larger community (Huckfeldt and Sprague 1987, 1991); but it certainly is one.

These data demonstrate that party activity matters. The seemingly ordinary act of putting up a yard sign sends a message to others who are exposed to the sign. But the yard sign is only one part of a more complex, highly structured process of political mobilization. Organizations make contact with potential

activists who, in turn, make contact with the population at large. Huge amounts of slippage occur along the way. Organizational contacts often fall on deaf ears; or, still worse, misdirected contacts serve to mobilize supporters of the other party. But the significance of the contact does not end once the contact is made. Rather, the contact serves as a catalyst that sets into motion a series of events that in the end has important consequences for ordinary citizens—concerned bystanders who pass judgment on the electoral contests of democratic politics.

CONCLUSION AND IMPLICATIONS

How should we view the efforts made by party organizations to mobilize the electorate? Most importantly, as long as we focus primarily upon the direct individual consequences of party contacts for voting behavior, we will continue to see the party as playing a marginal role in U.S. politics, and we will consistently underestimate the consequences of party activity. Kramer's (1970–71) work on party contacting deservedly became a watershed event in the analysis of party mobilization efforts; but it also led to a subtle but important redefinition of the expected consequence that derives from party activity. An earlier tradition of analysis (perhaps represented best by Katz and Eldersveld 1961) addressed the aggregate pattern of party activity and its consequence for the aggregate distribution of party support. Kramer's analysis turned toward an individual-level analysis of these phenomena, an approach that was readily embraced by social scientists with a heightened sense of skepticism regarding any aggregate data.

The problem remains: partisan mobilization is best understood at two levels of analysis and meaning—the individual and the aggregate. In the general context of U.S. politics, it is unreasonable to expect that party contacts should produce dramatic consequences for the voting behavior of those individuals who are contacted. Rather, party contacts are most important as a means of mobilizing potential activists. Most people do not give money and put up yard signs unless someone asks them to do so; thus, we see a catalytic function of party activity. Party organizations mobilize the faithful, and the activity of the faithful sends a message to the rest of the public.

It is ironic that political scientists have become so skeptical regarding party activity and its consequences. If party activity is so ineffective, why do parties and candidates continue to invest their resources in the activity? Gibson and his colleagues (1983, 1985) investigate the vitality of party organizations at both the state and local levels and show that state and local party organizations started to become more active during the 1960s and 1970s. Krassa uses national survey data to show that the incidence of party contacting has generally increased during the post–World War II period (1985, 5–6). It is possible, of course, that academics understand something that activists have missed; but in this instance it is doubtful.

There are, of course, limits upon the ability of party organizations to mobilize the electorate; and the limits are more pronounced today than they were 50 years ago. The effectiveness of Mayor Daley (the elder) and his precinct runners and precinct captains was genuinely impressive; but organizations such as these have always been the exception rather than the rule. Absent such an organization (which presents an impossible standard of effectiveness), we should not assume that political organization is absent.

Finally, the biggest challenge to any political organization is strategic—developing methods of mobilization that are efficient and effective. As we have seen, numerous obstacles inhibit this sort of effectiveness; and the majority party is perversely vulnerable to its own special strategic constraints. The lesson to be drawn from this analysis is that the parameters determining the relationships between parties and voters, and thus determining the success of partisan mobilization, are environmentally contingent. They cannot be understood from afar.

Notes

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1. As a matter of law, Indiana voters are not entitled to take the primary ballot of a political party unless they are supporters of that party. Indeed, voters are subject to challenge if they request the ballot of a party they do not support. As a practical matter, such challenges are virtually nonexistent; and few people (including the officials who are responsible for conducting primary elections) are even aware that they might be issued. Thus, voters *might* request different primary ballots at different elections.

2. The text of the contacting question was, "As you know, the political parties try to talk to as many people as they can to get them to vote for their candidate. Did anyone from one of the political parties call you up, or come around and talk to you about the campaign this year? Which party was that?"

3. For example, two-thirds of the respondents who voted in the 1984 Democratic primary report voting for Walter Mondale; and 94 percent of the respondents who voted in the 1984 Republican primary report voting for Ronald Reagan.

4. All measures of the vote used in this paper are validated with registration data. Respondents who do not appear on the registration list are counted as nonvoters. Respondents who appear on the registration list *and* who reported voting for Mondale or Reagan are counted accordingly.

5. A methodological possibility is that partisanship works well for the Republicans because much of partisan identification is a simultaneous function of vote choice. This alternative interpretation seems unlikely in light of the very strong education effect for Republican contacts. Whatever else this class measure may proxy, it is certain that vote does not determine education. Substantive, rather than methodological, interpretations are more persuasive.

6. In order to determine whether a respondent displayed a Democratic or a Republican yard sign or bumper sticker, their presidential vote is used as an indicator. For example, we assume that self-reported Reagan voters who also report displaying a yard sign or a bumper sticker did in fact display a Republican sign or sticker. They might, of course, have

displayed a sign or a sticker for a Democratic candidate running for some other office. Indeed, in view of the election results, where many Democratic identifiers voted for Reagan, we probably overestimate Republican signs and stickers and underestimate Democratic signs and stickers. The resulting bias is not likely to compromise our analysis to any serious extent. Split-ticket voters are less likely to display signs and stickers; thus, the bias is not likely to be profound. Indeed, these data show that (using our measure) less than 1% of self-identified Democrats put up a Republican sign or sticker and that not a single self-identified Republican put up a Democratic sign or sticker. Alternatively, none of the Democratic signs or stickers were put up by Republicans; and only 6% of the Republican signs and stickers were displayed by Democrats.

7. In light of work showing reciprocal causation between vote choice and party identification measured as a seven-point scale (Franklin and Jackson 1983), we have restricted our measure of partisanship to three categories: Democratic, Republican, and independent (the baseline category). This practice is based on the assumption that people are much more likely to shift the strength of their attachment within categories than they are to switch their actual attachment during the campaign—an assumption that is sustained by the South Bend data. For a more detailed discussion of these issues relative to the South Bend study, see Huckfeldt and Sprague 1991.

8. The incumbent congressman, John Hiler, was one of House Speaker Tip O'Neill's "Reagan robots," originally winning office in the 1980 election. He had only narrowly hung on to his seat in the 1982 midterm election, but in 1984 he took advantage of Reagan's coattails to win more easily (Mondak 1990).

9. An alternative analysis of these data was undertaken in which electoral involvement was truncated to a binary variable: those who had participated in *any* of the activities versus those who had participated in *none* of the activities. This variable was, in turn, the criterion variable in a logit regression with the same explanatory variables as in Table 7. The results were wholly consistent with the Table 7 analysis.

10. The strength of a respondent's ethnic attachment is measured with the question, "How important would you say your ethnic background is to you?" Possible responses are *very important* or *somewhat important* (coded one) and *not too important* (coded zero).

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THE STRATEGIC AGENDA IN LEGISLATIVE POLITICS

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We examine the politics of the strategic agenda. Abstracting a politics on the liberal–conservative dimension, we analyze Key Vote roll call data from the U.S. House of Representatives during the Eisenhower and Reagan administrations. The data suggest that politicians set the policy agenda in a strategic fashion. Because they consider such factors as long-term political goals, the changing institutional setting, and plebiscitary presidential politics, agenda-setters propose legislation that only imperfectly reflects their and the membership's wishes on the issue at hand. Thus, as the final stage in the political process, the strategic selection of an agenda provides a means by which factors other than policy preferences affect policy outcomes. The analyses affirm the strategic agenda as a core element in political life.

During the Summer of 1982, in response to mounting public concern over the nuclear arms race, Liberals in the U.S. House of Representatives proposed an immediate Nuclear Freeze (HJ Res 521). Conservatives coalesced around a counter-proposal that called for a reduction in nuclear weaponry to parity, thus endorsing the Reagan position that the U.S. weapons deficit would have to be erased in any arms control agreement. During a fortnight of debate and politicking, the Liberals discovered that they needed to make concessions to Moderates in order to attract votes. Accordingly, they accepted amendments that would weaken the link to SALT II, emphasize the goal of establishing nuclear equivalence with the Soviet Union, encourage the pursuit of arms reduction, and provide for on-site verification. Conservatives considered blocking these "sweeteners" in the hopes that the original proposal would be unacceptable to Moderates. They ultimately decided to refrain, largely because their supporters found the provisions appealing "on the merits." In the end, the Liberal concessions proved inadequate when the House adopted the Conservative substitute by a razor-thin 204–202 margin.

In the next Spring, Freeze proponents sponsored a similar resolution (HJ Res 13), this time agreeing to some 30 additional amendments to attract the support of Moderate and Conservative members. (The amendments allowed U.S. weapons buildup and further missile deployments prior to formal treaty ratification, provided for weapons modernization for deterrence or for "safety," exempted missile-launching submarines and aircraft, made on-site inspection and verification a *sine qua non*, linked the freeze to subsequent arms reductions, and conceded that the resolution did not compel Presidential action.) Having thus isolated the opposition by conceding almost every substantive point, the Freeze leadership prevailed 278–149. *During subsequent news conferences, both sides claimed victory.*

—(Adapted from *Congressional Quarterly Almanac)*

When we understand the politics of a particular contest, we need know the stakes, as well as the outcome. Often as not, the way that the alternatives are defined determines the result. Accordingly, practicing politicians make the definition of the alternatives (the political agenda) an integral part of their political strategy. Moreover, that strategy (and thus the agenda and the policy outcome) will naturally reflect considerations other than preferences about the policy matter at hand. It is in this sense that we wish to posit—and illustrate—the importance of the strategic agenda in legislative politics.

That agendas are strategic[1] has important implications. First, those who initiate proposals (agenda-setters) will tailor the policy content to have a chance to win. Thus, because the stakes of the game are free to be defined by the players, the nominal outcome of a legislative battle (who wins, who loses) masks, rather than reveals, matters of real consequence.

Second (and more significantly), the strategic character of agendas indicates the importance of the strategies politicians choose. These strategies will vary, perhaps in complicated ways, as politicians pursue different goals, consider institutional constraints, and take advantage of political opportunities; that is to say, factors independent of policy content will affect the way the agenda is set. From the point of view of democratic theory, the crucial result is that the connection between policy and preferences will depend on how politicians craft agendas to serve their own purposes.

We shall show that legislative agendas are strategic. To do this we build an empirical model with parts taken from a theoretical view of agenda politics. Each component generates a different empirical test, which we then confront with roll call data. The evidence reveals that factors other than policy preferences affect the way politicians formulate policy proposals and thus that preferences do not govern outcomes. The fundamental result affirms the strategic agenda as a core element in political life.

A STYLIZED VERSION OF LEGISLATIVE POLITICS

Consider first a single dimension for political conflict, the liberal–conservative continuum. While this characterization of politics may have little to do with logic or philosophy, it has the advantage of being recognizably dominant by politicians and political observ-

ers (Kritzer 1978). Because it represents the major way of seeing political coalitions and conflict, our focus on it as the political battleground should move us forward in understanding how legislative politics are structured. To be sure, much of what goes on cannot be characterized in such simple liberal–conservative terms. We shall be constantly aware that other factors may affect political strategy and political action. However, for purposes of analytic leverage, we shall keep such factors in the background, as unspecified.

The problem is a classic. Say that a liberal legislative committee wishes to propose a health care policy for a floor vote in the House of Representatives. How should the committee blend factors such as cost, accessibility, scope, and so forth? In our stylized one-dimensional space, how liberal or how conservative should the mixture be? When the committee expects the House to ratify the committee recommendation, then the answer is trivial—as liberal as the committee wants.

Things get more interesting when House action becomes political. The committee must trade off its preferences for a liberal policy content against the prospects of losing a floor vote and diminishing its political reputation; its choice will depend on the mixture of goals the committee seeks. It further must consider the institutional rules and norms and patterns of behavior that affect its ability to define the issue and to build a supporting coalition, as well as the opposition's willingness and capacity to organize behind counterproposals. Finally, the committee will want to think about the possibility that outside actors (notably the president) will enter the fray, change the scope of conflict, and transform the political situation. These sorts of factors will affect the committee's proposal—the agenda on which the membership is to vote. The calculus is strategic.

If we want to tell whether agendas are truly strategic, we need empirical data on agendas, and we need to know whether they are a function of such factors as politicians' goals, institutional structure, and political opportunities. We turn to data.

THE EMPIRICAL ESSENTIALS

To study agenda politics, we require a set of political issues that attracted enough political attention to merit the sorts of strategic thinking in which we are interested. We should also like to have issues that were sufficiently controversial to generate an actively engaged opposition so that we might observe strategic politics in a contested field. Finally, we should like to identify the particular roll calls on which the issue was actually decided (perhaps a procedural vote), rather than content ourselves with votes on either trivial amendments or on the sometimes consensual final passage. In short, we seek evidence on politics in its most sophisticated form.

We have chosen to analyze the politics of a set of *Congressional Quarterly*'s Key Votes. Of course, no solution will be perfect. Nevertheless, by selecting these items, we concentrate on issues that produced politics of white heat, that commanded the attention of the national press and of all political actors in Washington (Shull and Vanderleeuw 1987). These are not parochial fights for pork or turf. Relying on the staff of *Congressional Quarterly* means that we are trapped in their definition of what constitutes a crucial political test or a major national issue. Yet it is just the contemporary judgment of such keen observers that enables us to select issues that satisfy our need.

In the end, we have analyzed the politics of the Key Votes taken during the Eisenhower and Reagan administrations. Superficially, both the 1950s and the 1980s were characterized by popular conservative Republican presidents opposed by an organized and savvy Democratic House leadership. Importantly, then, these two eras provide us with sustained examples of divided government. Apart from the obvious contemporary interest in such government, the pattern allows us to observe strategic interactions when both congressional parties had a sharp interest in the politics of governing rather than in the politics of opposition. Further (and crucial for our work), the two periods are politically distinct. First, inside the House, we observe political patterns from very different eras, ranging from the concerted oligarchy of Sam Rayburn to the decentralized brokerage of Tip O'Neill. Second, we observe a contrast in the extent to which presidents mobilized public support in order to seek legislative success (compare the relative indifference of Eisenhower to the energetic efforts of Reagan). Thus, our choice of periods produces a contrast in both internal and external politics while maintaining similarity in the overall lay of the political land.

Further, we have limited our analytic frame to a single policy dimension.[2] At the cost of representative accuracy, this decision allows us to examine a large number of political issues. Having multiple cases proves crucial for our work because we intend to test, rather than illustrate, theoretical propositions.

In making this decision, we understand that we shall miss much of real substantive interest and that much of what we say will appear simplistic to the practitioner's eye. We surely appreciate the complexities of politics, the subtleties of the art. Our reading of the legislative histories here confirms our natural respect for the ability and inclination of politicians to play sophisticated, not to say Byzantine, politics. But we are also convinced that our representation captures, albeit incompletely, an integral part of everyday politics. Just as the nuclear freeze proponents and opponents calculated the sorts of policy concessions and maneuvers that might enhance their chances of success, so do most politicians most of the time. We suspect that the setting of the legislative agenda is neither idiosyncratic nor casual; we suspect that it is systematic and strategic.

We have chosen to concentrate on the single dimension employed by the Americans for Democratic

Action (ADA) in their annual ratings of individual members of Congress. We use ADA, rather than (say) the American Conservative Union (ACU), because their ratings extend to the beginnings of our time frame. This ADA dimension corresponds nicely with the liberal–conservative dimension that so dominates dimensional analyses of roll call voting (Poole and Daniels 1985; Poole and Rosenthal 1987; Weisberg 1978). Further, its terms are readily recognized by practicing professionals and correspond to representatives' and staff members' subjective judgments about the political landscape (Smith, Herrera, and Herrera 1990).

We gain empirical purchase here by scoring individual legislators in terms of their "era-specific" ADA ratings, that is, their average ratings during the Eisenhower or Reagan years.[3] The ADA score represents a member's voting tendency (in liberal–conservative terms); it is a measure of regularized voting patterns. As such, the member's ADA score reflects the combination of factors that establish his or her standing voting decision, including the political composition of the home district, the long-term influence of Washington-based political forces, and (of course) personal ideology.

We are thus able to identify interesting political battles and to define a common policy space that is immediately understandable. Further, both the selection of cases and the definition of the policy space are exogenously determined; that is to say, we take politics on its own terms (as understood in the Washington community) at the very outset.

MEASURING THE DECISIVE AGENDA

The main empirical difficulty lies in measuring just how far bill sponsors have gone in trading off the substantive content of the proposed legislation against the prospects of victory. We need a consistent measurement of the extent to which agenda-setters have trimmed their sails or raised their spinnakers.

We use a simplified version of the Poole–Rosenthal algorithm (Poole and Rosenthal 1985). The idea is based on a straightforward spatial model: each proposed bill (or amendment) may be represented by a point in our liberal–conservative domain. Similarly, the alternative (the current status quo or whatever legislators see as the likely outcome if the proposed bill fails) may also be so represented. A line (or, more generally, a plane) bisects the distance between the proposal and the alternative to provide an *indifference point*.

By way of example, consider the top line of Figure 1. Here a liberal committee has proposed a bill at 50 on a 0–100 ADA scale. The alternative lies at 40; thus, the indifference point is 45. Any legislator whose ideal point lies at 45 (a moderate conservative) will be indifferent between the bill and the alternative. All legislators to the liberal side will prefer the bill, while all those to the conservative side will prefer the alternative.

In real life, of course, legislators do not obligingly conform to academic theory. Let us say that a legislator at 60 votes *nay* on the roll call. Such an individual will have voted "unexpectedly" for any number of reasons. Perhaps the legislator's preferences are poorly measured by ADA, perhaps the bill's implications are ambiguous, perhaps the decision involves policy dimensions other than liberal–conservative, perhaps side payments are involved, or perhaps the legislator simply makes a mistake. In our health care example, a liberal legislator may choose to vote *nay* after meeting a delegation of health insurance lobbyists and a conservative may vote *yea* because the new policy would open a clinic in his home district.

The empirical question is, Can we uncover the policy mixtures of the bills having only the roll call records? With the data at hand, we think the placement of bill and alternative beyond our reach. However, we *can* get a clean measure of the indifference point. When the sponsor of the bill alters its policy content, he or she simultaneously moves the indifference point on the liberal–conservative continuum. More directly, this indifference point is the politically decisive part of the equation.

Consider the example again. If every legislator voted according to our unidimensional theory, then uncovering the indifference point would be easy. Observing the idealized voting pattern in line 2 of Figure 1, we would simply follow along the liberal–conservative scale until we discovered the point at which legislators shift from *yea* to *nay*.

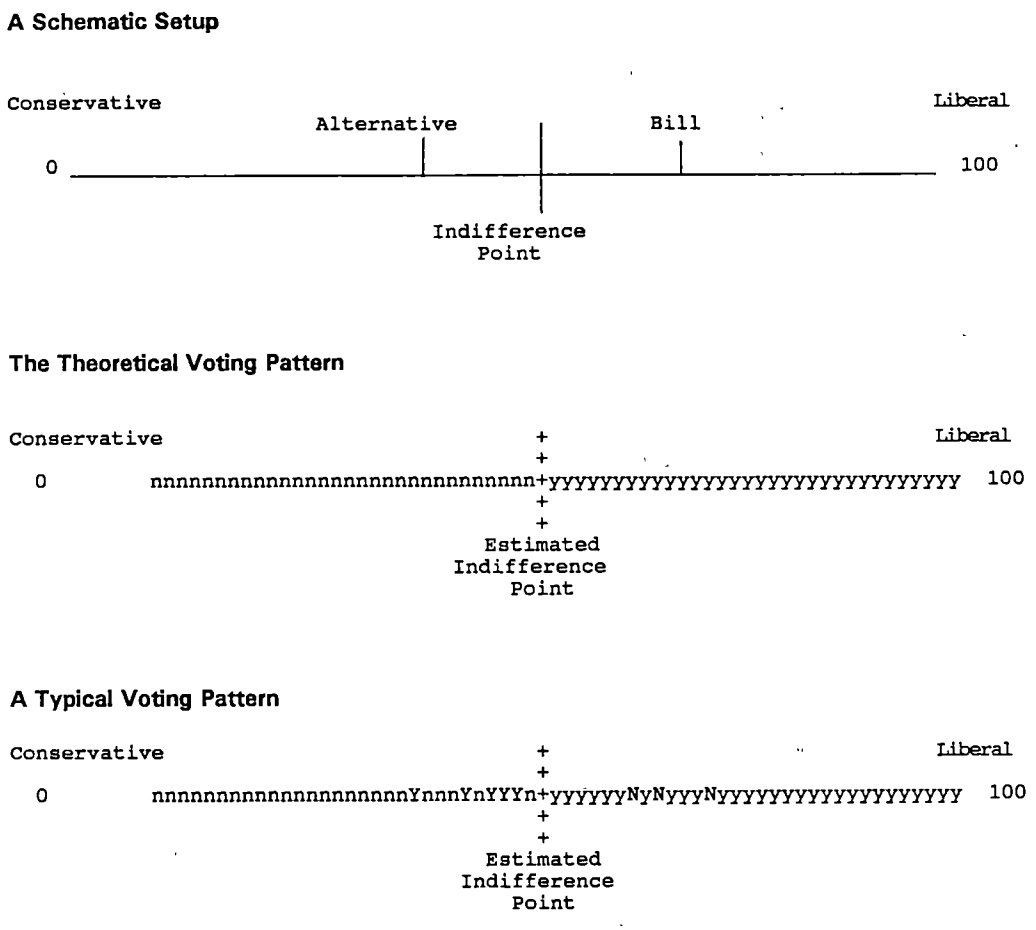
The actual data, of course, look much more like line 3 of Figure 1. Here, several members have chosen to vote in ways not predicted by the one-dimensional abstraction. Given these aberrations, we must develop an "error theory." Here, we simply assume that these extra considerations (having to do with lobbying and partial payoffs) are minimal; that is to say, we choose the indifference point that minimizes the "errors" associated with a liberal–conservative voting model, the one that requires the least amount of extraneous politicking. Technically, we employ an iterative estimation procedure that searches the policy space for the indifference point minimizing the amount of "error" in voting.[4] More fundamentally, we produce an indifference point estimate by assuming that all voting is governed by liberal–conservative considerations plus minimal unspecified disturbances. In the end, we are able to estimate the Key Vote indifference points within about two ADA points on the hundred-point scale.

EVIDENCE ON STRATEGIC AGENDAS: AN EMPIRICAL MODEL

Given the data, we need an empirical model of agenda politics that is grounded in a theoretical view. For this purpose we seek a powerful yet spare representation of congressional agenda strategy. Step by step, we shall construct and test our model using four

Figure 1

One Dimensional Voting and Estimating Indifference Points



elements taken from established theory: (1) the dominance of the median voter equilibrium, (2) the goals of the political actors, (3) the institutional setting in which contests take place, and (4) the effect of the president's actions on political opportunities. We begin with a model of what occurs when agendas are not truly strategic.

The Null Model: The Median Voter Equilibrium

The most elementary part of our model incorporates preference distributions into agenda formation. Of course, the legislators' individual preferences, by themselves, do not necessarily define a collective preference. In fact, much theoretical work (notably, that inspired by Arrow 1963) has shown the inherent difficulty in translating individual choices into collective choices. Thus, no matter how clever, legislators cannot pick policy proposals that will be sure to win a majority vote (McKelvey 1976).

Nevertheless, legislators press on, unencumbered by general theory: they work with models that work "well enough." We, too, wish to proceed using a model that captures reality "well enough." We concentrate on politics played *mainly* on a one-dimensional field, one that runs from the liberal to the conservative pole. Since such a politics incorporates Black's (1958) assumption of single-peaked preferences, it yields a powerful theoretical prediction: majority voting produces the outcome preferred by the median voter. This basic result is most famously illustrated in Downs's (1957) spatial model of party platforms and party competition and in Riker's (1962) theory of minimal winning coalitions.

In our bare-bones model, we expect that politics on the House floor will center around the median House member; that is to say, congressional agenda-setters, intuitively understanding Black's result, will propose policies that attract the support of the median member. Consider liberal agenda-setters. If they should nominate a "sincere" liberal proposal, they will find that the opposition will quickly introduce a (winning) counterproposal to the right of the median.[5] A single-play agenda contest thus punishes agenda-setters who start with sincere positions.

To be sure, contests over the agenda are not single-play but will include proposal, counterproposal, countercounterproposal, and so on. In theory,

agenda-setters who propose policies far from the median will generate a sequence of successful amendments, each closer to the median member's preference. In practice, agenda-setters will anticipate such moves and propose policies already near the expected final result. The implicit sequential game converges quickly.[6]

The question is, Do real politicians take this logic into account? Figure 2 presents the graphic evidence on the point. The vertical dimension represents the passage of time (sequencing the key votes) and the horizontal dimension shows the ADA scale (0 being the most conservative and 100 the most liberal). The vertical hashes in the center of the diagram represent the median member's policy preference (in ADA units) for each Congress. Each circle represents the indifference point for a given bill. Note that the placements do center around the median House member positions. Note also the substantial scatter.

Clearly, the policy preference of the median member changed from one Congress to the next. The rise and fall of each party's electoral fortunes affected the policy preferences of the House as a collectivity. The biggest shifts come from the Democratic victories associated with the recessions of 1958 and 1982. Liberal Democrats replaced conservative Republicans, and the changeover moved the center of the political landscape to the left.

This historical movement provides us with analytic leverage, as we may see if the bill placements change with the median preference. When the House moves to the left, so should the placements of proposals. Table 1 gives the first-cut empirical evidence: the first column presents the regression of each bill's indifference point on the ADA score of the median House member at the time of the bill's introduction. Included are 158 bills from the Eisenhower and the Reagan years. The F-test ($p = .01$) clearly indicates that political actors considered the changing composition of the House when they chose their agenda strategies. The coefficient estimate (.39) indicates that bill sponsors adjusted to 39%[7] of the change in the median preference. The median voter, or minimal winning coalition, expectation is confirmed.

The overall fit disappoints. However, agenda-setters do not always manipulate their policy proposals toward a winning position. Sometimes, because they see longer-run political advantages or perhaps because they believe their policy proposal must not be compromised, agenda-setters may "stick to their guns." For example, just before the 1954 congressional elections, the Democratic Leadership backed an enhanced-benefits amendment to the administration's unemployment compensation bill (a straight New Deal move), knowing full well that they had no chance at victory. In 1959 the Eisenhower administration pushed a raise in the ceiling for long-term Treasury bonds (private long-term rates were above the ceiling, and the government could not borrow), again knowing that they had no realistic chance. In both cases, the agenda-setter knew that the proposal could not pass but pursued the course nevertheless.

Our median voter model should operate most powerfully when both sides "play the game." Consider, instead of the entire set of roll calls, those that were closely contested, say, those decided by margins of 80 or fewer votes (a vote of 250 to 170 or closer).[8] For these competitive votes the median voter model does considerably better: note the rightmost column in Table 1. Now the agenda-setters "adjust" to 58% of the change in the median preference. Further, the simple model now "accounts for" 22% of the variance in indifference point placement. Clearly (in closely contested political battles at least) agenda-setters incorporate the collective distribution of preferences into their strategic calculations.

If congressional politics produced only median voter outcomes, then we should merely cite Black (1958) and close up shop. Obviously, we observe outcomes that suggest we consider additional theoretical factors.

The Mixture of Objectives

The elementary median voter result ignores the agenda-setters' motives. To move forward, we introduce two complications: (1) we allow members' preferences for policy proposals to be affected by factors other than liberal–conservative policy, and (2) we permit political actors to pursue multiple goals when they propose legislation.

The unidimensionality assumption is too restrictive. Instead, we posit that any individual member's preference over policy proposals is defined *primarily* by the proposals' placements on a liberal–conservative dimension but also by other considerations. Here, we wish to be vague. These other considerations might include policy factors that cannot be properly termed liberal or conservative, or they may include matters extraneous to the content of the legislation. In general, we shall refer to these as *side payments*, meaning to include all inducements that might cause members to change their votes on a given matter. In our model, this factor has the effect of introducing uncertainty into the calculus: a voter's preference is a function of liberal–conservative policy plus unknown, unspecified factors.

More interesting, members of Congress have different types of career objectives and choose agenda strategies accordingly. Legislators seek victory in the electoral arena, formulate "good public policy," and pursue personal political power (Fenno 1973, 1978).[9]

When proposing legislation, agenda-setters must choose. On the one hand, they might put forward a policy that reflects an ideal point (perhaps a personal preference but more likely a policy that has political payoffs); on the other hand, they might pick a policy that has a maximum chance of winning a majority vote. Politicians pursue different strategies to pursue different goals. (For convincing evidence, see Poole and Smith 1986.) Yet the trade-off is not easy.

Agenda-setters motivated primarily by electoral politics, provocateurs, will emphasize symbols over substance. Concerned with the broad array of public

Figure 2

Bill Indifference Points

Eisenhower Administration

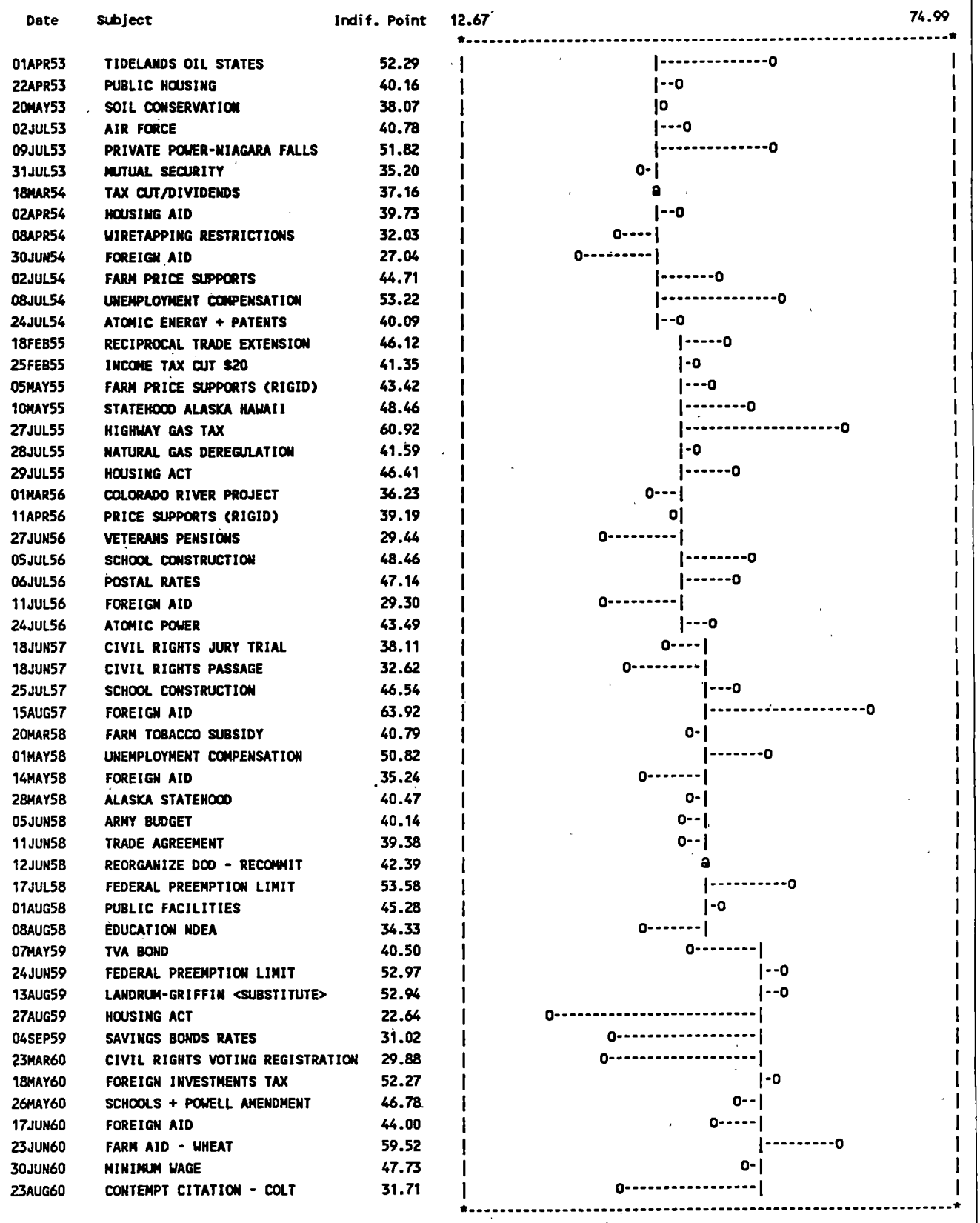


Figure 2

Bill Indifference Points (Continued)

Reagan Administration

Date	Subject	Indif. Point
07MAY81	BUDGET LATTA SUBSTITUTE	55.22
18JUN81	LEGAL SERVICES	20.49
26JUN81	RECONCILIATION - LATTA	42.20
23JUL81	TOMBIGBEE	42.83
24JUL81	CLINCH RIVER	41.58
29JUL81	TAX CUT	49.00
31JUL81	SOCIAL SECURITY	57.57
05OCT81	VOTING RIGHTS ACT	19.54
06OCT81	HHS, EDUCATION, LABOR	26.44
14OCT81	AWACS TO SAUDI ARABIA	18.55
18NOV81	B-1 BOMBER	62.53
18NOV81	MX MISSILE	59.09
11DEC81	FOREIGN AID	36.74
16DEC81	FARM PRICE SUPPORTS	41.64
12MAY82	HOUSING MORTGAGE SUBSIDY	15.29
27MAY82	MEDICARE AND DEFENSE	37.26
10JUN82	BUDGET - LATTA SUBSTITUTE	41.29
22JUL82	CHEMICAL WEAPONS	35.34
28JUL82	BUDGET RECONCILIATION	43.22
05AUG82	NUCLEAR FREEZE - START	40.50
29SEP82	SOVIET SANCTIONS	44.07
01OCT82	BALANCED BUDGET AMENDMENT	47.63
06DEC82	HIGHWAY & TRANSIT	42.61
07DEC82	MX MISSILE	28.23
14DEC82	CLINCH RIVER	36.73
14DEC82	RADIO LIBERTY & VOA	38.63
15DEC82	DOMESTIC CONTENT - AUTOMOBILES	40.11
16DEC82	CONTEMPT CITATION - GORSUCH	20.73
09MAR83	SOCIAL SECURITY	53.90
23MAR83	BUDGET RESOLUTION 1ST	44.82
04MAY83	NUCLEAR FREEZE	26.12
11MAY83	HOME LOANS & HOMELESS	47.62
28JUL83	CONTRA AID	45.36
27SEP83	COAL PIPELINE	44.92
28SEP83	LEBANON FORCE - STAY	57.23
01NOV83	MX MISSILE	52.36
08NOV83	EDUCATION APPROPRIATIONS	36.74
09NOV83	DAIRY PRICE SUPPORTS	45.87
10NOV83	TELEPHONE ACCESS CHARGE	31.37
15NOV83	CHEMICAL WEAPONS	32.75
02FEB84	CHILD ABUSE	53.95
20MAR84	DAM PROJECTS	48.16
05APR84	BUDGET RESOLUTION 1ST	41.76
12APR84	BUDGET RECONCILIATION	40.53
10MAY84	EL SALVADOR AID	52.54
23MAY84	ASAT WEAPON	41.05
24MAY84	CONTRA AID	39.89
31MAY84	MX MISSILE	49.39
26JUL84	SCHOOL PRAYER	42.04
09AUG84	SUPERFUND LAWSUITS	51.74

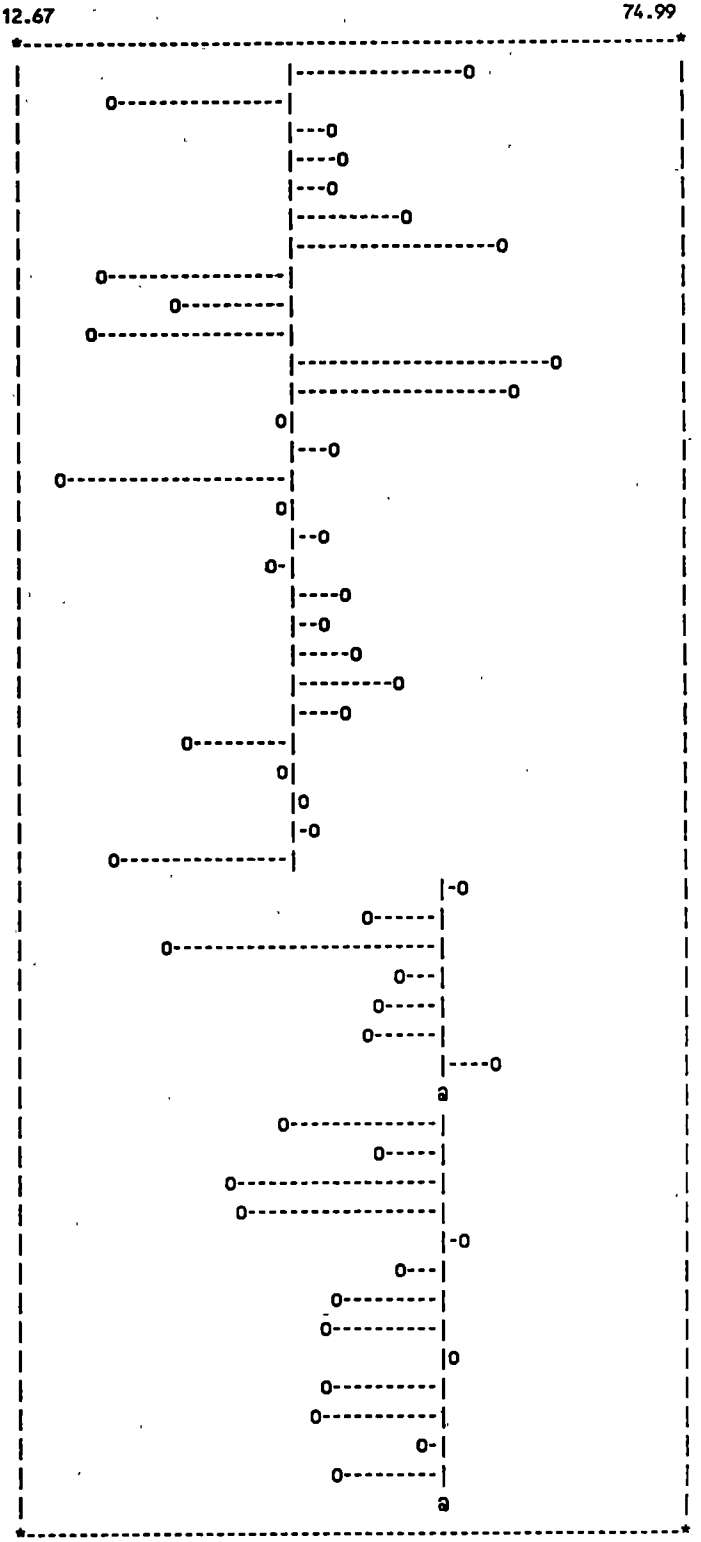


Figure 2

Bill Indifference Points (Continued)

Reagan Administration

Date	Subject	Indif. Point
25SEP84	APPROPRIATIONS ADD CRIME	63.02
03OCT84	GEPHARDT AMENDMENT	60.76
11OCT84	SOUTH AFRICA LOANS	12.67
26MAR85	MX MISSILE	47.80
30APR85	INDIANA 8TH DISTRICT	44.96
05JUN85	SOUTH AFRICA ANTI-APARTHEID	17.45
12JUN85	CONTRA AID (BOLAND AMENDMENT)	53.52
19JUN85	CHEMICAL WEAPONS	48.66
20JUN85	STAR WARS	50.75
10JUL85	ANGOLA - REPEAL CLARK AMENDMENT	57.82
03OCT85	FARM PROGRAM REFERENDUM	58.88
10OCT85	TEXTILE IMPORT QUOTAS	40.34
24OCT85	BUDGET RECONCILIATION - LATTA	46.58
01NOV85	DEBT LIMIT	36.87
21NOV85	PLANT CLOSING	46.98
10DEC85	SUPERFUND TAX BUSINESS	44.00
10APR86	GUN CONTROL EASE	66.53
05JUN86	PUBLIC HOUSING	55.95
25JUN86	CONTRA AID	49.75
06AUG86	SPEED LIMIT 65	46.99
08AUG86	DEFENSE BUDGET	31.72
08AUG86	NUCLEAR TEST BAN	34.62
12AUG86	STAR WARS	35.93
15AUG86	DEBT LIMIT & GRAMM-RUDMAN	34.05
17SEP86	ANGOLA REBEL AID	54.75
19SEP86	EPA REGULATE PESTICIDES	51.38
25SEP86	TAX REFORM	34.43
25SEP86	CONTINUING RESOLUTION (DEFENSE)	49.77
01OCT86	CHEMICAL WEAPONS	44.22
18MAR87	SPEED LIMIT 65	50.40
09APR87	BUDGET RESOLUTION	45.60
23APR87	BUDGET CUT - ACROSS BOARD	61.17
29APR87	GEPHARDT AMENDMENT	55.55
19MAY87	NUCLEAR TEST BAN	39.76
08JUL87	REFLAG KUWAIT SHIPS - DELAY	40.34
22JUL87	CATASTROPHIC HEALTH INSURANCE	42.68
30JUL87	PRICE-ANDERSON ACT	54.96
21OCT87	INDEPENDENT COUNSEL LAW	36.02
29OCT87	BUDGET RECONCILIATION	55.42
03DEC87	DELAY CLEAN AIR - EPA	38.45
15DEC87	WELFARE REFORM	51.23
03FEB88	CONTRA AID	48.15
23MAR88	BUDGET RESOLUTION	27.82
21APR88	PLANT CLOSINGS & TRADE	35.32
28APR88	NUCLEAR TEST BAN	45.02
04MAY88	STAR WARS	33.22
02JUN88	CATASTROPHIC HEALTH INSURANCE	17.61
22JUN88	APPROPRIATIONS - NASA, HUD, EPA	59.08
23JUN88	HOUSING DISCRIMINATION - CHILDREN	24.57
28JUN88	ABORTION D.C.	56.56
07JUL88	WELFARE REFORM	61.25
04AUG88	JAPANESE AMERICANS REPARATIONS	32.41
08SEP88	DRUGS DEATH PENALTY	75.00
15SEP88	HANDGUN CONTROL WAIT	49.80

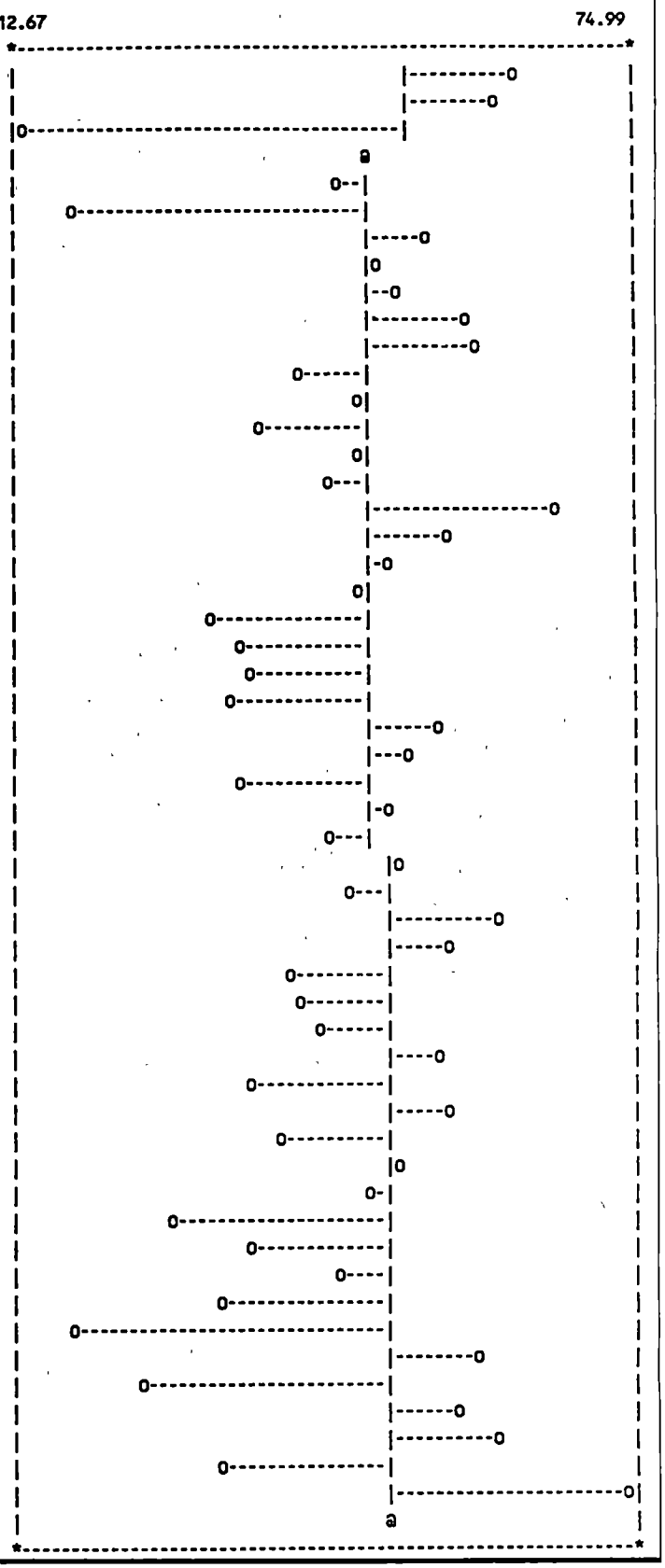


TABLE 1

Bill Indifference Point As a Function of Median Preference of Current Congress

VARIABLE	ALL ISSUES COEFFICIENT	CLOSELY CONTESTED ISSUES[a] COEFFICIENT
Median preference[b]	.39	.58
	(.16)	(.11)
Constant	25.55	19.13
	(7.25)	(4.92)
R^2	.04	.22
Adjusted R^2	.03	.21
F-value	6.12	29.49
p value for equation	0.01	0.00
Number of cases	158	107

Note: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal-conservative. The metric is in ADA points, ranging from 0 to 100. Standard errors are in parentheses.
[a] These include only the roll calls decided by 80 votes or less.
[b] The ADA ideal point of the median member for each Congress.

opinion and understanding the power of symbolic gesture (Edelman 1971), such actors will take great care to shun compromise and instead frame political choices in stark, dramatic, unrealistic, and perhaps confrontational terms. When electoral politics dominate actors' minds, we expect agenda-setters to ignore their chances of winning on the floor and instead to concentrate on taking the high ground.[10]

Next, legislators who seek primarily to shape the contours of public policy will engage in persuasion, compromise, and bargaining. In such legislators' minds, a median preference proposal represents no gain at all, and a "sincere" proposal of liberal or conservative purity will be unrealistic. Instead, these actors seek the support of potential opponents: they compromise, they promise future political support, they introduce side payments. Limited by their resources, such legislators produce proposals shaded toward—but not fully realizing—their own preferences.

Finally, many legislators (and certainly the party leaderships) act primarily to maintain and enhance their own political power. (For a theoretical discussion, see Calvert 1987.) The pursuit of political power, and equivalently the appearance of political power, requires that actors gain political successes: legislators understand the sense that weakness begets weakness and strength begets strength (Neustadt 1960). Effective leaders must not only produce policies but claim credit for those policies.

The presence of uncertainty leads to a seemingly perverse result. The initiating legislator will know the preferences of other actors, including the median member, only with uncertainty: Policy matters involve value trade-offs that are complex, side payments make the voting preferences of legislators unpredictable, and so on. Thus, the agenda-setter can only guess about the median position. This fact has a different effect on those shaping policy and on those seeking power.

The *policy shaper* knows that the "null" model implies a victory for the true median preference position. If the liberal actor proposes a policy slightly to the left of Center, the actor will have two chances of victory: (1) the bargaining and side payments may attract enough would-be opponents to pass the legislation, and (2) the true median position may be to the left of the actor's guess. The downside of proposing liberal legislation is that the result will be the median preference position—neither loss nor gain.[11] The policy shaper is thus risk-neutral and perhaps risk-acceptant.

Unlike the symbolic provocateur and the policy shaper, the *powerseeker* must avoid losing the nominal vote and is thus risk-averse. Knowing the median preference only with uncertainty, the liberal legislator will introduce proposals *more* conservative than the median. Thus, those mindful of winning will trade off policy content for prospects of victory.

As a first cut, consider the possibility that the final agenda-setter of a bill (or amendment) has a strategic advantage over the opponent. By making the motion, the agenda-setter makes the decision to contest an issue on a particular substantive mix. From the historical record we are able to determine the individual actor who moved the bill. Most often, these actors' objectives are transparent, either in form of final passage or of an amendment to weaken or kill a piece of legislation.[12]

In a free-floating, unstructured, political situation the agenda-setter has no advantage because the opposition may nominate another alternative that will approximate the median voter outcome more closely. Yet House floor politics are hardly free-floating. Obviously, House rules allow only limited opportunities for repeated considerations. In most of our cases, the agenda-setter of a bill (or amendment) served as a *final* agenda-setter (in the sense of Romer and Rosenthal 1978); that is, the Key Votes we analyze were considered at the time as the decisive vote.

Figure 2

Bill Indifference Points (Continued)

Note. The indifference points for each bill represent the point on the liberal-conservative scale where the legislators shift from *yea* to *nay*. They are graphed (small circles) on the 0–100 ADA scale, running conservative to liberal. Conceptually, each represents the ideal point of the legislator who is indifferent between the proposed bill and the expected alternative. The vertical hash marks represent the ADA score of the median member for each congress. The estimates are produced by an iterative estimation procedure that searches the policy space for the indifference point minimizing the amount of error in voting. For a more explicit and technical statement, see n. 5.

TABLE 2

Bill Indifference Point (Deviation from Median Voter) as a Function of Agenda-setter

AGENDA-SETTER	COEFFICIENT
Conservative	1.44
	(1.37)
Liberal	−5.29
	(1.31)
R^2	.08
Adjusted R^2	.07
F-Value:	6.69
p value for equation	.00
Prob. that conservative and liberal agendas are same	.00

Note: Standard errors for the coefficients are in parentheses. The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). N = 158.

TABLE 3

Bill Indifference Point (Deviation from Median Voter) as a Function of Agenda-setter Leadership or Maverick

AGENDA-SETTER	COEFFICIENT
Conservative leadership	4.63
	(1.90)
Liberal leadership	−7.32
	(1.41)
Conservative maverick	−1.71
	(1.66)
Liberal maverick	.99
	(2.27)
R^2	.18
Adjusted R^2	.16
F-Value:	8.26
p value for equations	.00
Prob. that conservative maverick and leadership agendas are same	.04
Prob. that liberal maverick and leadership agendas are same	.00

Note: Standard errors for the coefficients are in parentheses. The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole, and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). The party leadership here is indicated by the unanimous positive support of both the majority (or minority) leader and the whip (plus the president for the Republicans). Unsupported moves are termed *mavericks*. N = 158.

It is theoretically instructive to observe distinctions between liberal and conservative placements. If actors merely push their own preferences, then liberals should nominate bills with indifference points to the left of the median voter (produce a positive score in ADA terms), while conservatives should place theirs with indifference points to the right (a negative score). If, instead, the actors value victory more than policy, they will choose indifference point placements to the opposite side of the median preference (liberals to the right, conservatives to the left).[13] When these motives are neatly balanced, the median voter outcome will dominate; we shall not gain any empirical leverage.

Table 2 provides statistical estimates of how liberal and conservative agenda-setters choose to place their bills' indifference points *relative to the median voter*.[14] Both liberals and conservatives were inclined to concede on policy in return for political success. Liberals set the agenda 5.29 ADA points rightward of the median preference, conservatives 1.44 points to the left. The distinction between liberals and conservatives is statistically discernible and of modest magnitude. It indicates that agenda-setters consider power, as well as policy outcomes.

This first cut, however, includes moves by all sorts of members in either coalition. Importantly, the analysis mixes the initiatives of backbenchers and renegade committees with the more calculated moves of the two coalitions' leaderships. If we are to take full advantage of asymmetries in motivations, then we should separate the *mavericks* (for whom symbolic outcomes may be most important) from the political professionals (who must consider both the importance of achieving genuine policy outcomes and the importance of winning for political power's sake).

Table 3 makes just this distinction. Here, we distinguish the moves of liberal or conservative mavericks (that is, agenda-setters who operate independently from their political leaders) from the strategies of the leaderships.[15] The pattern looks surprisingly satisfying. For mavericks, we observe only a modest difference between liberals and conservatives. Pointedly, these differences reflect modest symbolic gratifications, with liberals making moves slightly to the left (.99 ADA points) of the median voter, conservatives slightly to the right (−1.71). On the other hand (and most importantly), the political leaderships evince a distinctive inclination to trim their policy objectives in an effort to obtain political victory. The Democratic party leadership initiates action substantially to the right of the median preference (−7.32), while the Republican leadership (when representing the presidential administration) initiates action to the left (4.63). In the name of political success, these moves concede considerable policy to the opposition.

The Institutional Setting

Much of what political science has learned about legislative politics indicates that institutions (the sets of rules and norms that govern behavior and expec-

tations of behavior) play a central role when preferences get turned into policies. In a general theoretical sense (Riker 1980), rules and norms effectively dominate politics. In our simplified agenda contest, institutions are important because they govern the management of uncertainty. Politicians will concede policy goals (in the pursuit of success) when they are uncertain about outcomes and thus need to build a "margin of safety" into what might otherwise be a minimal winning coalition.

Clearly, technical rules matter. Agenda-setters have an enormous advantage when the opponents' cannot propose amendments (McKelvey 1976; Romer and Rosenthal 1978). Further, when rules such as *germaneness* constrain tactics, strategic calculations take on more order (Shepsle 1979). Often, the combination of rules and available strategies will produce "structure-induced" equilibria.[16] Yet as the structural setting ranges from "closed" to "open," uncertainty rises—and so do considerations of risk.

At the same time, informal norms affect uncertainty. When the membership adheres to traditional institutional guidelines such as *apprenticeship, specialization, reciprocity,* and *committee deference,* then agenda-setters can expect few floor maneuvers in opposition to committee-sponsored legislation. Junior members refrain from floor activity and senior members defer to their colleagues' judgment. When those norms disappear, then more members with different objectives will participate, and uncertainty will accordingly rise.

Finally, shared understandings about the exercise of internal political power shape uncertainty. When political power is organized hierarchically and when non-policy-based sanctions are concentrated in the formal leadership structure, then the number of "independent" political actors will be restricted. When political power is widely dispersed, then the number of actors and interests of whom coalitions must be formed will increase. Commensurately, uncertainty will rise.

Thus, our theory predicts that institutional structures affect politicians' strategies in the agenda game. Under complete information, politicians may safely maximize policy gains by creating minimal winning coalitions. However, when institutions change to increase uncertainty, then we expect that politicians will trade away policy to ensure political success. For a test, we take advantage of the large-scale historical change that characterized the House between the 1950s and the 1980s.

In the days of Rayburn, McCormack, Martin, and Halleck, Congress watchers saw a highly structured politics. Committees did not only the House's work but its politicking as well. The substance of politics was often determined by close negotiations between Rayburn and the committee chairs. Members subscribed to restrictive norms (such as apprenticeship, committee deference, and so on). Finally, the formal leadership (most notably the senior committee chairs) could and did exercise internal sanctions to keep junior members in line.

This stable order was upset by revolutionary change. Starting in the late 1960s and early 1970s, members took on a more individualistic bent, becoming *managers of enterprises* (Salisbury and Shepsle 1981) and *policy entrepreneurs* (Loomis 1988). The old order of "go along, get along" died a quick death. The new breed of members altered the face of House politics to empower individual members at the expense of the formal hierarchy. This fundamental transformation left the House drenched in uncertainty (Shepsle 1989; Smith 1989).

The changes in House politics dictated a change in leadership styles. O'Neill and Wright, taking advantage of the reforms of the 1970s, gained—and began to exercise—control over the formal rules.[17] Perhaps more telling, they adopted a more inclusive and extensive leadership that attended to the interests and policy objectives of the growing number of policy entrepreneurs (Sinclair 1989). Coalition formation began to emphasize a coincidence of policy interests rather than the mutual accommodation that characterizes the maintenance of a governing coalition.

Thus, our agenda-setting prediction depends on comparing the 1980s' *decreased* uncertainty (due to the leadership's exercising control over the formal House rules) with the 1980s' *increased* uncertainty (due to the leadership's need to organize an increasingly atomistic membership). Given our reading of the anecdotal evidence, we suspect that the 1980s were characterized by *more,* rather than *less,* uncertainty. Our empirical work will provide a test.

Our theory says that institutional change influences agenda-setters because it affects uncertainty. When the strategists can reasonably expect to define the ground over which the floor fight will take place, then they might reasonably cut the margin razor-thin. For example, if the roll call were to be decided on liberal–conservative terms, then a concession of two ADA points would yield a margin of about 15 votes (producing a final total of, say, 225 to 210). Yet, the data in Table 3 suggest that the leaderships, on average, calculated safety margins *two to three times* that amount.

Given our understanding of the different political worlds of the 1950s and 1980s, we should expect that the eras' strategists would have planned accordingly; that is to say, while trading off between policy and political success, Rayburn's Democrats might have allowed much smaller "margins of safety" than the Democrats lead by O'Neill and Wright could afford.

The data in Table 4 confirm our expectation. The conservative leaderships maintained a rough margin of safety in both eras (4.78 and 4.56 points). The liberals (Democrats) clearly cut things dramatically closer in the 1950s than in the 1980s (compare −1.68- with −10.28-ADA-point concessions). The Democratic leaders, under Rayburn, were able to manage House politics to their advantage. The disappearance of restrictive norms, the decentralization of political power, and the rise of policy entrepreneurship forced O'Neill and Wright to turn toward policy concessions in order to assert their political leadership.[18] If this

TABLE 4

Bill Indifference Point (Deviation from Median Preference) As a Function of Agenda-setter by Political Era

AGENDA-SETTER	1953–60	1981–88
Conservative leadership	4.78	4.56
	(3.09)	(2.33)
Liberal leadership	−1.68	−10.28
	(2.29)	(1.74)
Conservative maverick	1.88	−2.12
	(2.56)	(2.10)
Liberal maverick	−1.07	1.60
	(4.58)	(1.74)
R^2	.06	.24
Adjusted R^2	.00	.22
F-value	.70	10.48
Number of cases	54	104
Prob. that conservative maverick and leadership agendas are same	.48	.04
Prob. that liberal maverick and leadership agendas are same	.91	.00

Note: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole, and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). The party leadership here is indicated by the unanimous positive support of both the majority (or minority) leader and the whip. Unsupported moves are termed *mavericks*. Standard errors are in parentheses. The coefficients are the mean indifference point generated by each type of agenda-setter.
* $p = .79$.

simple test is a guide, structure (and lack of structure) sets the bounds of agenda strategy.

Plebiscitary Presidents and Political Opportunity

The institutional changes in Congress comprise part of a broader array of changes in U.S. politics. Polsby (1978) suggests that the key lies in an erosion of the traditional means of "intermediation." Political actors now emphasize mass media persuasion, rather than interpersonal bargaining: confrontation has replaced negotiation. Agenda politics is out in the open.

Presidential politics has been in the vanguard of this revolution. Presidents commonly assert—and the public apparently agrees—that only the presidency stands for national interests and can assume the lead in national policymaking. Accordingly, presidents have come to adopt strategies in which they mobilize mass political support to alter the complexion of congressional politics. In the current style, presidents strategically "go public" (Kernell 1986) to alter the political opportunities for legislative success. Straightforwardly, when they mobilize public support, presidents are able to enhance the chances of their policy proposals.

Of course, the mechanisms here may vary. For example, when the president gives a national television address and generates thousands of supportive letters, moderate Democrats may act *as though* the president's conservative bill is, in fact, moderate. In this case, they anticipate that their constituents will see the bill as acceptable when otherwise, they would not. Of course, the president need not actually convince anyone. The president may give speeches, with legislators as his real audience—to give members pause because they cannot be sure that their constituents are unmoved. This is a classic case of the third-party effect in communications (Davison 1983). Alternatively, when the president energizes public support or when he is evidently "popular" in the electorate, legislators may accept the president's program simply because no single individual is willing to stand up and criticize.

A quick test of this idea may be seen by adding three variables to the agenda-setter model, the first measuring presidential approval and the second and third indicating whether the president went public on the issue (taking a conservative or a liberal position).[19] If plebiscitary politics affects Congress directly, we expect approval and conservative speechmaking to move the indifference point in a liberal direction (that is, public pressure would make moderately liberal members indifferent to a bill that they would ordinarily oppose) and liberal speeches to move the indifference point in a conservative direction. These expectations are not sustained. The additions are *not* statistically significant for the full set of data ($p = .75$), nor are they significant for either the Eisenhower or the Reagan eras ($p = .38$ and $p = .65$).[20] At first blush, plebiscitary politics has little influence on congressional agendas.

Yet things may not be so direct. Suppose agenda-setters take presidential activity into account before deciding on the policy mixtures to offer. Conservative agenda-setters will calculate the expected effects of presidential speechmaking into their sense of what is possible to achieve, perhaps adding a little more "conservative tonic" into the policy mixture. Likewise, when faced with an active and popular president, liberal agenda-setters will water down their proposals.

This effect of presidential activity on agenda-setter behavior depends on the precision of politicians' expectations about presidential effects. Under perfect information, when the effects of presidential activity are known with certainty, agenda-setters may exactly balance the effects of presidential activity against the policy trade-offs they might otherwise make: we should observe policy proposals that *apparently* center around the median member position.

Things get more interesting when agenda-setters cannot be sure about how much influence the president wields. Legislators' uncertainty about the effects of presidential activities, like legislators' uncertainty about the median voter position, allows us to detect differential weighting of objectives. Because they cannot be sure about the outcomes, agenda-setters must balance policy gains against political success. If

their trade-offs are consistent, leaning in one direction or the other, then we should see a pattern in the agendas they offer.

Plebiscitary politics should affect the nature of political opportunity and thus, indirectly, political agendas. When the president is popular, his supporters should have a greater chance of "winning." Further, when the president goes public, he also raises the stakes of the game, in effect signaling a *must win* situation. Agenda-setters should respond to the changing political situation by altering their strategies.

Conservatives may pursue policy gains that might otherwise be unobtainable: they would then err on the side of proposing bills that end with the indifference point on the conservative side of the median preference. On the other hand, conservatives may emphasize politics and "play to win"—mainly when the president enjoys popular support or goes public, these being their only realistic chances for victory. Of course, when conservatives seek victory, they must make policy concessions to ensure success; they must maneuver toward the liberal side of the median House member. Thus, our expectation for conservatives depends on how plebiscitary politics affects the relative importance of policy and power.

Similarly (though not symmetrically), the changing political circumstances should affect the liberals' strategies. When the president rides high or mobilizes public support, we should expect liberals (both policy shapers and powerseekers) to produce proposals even more conservative than they otherwise might.

Thus, if the effect of plebiscitary politics on congressional proposals is not direct but, instead, indirect—influencing not preferences but, instead, agendas—we should observe differently signed effects for conservatives and liberals, respectively. This we find to be true. In simple bivariate regressions, we find that a president's going public moves conservative agenda-setters to concede seven ADA points to the left and moves liberal agenda-setters to concede seven ADA points to the right. Significantly, both conservative speeches and liberal speeches produce the same effect (a concession to the opposition), indicating that the important part of going public is that it changes the stakes of the political contest. Similarly (though less consistently), approval also produces strategic concessions on the part of both conservatives and liberals.

Understanding that plebiscitary politics produces approximately equal and opposite effects, depending on the agenda-setter, we may examine its power to augment our agenda-setter–institutional change model. In Table 5 we introduce two "composite" variables[21] for approval and going public, signed to match our expectation that presidential activity and popularity will generate agenda-setter concessions. In the context of the full model, we observe that agenda-setters concede an average of about one-fifth of an ADA point (−.22) for each point change in presidential approval and about five ADA points (−5.09) whenever the president goes public. The numbers pass statistical muster, with *p*-values of .10 and .06 individually, .05 jointly.[22] Thus, we may say that plebiscitary politics matters.

TABLE 5

Bill Indifference Point (Deviation from Median Preference) As a Function of Agenda-setter and Plebiscitary Presidential Politics

VARIABLE	COEFFICIENT[a]
Going public–agenda-setter interaction[b]	−5.09 (2.71)
Presidential approval–agenda-setter interaction[b]	−.22 (.13)
Conservative leadership	2.88 (2.58)
Liberal leadership	−6.72 (2.16)
Conservative maverick	−.68 (1.57)
Liberal maverick	1.21 (2.76)
R^2	.19
Adjusted R^2	.16
F-value	7.08
Prob. of no effect of	
Going public	.06
Approval	.10
Going public and approval	.05

Notes: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. Note that this metric means that a positive move is toward the liberal pole and that a negative move is toward the conservative pole. The agenda-setter variable reflects the sponsor of the bill and whether or not the bill received the backing of the party leadership (for Democrats) and of the party leadership and the president (for Republicans). The party leadership here is indicated by the unanimous positive support of both the majority (or minority) leader and the whip. Unsupported moves are termed *mavericks*. Standard errors are in parentheses. N = 158.

[a] As before, the agenda-setter coefficients are the mean indifference point generated by each type of agenda-setter. Those associated with going public and approval variables show the amount of *additional concession* associated with the president's going public or with each point change in public approval.

[b] These terms represent agenda-setter (1 = liberal, −1 = conservative) × going public (1 = *yes*, 0 = *no*) and approval (percentage points deviation from the president-specific mean).

Further, it matters just as conventional wisdom expects. In separate analyses for the 1950s and the 1980s, we observe that public presidential politics became more important in the latter period. The joint tests produce statistical tests of .28 and .05, respectively. However, we observe relatively equal power for going public during both eras (it induces strategic concessions of about five to six ADA points). The main difference is that presidential popularity played practically no role in the 1950s, while it surely did in the 1980s.

The overall picture is clear: politicians adapt their agenda strategies to changing political opportunities. When presidents go public, they raise the *political*

TABLE 6

A Comparison of Components by Political Era: Bill Indifference Point As a Function of Median Preference, Agenda-setter, and Plebiscitary Politics (Closely Contested Issues)

MODEL COMPONENTS	1953–60	1981–88
Median preference[a]	.00	.00
Agenda-setter[b]	.55	.03
Plebiscitary politics[c]	.71	.06
Agenda-setter and plebiscitary politics	.54	.00
R^2	.48	.36
Adjusted R^2	.38	.31
F-value:	4.95	7.62
Number of cases	33	74

Note: The estimated indifference points are those that would split *yeas* and *nays* if the vote were strictly liberal–conservative. The metric is in ADA points, ranging from 0 to 100. These include only those roll calls decided by 80 or fewer votes. Each entry is the probability that the component, or combination of components, is not statistically discernible.

[a] The ADA ideal point of the median member for each Congress.

[b] Dummy variables for liberal leadership or for conservative leadership. Mavericks are not differentiated.

[c] Interactive variables for going public and for approval combining with the camp (liberal or conservative) of the agenda-setter.

stakes of the game and thus encourage power-sensitive agenda-setters to build in increasingly large margins of safety. Further (in recent years, at least), when presidents enjoy popular support, they and their legislative allies "play to win." In effect, when presidents activate the mass public, they alter the character of politics: they encourage politicians to trade away policy objectives in pursuit of raw political power.

A Comparison of Model Components by Political Era

The evidence for a transformation in strategic agenda-setting becomes striking when we put all the parts together. Table 6 incorporates three empirical components (the median preference, the agenda-setter, and plebiscitary politics) in separate equations for each of our two political eras (represented in the two columns). Here, we analyze only the closely contested issues (those with margins of 80 or fewer votes), so that we may reintroduce the median preference into our consideration (see Table 1). Table 6 summarizes the statistical power of each model component (rather than of the separate variables within each component). They are partial F-tests and give the probability of each component's being statistically insignificant. Thus, we may judge the applicability of the median preference, the agenda-setter, and the plebiscitary politics components before and after the changes of the 1960s and 1970s.

Compare the two eras. In the 1950s, when the House was relatively orderly and Eisenhower made sparing use of television, agenda politics closely followed the House median preference. The effects of the agenda-setter and plebiscitary politics components are statistically insignificant ($p = .54$). Notably, the median preference component, by itself, accounts for more of the indifference point variance for the 1950s (48%) than does the full model for the 1980s (36%). In contrast, when Reagan was president, the median preference was important but did not dominant: half of the "explained variance" is uniquely attributable to the agenda-setter and plebiscitary politics components. The party leaderships carefully crafted their proposals to pursue political power and to reflect rise and fall in presidential politics. Thus, the dramatic historical changes, both in the institutions that govern House politics and in the character of external politics, have fundamentally transformed the strategic calculus that governs the political agenda.

CONCLUSIONS

We now know that the connection between legislative policy and members preferences depends on how politicians craft agendas. Produced by such elements as the career goals of the actors, the institutional setting, and the structure of political opportunity, the strategic agenda transforms options and preferences to shape legislative outcomes. As the final stage in the political process, the strategic agenda acts as a camera lens that produces meaningfully different results as its focus is manipulated.

Four empirical tests support this view. First, strategic behavior appears at the most elementary level. Our examination of the median voter model indicates that political actors do indeed consider the changing composition of the House when they choose agenda strategies. Second, goal trade-offs affect strategic behavior. Concerns with political power produce agenda strategies that contrast sharply with those associated with policymaking and symbolic political goals. Third, structure constrains strategic behavior. In the 1950s, rules and norms allowed the actors to chart their course with great precision; in the 1980s, greater uncertainty made planning House politics a risky adventure. Finally, political opportunity affects the agenda. When presidents mobilize mass public sentiment, they are able to frighten opponents and to encourage their supporters to seek political victory.

No one will be surprised that the agenda is part of the political calculus in which politicians engage. However, not all will see the deeper implication. Because the stakes of the battle are relatively free to be defined by legislators and because outcomes depend on how the stakes are defined, winning and losing depend on the aggressiveness with which various actors pursue policy at the expense of nominal success. The agenda stage in the political process represents the nexus of important activity; it defines policy. The nominal outcomes are almost uninteresting.

Understanding this, the agenda becomes difficult to study. Because everyone, including the politicians,

knows the score, the agenda may represent nothing more than a sophisticated version of the outcome. Were this all, agendas would be endogenous to politicians' preferences.

Our evidence, however, suggests that we gain theoretical leverage because exogenous factors affect agenda politics. When selecting these empirical factors, we made a careful attempt to pick ones that were largely independent of any bill's policy mixture. First, while the coalition leaderships choose which bills they back, they do so not as bettors (who back winners for profit) but as full-fledged participants (who expect to shape proposals in return for political support). Second, presidents may go public only on issues that are likely winners. Yet our data indicate that Eisenhower and Reagan chose to go public when they were otherwise likely to lose (Eisenhower on foreign aid, Reagan on program cutbacks). Third, the gross institutional change between the 1950s and the 1980s was very clearly independent of the specific issues that comprise any particular roll call. Finally, all our instances of agenda politics were matters of considerable moment. The president and leaderships could not ignore the issues; choices to support or not support were political decisions, the consequences of which must have been anticipated by all. Hence, their involvement defined—rather than was defined by—the agenda strategy.

In the end, our evidence shows that agendas are strategic because they are grounded in considerations outside the policies at hand and beyond the immediate preferences of the politicians. This we believe to be true and important. We have offered several explanations, including agenda-setters' goal mixtures, uncertainty in the institutional context, and presidential politicization. We do not wish to suggest that these are the only, or the primary, factors involved.[23] Instead, we believe that the evidence establishes the substance of agenda politics as something identifiable, something other than a trivial transformation of options and preferences.

Knowing that the legislative agenda is thus strategic invites us to reconsider the process and the character of structure-induced outcomes. Rather than concentrate solely on institutional arrangements, we need to expand our view to the various agents that govern agenda-setting activity. For it is here that forces exogenous to individual preferences get translated into public policy.

APPENDIX: TESTING THE SENSITIVITY OF THE MEASUREMENT MODEL

Our measurement technique makes two critical assumptions. First, we assume that the main line of House politics can be captured by a single dimension; second, we assume that that dimension is well represented by the liberal–conservative conflict embedded in the ADA ratings. Less heroically, we also assume that the policy preferences of individual members remain (relatively) constant over each eight-year period. These assumptions are, of course, simplifications. We believe that they are reasonable enough. Nevertheless, we offer side evidence that we are on solid ground.

Here, we compare our measurements of the bills' indifference points with those generated by Poole and Rosenthal (1985, 1991).[24] The Poole–Rosenthal measurement strategy makes fewer simplifying assumptions and, in general, is considerably more thoroughly developed than ours. Importantly, the Poole–Rosenthal scoring is based on an analysis of each Congress's entire set of roll calls (not just the Key Votes or the ADA-selected issues). Further, it allows each member's policy ideal point to vary (linearly) over time. Finally, it leaves the dimensionality open for empirical test. (The order of the dimensionality is a matter of continuing discussion; see, for example, Koford 1989 and Poole and Rosenthal 1991 response.) Here, we examine the first (principle) dimension from a two-dimensional solution.

First, and most revealing, note that our ADA-based measure correlates with the Poole–Rosenthal first (policy) dimension at .72 (the Poole–Rosenthal metric is reversed for comparability). Usefully, the overall correlation rises to .84 when three problematic bills for the 1950s are eliminated.[25] The correlation is even higher within political periods (.86 and .89 for the 1950s and 1980s, respectively). The better correspondence for the 1980s is notable because our important inferences are grounded in Reagan-era politics.

Second, the results (which produce the tables in the text) are similar when we compare the Poole–Rosenthal and ADA-based measures of indifference points; that is to say, (1) the indifference points follow the Congress-specific median preferences; (2) liberal agenda-setters produce bills to the right of conservative agenda-setters; (3) liberal leaders sponsor proposals well to the right of the median, conservative leaders produce measures well to the left, and mavericks center around the median; and (4) this leadership–maverick pattern disappears for the 1950s, while it is truly strong for the 1980s. The plebiscitary politics inferences are not statistically replicable in the Poole–Rosenthal data because that series ends in 1985, before Reagan's downfall. (When the later Reagan years are left out of both series, the numerical estimates are similar for both the ADA and the Poole–Rosenthal data, neither producing statistically powerful results.)

Finally, we test the unidimensionality assumption for our Key Votes. Here, we model the aggregate outcome of each vote (both the margin of victory and the probability of victory) on the two indifference point coordinates (one for each of two dimensions) estimated by Poole and Rosenthal. Although it appears that the second dimension may be helpful to account for individual member votes (notably in the 1950s; see Poole and Rosenthal 1991), the aggregate outcomes evince sensitivity only to the indifference point placement on the first dimension. The probabilities that policy concessions on the first and second

Poole–Rosenthal dimensions *did not* affect the percentage of *yea* votes are $p = .00$ and $p = .98$, respectively, and the probabilities that they *did not* affect the aggregate outcome are $p = .00$ and $p = .94$; that is to say, when agenda-setters manipulated the content of their proposals in order to shape *aggregate* outcomes, they did so most effectively when they concentrated on issues similar to the main liberal–conservative axis.

Notes

An earlier version of this paper was presented at the 1990 annual meeting of the American Political Science Association, San Francisco. The data utilized were made available in part by the Inter-University Consortium for Political and Social Research. The data for voting studies for members of the United States Congress were originally collected by the Congressional Quarterly. We thank Keith Poole and Howard Rosenthal for sharing their data. We are grateful to Jim Weikel for his research assistance. We thank Robert Erikson, Dennis Judd, George Marcus, Gary Miller, Keith Poole, William Riker, David Rohde, Kenneth Shepsle, Terry Sullivan, and Barry Weingast for helpful comments.

1. The idea of a strategic agenda is both simple and subtle. Political scientists use the term *agenda* to describe two distinctive phenomena. The first and more technical use refers to how proposals are sequenced in a legislative body. The formal literature indicates that political actors who control the agenda can often determine collective policy choice. Famously, control over the agenda in the Powell Amendment case allowed conservatives to stall a federal aid to education bill (Riker 1986). The second meaning refers to the substantive content of politics. For example, the agenda of one Congress might be dominated by foreign policy, the next by civil liberties, and the next by budgets. (For excellent work on the substantive agenda, see Kingdon 1984; Shull 1983; Sinclair 1982; and Walker 1977.) Our use of the term *strategic agenda* is closer to the first meaning than to the second. Yet we are concerned with how legislators frame policy choices, rather than the art of manipulating voting sequences. To be sure, most legislative proposals touch on a number of substantive issues. Taking advantage, skillful politicians alter the attractiveness of any proposal by modifying its mixture of policy considerations; that is, they manipulate the strategic agenda (first meaning) by altering the substance of the bills that form the policy agenda (second meaning).

2. We have pruned the initial set of *Congressional Quarterly* bills. First, we have excluded the issues whose definition in the liberal–conservative space is convoluted or over which members' preferences were evidently *not* single-peaked. We examined the votes of each Congress's 10 most liberal and 10 most conservative members and excluded issues on which majorities from the two groups concurred. (This trick eliminates the famous sophisticated agenda game of the Powell Amendment [see Riker 1986], as well as several one-sided issues.) In addition, we have eliminated votes that required super majorities (veto overrides and suspension of rules) because these politics are embedded in a more complicated series of games.

3. We score each member by the average of his or her ADA ratings for the eight-year tenure of the Eisenhower or Reagan administration. We choose to limit this "era-specific" score to the agenda of the relevant administration in order to make the ratings comparable across individuals (see Asher and Weisberg 1978). This multiple-indicator tactic does much to smooth out the ratings that often jump from one year to the next. As a trade-off, we give up an ability to measure genuine (rather than measurement-induced) movement in the members' preferences. We accept this loss because (1) it greatly increases the stability in our "ideal point" estimates and (2) we strongly suspect that members' preferences do not change much from year to year. (For evidence, see Poole and Rosenthal 1991.)

The use of the ADA rating is less arbitrary than it might seem. The rating measures each member's voting agreement with the ADA's preferences on a set of selected roll calls. If one dimension dominates congressional policy voting, then any particular choice of scaling device will prove about as good as any other. We could factor-analyze our Key Votes (using LISREL, MDSCALE, or some other technique) and get another liberal–conservative scale (that would add up liberalism with a marginally different set of item weights). We have chosen to base our analyses in a commonly understood measure (the ADA rating), so that the results might make better intuitive sense. For further discussion and evidence, see the Appendix. Of course, politics and policy are more complicated. We are sympathetic to the richer representations exemplified by Clausen (1973) but here we need to simplify matters to concentrate on the essential question of strategic interaction.

4. We assume that all voting is sincere. (In a unidimensional world, there is no incentive for "sophisticated voting.") Relaxing this assumption, we model voting as though insincere voting were minimal. We have eliminated roll calls that were demonstrably "sophisticated." See n. 2. In their original exposition, Poole and Rosenthal (1985) chose to treat surprise votes as "errors" from proper judgments. The labeling is a matter of semantics. We only wish to note that the "other" factors play the same role here as they do in any empirical estimation equation: they represent unspecified but potentially interesting and important factors. In practice, we minimize the squared errors, assuming a quadratic loss function. Our estimates stem from an initial grid search and then a steepest descent gradient method—all limited to the unidimensional ADA scale. For each Key Vote, we begin with an initial guess, calculate the sum of squared ADA distances from each legislator's ideal point to the selected indifference point (for those voting "incorrectly"), choose another possible indifference point estimate, calculate the "errors," and decide between the previous and current estimates on the basis of a minimum squared error, and so on. In all cases, convergence was achieved fairly easily. Asymptotic standard errors run in the range of about two points on the 0–100 scale.

5. The conservative opponent has no incentive to offer something to the left of the median preference. In fact, the opponent will choose a counterproposal that is just slightly less to the right of the median voter than the liberal proposal is to the left.

6. That the agenda game is sequential is critical. When agenda-setters must anticipate counterstrategies on the part of opponents, the practical alternative to any proposal is *not* the status quo but, instead, a guess about what is likely to occur without the bill. In particular, agenda-setters cannot safely choose proposals that merely beat the status quo; they must expect their opponents to nominate an even more attractive alternative. This judgment is, to be sure, subjective. Fundamentally, agenda politics is rooted in anticipated reactions. We thank Gary Miller for suggesting the importance of this point. The implicit bargaining that characterizes Washington politics are similarly explored in Peterson (1990a, 1990b) and Sullivan (1988a, 1988b, 1990a, 1990b).

7. Here, as later, we treat the intervals in indifference points as linear in the explanatory variables. Obviously, they cannot be absolutely linear for all combinations, since they are bounded 0–100.

8. We introduce this distinction with care. By dividing by *eventual outcome*, we introduce information that was not, strictly speaking, available to the agenda-setters when they developed their strategies. However, on Key Votes at least, it is plausible that the contestants would have vote counts good enough to know whether it made sense to compromise or not. (The choice of 80-vote margins is arbitrary; larger and smaller cutoff points work about as well.)

9. This characterization applies not only to the motives of individual members but also to the rationales motivating collective leaderships.

10. That legislators often choose to pursue the longer-run game of symbolic politics over the possible gains offered by current compromise is underappreciated. As Gilmour (1990) indicates, such a political strategy has often produced policy outcomes (or nonoutcomes) distinct from those one might expect, given a short-run perspective. Symbolic politics may be more prevalent than is often thought.

11. The downside is the median preference position, because we assume that all proposals are part of a sequence (see n. 6). Future proposals converging to the median preference position will always beat the status quo.

12. We treat the agenda-setter as part of a protocoalition, not as an individual with personal preferences. In House politics, on major issues, members do considerable politicking; they form coalitions before an item reaches the floor. The nominal agenda-setter may be a mere figurehead for the real strategists. Thus, we limit our identification of the actor to a liberal–conservative tendency. Occasionally, things get a little murky. We have excluded from our analysis a handful of items in which the impetus behind a bill was bipartisan (the president joining with both party leaderships), the coalition of movers and supporters did not fit a liberal–conservative coalition battle, or the agenda-setter's motives appear potentially insincere.

13. For example, consider the liberal agenda-setter faced with a conservative proposal five points to the right of the median preference. The policy shaper may counter with a moderate liberal bill (say, three points to the left of the median) and hope that the sequence stops before converging on the median. The powerseeker, however, might counter with a marginally conservative proposal (say, three points to the right) to ensure a win. All liberals and a few moderate conservatives will prefer the new proposal over the old, and the conservative agenda strategist will be stymied. Thus, while shrewdly gaining nominal success, this liberal agenda-setter will produce a less liberal policy than would otherwise be possible. The political game reverts to claiming victory in the postgame interviews.

14. Similar numbers appear when we model the indifference points with the median voter in the right-hand side of the equation, adding 3% to the fit. We have chosen to present the model as deviations from the median voter, so that the parameter estimates will show the theoretically important distinctions. The signs of the coefficients reflect the directional component of the ADA score: *positive scores are more liberal, negative scores are more conservative.*

15. Here, the liberal and conservative leaderships are respectively indicated by a unanimity of support from (1) the Democratic leader and whip or (2) the president, Republican leader, and whip. (Abstention "breaks" the leadership.) The leadership variables are constructed to include only those cases in which a liberal or a conservative initiates an action with the support of the respective leadership. (Notice that the "liberal" leadership is equivalent to the formal House leadership for all years except 1953–54.)

16. Shepsle (1979) coined the phrase; for theoretically important extensions, see Denzau and Mackay 1981, 1983; Krehbiel 1987; and Shepsle and Weingast 1981, 1987. Examining cross-institutional structure, Hammond and Miller (1987) produce "constitutionally induced" stability in equilibria. The rise of uncertainty has been addressed in two ways: (1) legislators are uncertain about the other legislators' preferences (Denzau and Mackay 1983; Enelow 1984; and Enelow and Hinich 1983a, 1983b); and (2) the relationship between bills (policy choices) and outcomes (policy consequences) is uncertain (Krehbiel 1988). In addition, the effect of presidential strategic behavior on stable equilibria is addressed in regard to appropriations decisions by Kiewiet and McCubbins (1985, 1988).

17. The 1980s leadership responded to increased uncertainty by increasing the use of special and restricted rules. The extent to which this tactic has reduced uncertainty is not yet clear. Bach and Smith (1988) suggest that the new procedures have had the effect of reducing uncertainty in process but not in policy, that is, while the number and types of amendments have been restricted, the ideological content of them has not.

18. This result is less tidy than it seems. Note that floor voting was less clearly determined by matters of ideology in the 1950s than in the 1980s. The median R-squared between a member's ADA score and vote on the Key Votes was .33 during the 1950s and .54 during the 1980s. Thus, in policy terms, the 1980s were a period of greater certainty than the 1950s. However, recall that the nonideological factors influencing voting include the sorts of informal leverage—the side payments—associated with internal control of the House. By all historical accounts, the 1950s party leadership controlled and exercised this nonpolicy leverage over individual members' choices: "go along, get along." We get an initial resolution by looking at "ideological" and "nonideological" issues for both the 1950s and the 1980s. If all is a matter of ideological structure, then the agenda-setter model should be seen to work better for the more ideological issues. On the other hand, if the theoretical perspective associated with agenda uncertainty predominates, then we should see something more subtle. In the 1980s, what was nonideological was uncertain; thus, the model should produce a stronger result for the nonideological issues. Crucially, though, the 1950s leadership's greater control over the nonideological factors should produce a weak agenda-setter result for the nonideological issues. And this is just what the data show. The following table shows the signed difference between the conservative and liberal leadership agendas for the 1950s and 1980s. (High and low ideological content refer to scores above and below, respectively, the overall median R-squared between individual ADA score and roll call vote [.40]; −.35 is a wrong sign; for the remaining data, $p < .05$; Ns are in parentheses.)

	1950s	1980s
High ideological content	6.56 (22)	6.73 (68)
Low ideological content	−.35 (32)	10.82 (36)

Notice that things align nicely for both types of issues in the 1980s, with the conservatives producing agendas (indifference points) well to the left of the liberal agendas. Contrary to the "ideological" explanation, the difference is larger for issues of lower ideological content. Notice especially, however, that the pattern is reversed in the 1950s. Because nonpolicy matters were largely under the control of Rayburn and his barons, the leaderships were able to pursue policy success (with a minimal safety margin) most clearly on the issues whose policy content was least ideological. (This result is particularly powerful because it is produced by a nonlinearity that falls directly out of theory.) Uncertainty is what matters here.

19. We measure presidential prestige by using the familiar Gallup approval polls. This is the common presidential approval measure that has dominated studies on presidential success or influence with Congress (Bond and Fleisher 1980, 1984; Edwards 1976, 1980, 1989). The data are taken from Edwards's (1990) compilation of the series and aggregated to quarters. Further, because we have data that span two different presidencies, from two different political eras, we subtract each president's mean score from the relevant reading. (Eisenhower was, on the whole, much more popular than Reagan.)

As suggested by Kernell (1986), we measure *going public* in two forms. We distinguish between major and minor addresses, with major addresses being televised talks and addresses to a joint session of Congress and minor addresses being those directed toward a specific constituency. For all of Eisenhower and for the first five years of the Reagan administration, we took Kernell's (1986) coding for the major public

addresses. We obtained a measure of major addresses for the remaining three years of the Reagan administration and minor addresses for both presidencies from the Public Affairs Information Service Index. After some experimentation, we settled on a variable that counts both major and minor address (if they occurred in the half-year preceding the key vote). Further, we decided to indicate going public in simple dichotomies, that is, whether he went public or not (as a conservative or a liberal). The additional information, assessing the amount of speechmaking, contributed little to our explanation. Our simplification eases the interpretation of the empirical results.

20. For the entire data set, the relevant parameter estimates are as follows (cf. Table 3).

Variable	Parameter	SE	*p*
Approval	.13	.13	.32
Go public (conservative)	.18	3.10	.95
Go public (liberal)	2.91	4.99	.56

with the remainder of the table resembling Table 3.

21. Here, going public and approval (the deviation from the president's mean) are multiplied by one for agendas offered by liberals and by minus-one for agendas offered by conservatives. Thus, a negative coefficient (as seen in Table 5) indicates a *concession* on the part of the agenda-setter.

22. We do not wish to make too much of statistical inference here. Of our 158 Key Votes, only 20 were accompanied by the president's going public. In effect, our estimates' magnitude and precision depend on those 20 cases. Given the paucity of data, we suspect these "significance" tests to belie stronger relationships than we can prove. Further, these are two-tailed tests. Obviously, a more powerful view asserts a one-tailed test (given our theory) and generates *p*-values of one half those in the text.

23. Our empirical model is very simple. In essence, it includes the Congress median ADA score, the presence or absence of the House leaderships, and (marginally) the popularity and speechifying of the president. We do not expect to account for the full subtleties in agenda strategy with such a simple model. Instead, we only wish to establish the strategic character of those agendas qualitatively. Nevertheless, our models do account for about one-fifth of the variance in indifference points. If we wish to engage in the variance-accounted-for exercise, the full model implied in Table 4 (which takes into account era-specific differences) accounts for 28% of the total variance. Further, for this exercise, we may exclude relatively one-sided contests (bills whose margins of victory or defeat exceeded 80 votes) on the grounds that our model cannot explain strategic behavior beyond establishing a substantial "safety margin." Then the simple model "accounts for" 40% of the variance and produces a root mean squared error of 5.71 ADA points. This exercise merely establishes the plausibility of our simplification; it does not propose our model as a complete substantive explanation.

24. We should like to express our thanks to Keith Poole and Howard Rosenthal for kindly making their data available to us.

25. The three bills include two foreign aid bills (1957 and 1960) intended to enhance appropriations beyond the committee report. Rather than relying on the Democratic leadership for political leverage (as they typically did in such cases), the administration pushed its case against the united Democrats. The third is the civil rights bill of 1957, which (as ever) split dominant coalitional bases. Of our Key Votes, only these are estimated (by Poole and Rosenthal) to have indifference points *outside* the cloud of legislators on the first dimension; thus, they are poorly replicated in one dimension (the prediction is a unanimous vote)—hence the trouble.

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ERIC VOEGELIN'S THEORY OF CONSCIOUSNESS

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The self stands in the way of understanding and appreciating consciousness. The self is a reflexive, asymmetrical bit of consciousness that displaces the whole of consciousness and the open nature of consciousness. Such displacements of consciousness are expressions of gnosticism. Some political movements (e.g., fascism) are expressions of such restrictions of the horizon of consciousness. To understand and appreciate consciousness requires a degree of selflessness on the part of the inquirer. Philosophy and myth are modes of discourse that embody and cultivate the selflessness necessary for participating in, appreciating, and understanding consciousness. Such philosophizing enhances the possibility of more inclusive joint political action.

Eric Voegelin is explicit about the political significance of a theory of consciousness. He says that his "analysis of the movements of Communism, Fascism, National Socialism, and racism, of constitutionalism, liberalism, and authoritarianism had made it clear beyond a doubt that the center of a philosophy of politics had to be a theory of consciousness" (1977, 3).[1]

I shall elucidate Voegelin's theory of consciousness and show how it is central to a philosophy of politics. We begin to see the relationship between consciousness and politics by noting that the point of political action is not only to accomplish particular purposes but also "to speak solemnly in everyone's name, in the name of society, about what it holds dear" (Nozick 1989, 289). This is so because joint political action constitutes a relational tie; by political action we "express and instantiate ties of concern to our fellows" (p. 288). The ties of concern and the values of the community go together; without the relational ties, it would not matter to us whether or not the others were included in regard to what we held dear. Voegelin's theory of consciousness helps to explain the nature of these relational ties of concern at the heart of politics. His theory especially helps to explain their absence.

The thrust of my argument is that by its very nature the *self* cannot participate in, and appreciate, the reality of the whole of consciousness. One form that participation and appreciation of the reality of the whole of consciousness takes is philosophy. Voegelin describes the participation that is philosophy as the "compelling experience of eternal being in time," an "experience of transcendence" (1964, 117). Thus, the experience of philosophy *is* the transcendence of self, not merely a means to such transcendence. This existential, or experiential, quality of Voegelin's meaning is absolutely essential and is also reflected in his pronouncement on the shortcoming of histories of ideas, namely, that they do not adequately take into account experience and its symbols (1989, 63). (It is also true of the self that it does not adequately enter into, and account for, experience and its symbols.) This experiential dimension points to the significance for philosophy and consciousness of "anamnetic experiences," in which transcendence of self both occurs and can be further cultivated. Transcendence, however, does not remove the "tension in existential reality pointing toward an order beyond itself" (Gebhardt 1987, 117). Political movements such as fascism and racism are extreme forms of "selfness"—"restrictions of the horizon of consciousness" (Voegelin 1977, 4). Moreover, philosophy as participation in consciousness (the experience of eternal being in time, or transcendence) enhances both knowledge of the political significance of the self and the possibility (through transcendence of the self) of more inclusive joint political action.

SELF AND CONSCIOUSNESS

The first and absolutely critical step toward understanding consciousness is to distinguish consciousness from the I, or self (hereafter used interchangeably). According to Voegelin (1978):

> It is doubtful whether consciousness has the form of the I, or whether the I is not rather a phenomenon in the consciousness. (p. 19)
>
> Consciousness is not constituted as a stream within the I. (p. 36)
>
> No "human" in his reflection on consciousness and its nature can make consciousness an "object" over against him. . . . (p. 33)

To make an object over against entails the formation of the self; the self is the subject that stands over against, that separates itself from the objects. The presence of the self, therefore, constitutes a denial by the self of the whole of consciousness. From a perspective outside or beyond the self, it is possible to see that such denial does not result in the absence of consciousness, since consciousness is ever present as the *context* within which the I exists. But for the self, consciousness as a whole is not present. Voegelin calls the self the transcendental I and says that "the creation of the transcendental I . . . implied the destruction of the cosmic whole in the subjectivity of the egological sphere" (1978, xi).

Voegelin's attention is on consciousness, rather than the self; the nature of the self emerges in his

work only indirectly as he clarifies the nature of consciousness. In contrast, an analysis that focuses directly on the self appears in the recent work of Robert Nozick (1989). We turn immediately to Nozick because seeing the self sharply makes it easier to understand Voegelin's analysis of consciousness. The essential point that emerges is that with the formation of the self, one is kept from understanding, participating in, and appreciating the whole of consciousness.

For Nozick, as well as for Voegelin, self and consciousness are connected. Nozick maintains that the self is "a bit of consciousness." Bits of consciousness may be considered experiences, thoughts, and so on (1989, 144). Each such bit is a case of *figuration* (Barfield 1965, 18). Figuration is what is added by the percipient to pure sensation to form a representation of what had been unrepresented (i.e., what had been formless to the percipient). Two steps are involved in figuration: (1) the sense organs are related to the unrepresented so as to elicit sensation and (2) the sensations are constructed by the percipient's mind into a representation (p. 20). Some figurations are figurations of other figurations. In other words, some bits of consciousness include, and are about, other bits. An example is a memory of an event: the event is a bit that comes to be included in the memory bit of consciousness.

The self is one of these bits, but it is different from the others in that "this bit is an awareness of many of the other bits of experience and thought, *plus* an awareness of itself, a reflexive self-awareness" (Nozick 1989, 145). Reflexive self-awareness entails awareness of an awareness that is unique: the awareness of "I," "me," or "myself"—not *any* awareness. Such awareness is exclusionary; it draws a boundary that includes some bits of consciousness and excludes others. Certain of the enclosed bits are called "me"; somehow, those bits are separated from others, then identified with (appropriated, acquired).[2]

The self entails asymmetrical knowing (Nozick 1989, 145). The bit that is the self knows the other bits; but as a self, it is uninterested in being known by other bits that are less important than it is. As the center of awareness (although unconsciously so), the self is superior to other bits. The self orders them, determining how important each is, which to ignore, which to repress, and so on. Every self might be said to be the sovereign among bits of consciousness; each self is like a miniature modern state within the vast field of bits of consciousness. Its sovereinty lies in the power to demarcate that part of all consciousness it calls itself. In this regard, the self is unique; only it has this particular demarcation, this particular set of bits of consciousness ordered in its particular way (p. 146). In sum, the sovereignty of the self lies in its self-interested control of awareness.

The self is not necessarily stagnant; it may continue acquiring, ordering, and identifying itself with new sets of bits of consciousness. It may also stop acquiring new bits and may lose what it has acquired. Yet no matter what the size of its "territory," the self does not give up its sovereignty. In other words, its asymmetrical relationship to its objects does not change.

The self does not allow itself to become equal to all the other bits of consciousness. If it did, it would disappear; and consciousness as the whole of consciousness could be present. The presence of the whole of consciousness, however, cannot be known in a conventional way of knowing; for conventional knowing is the knowing of the self. Voegelin points out that "it is the function of human consciousness *not* to flow but, rather, to constitute the spaceless and timeless world of meaning, sense, and the soul's order" (1978, 16). The knowing of the self is not the kind of knowing that can know a spaceless and timeless world of meaning.

One can see the asymmetrical nature of the self more sharply by positing a context of symmetry or *equality of bits*. Assume that all awarenesses (bits of consciousness) are equal. Then no one of them would unilaterally consider its "knowledge" of reality to be superior to any of the others' "knowledge" of reality. Each "knowledge" would be equal in the sense that no one bit would be inclined to impose itself on any other. The whole of consciousness entails such equality of bits; by its inherent asymmetry (inequality), the self disrupts that equality. As a bit of consciousness, in contrast to all of consciousness, the self orients itself by its own perspective. Thus, by its very own existence, the self does not and cannot realize that it is not open to grasping and appreciating its actual situation—that is, its context, consciousness. (One interpretation of Genesis 3 is that eating from the tree of knowledge represents the emergence of the self and constitutes the source of human alienation.) In short, the self, because of its asymmetrical nature, displaces consciousness. This displacement is one form of what Voegelin means by *gnosticism*.

The asymmetrical knowing constitutive of the self is, in theological terms, a form of *idolatry*, that is, the "identifying [of] reality with the picture of representation of it framed by the conceptualizing imagination" (Cochrane 1940, 244). (The conceptualizing imagination appears in the form of the self.) Idolatry is also described as "the effective tendency to abstract the sense-content from the whole representation and seek that for its own sake, transmuting the admired image into a desired object" (Barfield 1965, 111). In short, idolatry is a case of the fallacy of misplaced concreteness, of treating "what is but an abstract aspect of some larger whole as though it were the whole itself, or at any rate something concrete" (Ogden 1982, 29). Inasmuch as this is precisely what the self does, it seems accurate to describe the self as a form of idolatry.

The self has a tendency to project (usually unawares) its order of bits of consciousness on others as if its reality were *the* whole truth; in effect, it wants its reality to be the only reality. The self's identity is idolatrous in the sense that it rejects the whole of consciousness for its particular version. It does not say to itself, "Well, I know that the reality that I am

aware of is a distortion of reality itself, taking into account only a limited, and therefore biased, view." Rather, the self identifies unawares with its organization of bits of consciousness. It rejects the equality of bits in spite of the fact that it will itself likely change as it reorders itself to take into account new bits of consciousness. Indeed, it may relate to each newly ordered set of bits of consciousness by identifying itself unawares with that set. It does this time and time again.

The self is busy protecting itself against the ongoing attempts of other selves to displace it (Hobbes 1958, 86–88). In fact, it is so busy protecting its identity against the threat of other selves that it cannot give itself up. This is understandable; for it maintains its identity and assymetry by identifying with bits of consciousness, or content (as opposed to appreciating the whole of consciousness, or context). If it were to understand, accept, and appreciate consciousness (the context that is realized, without the self, to be timeless and spaceless), it would so open itself to the equality of other sets of consciousness as to lose its power of asymmetry: it would lose selfhood itself. If it did that—gave itself up—it would experience transcendence.

TRANSCENDENCE

The difficulties encountered in regard to thinking about consciousness are similar to the difficulties experienced in thinking about God. Robert Sokolowski, discussing Saint Anselm's argument for the existence of God, says that "God must be thought to exist; for if he were seen to exist only in the mind, he would not be appreciated as that than which nothing greater can be thought" (Sokolowski 1982, 8). "God must be thought to exist" does not mean that the thought "God exists" is necessary if God is to exist. Rather, it means that the presence of God is the whole of reality, which includes more than all thoughts existing in all of human consciousness—a reality much more inclusive than the thought "God must be thought to exist."

"That than which nothing greater can be thought" points toward God, or absolute, ultimate, all-inclusive, reality—that is, consciousness. Consciousness, the whole of reality, may be considered a context or, more accurately, *the* context (the context of all contexts). Some non-Christian theological traditions speak of this context in strikingly similar language. In the Hindu scripture we read, "That which is not thought by the mind but by which the mind thinks—know that as the Absolute" (Kena Upanishad 1.6).

Voegelin describes consciousness "as the experience of a finite process between birth and death" (1978, x). He also speaks of "tensions between human consciousness and transcendence" and says that "the capacity of transcendence is a fundamental character of consciousness" (p. xi). Most importantly, Voegelin speaks about the radicalism and breadth of philosophical reflection. He says that the radicalism of philosophizing must be gauged by "the experiences that impel toward reflection and do so because they have excited consciousness to the 'awe' of existence" (1943, 36).

Experiences that inspire the awe of existence (e.g., anamnetic experiences) are in sharp contrast to those of the self. The self is comprised of what the mind has thought. The self is a bit of consciousness always having content and identifying with content, which the self as the mind has provided. Just as the Absolute is never the content of what has been thought (but rather, is that by which the content appears), neither is consciousness ever exhausted by what has been thought (i.e., by content). Consciousness and philosophy converge in open wonder—in the awe of existence that sees beyond bits of consciousness—and opens the soul to further experiences of wonder and awe (1978, 36). Thus, in comparison to the life of philosophizing, the life of the self has a dead quality. The self seems to exist in the already-existing past (in the self's mind), while the *soul* lives in the wonderous opening of the present, of the ground of being (the "compelling experience of eternal being in time").

MYTHOLOGY AND POLITICAL SCIENCE

The importance of the experience of the awe of existence must not be underestimated. Indeed, Voegelin says that "the decisive event in the establishment of *politike episteme* was the specifically philosophical realization that the levels of being discernible within the world are surmounted by a transcendent source of being and its order" (1968, 18). The Platonic–Aristotelian analysis operates "on the assumption that there is an order of being accessible to a science beyond opinion" (p. 17). This science beyond opinion rests on experience beyond the self. In other words, the self is a barrier to the infinite experience of awe and the transcendent order of being. No science of politics is therefore possible when the self in its existence shuts off the opening of the soul to the transcendent order of being.

The words *specifically philosophical realization* imply that philosophy *is* the meditative state of being in which the self drops away into transcendence. Philosophy is not the means to transcendence of the self, it is such transcendence. Certain difficulties arise in connection with this. Voegelin refers to "the experience of meditation, at the climax of which consciousness apprehends the contents of the world nonobjectively" (1978, xi). This means that the "knowing," or apprehending, of the contents of the world by meditative consciousness is not the knowing of the self; for the knowing of the self is not nonobjective. The knowing of the self entails a subject over against objects, which, because of the asymmetrical nature of the self (a subject), forecloses the possibility of nonobjective, transcendental, meditative knowing. This point is applicable to anyone reading, writing, or

thinking about consciousness as Voegelin presents it. In other words, the meaning of Voegelin is not available to us insofar as we are only selves.

It makes sense, therefore, to turn to modes of expression in which it is more difficult for the self to operate and to modes that are not expressions of the existence of the self (although the receiving self is not likely to recognize these for what they are). This is what Voegelin does both in his essay "Anamnetic Experiments" (1978) and in his discussions on the nature of myth. (One could say, moreover, that it is possible to see in Voegelin's philosophizing the remarkable open seeing and expression of selflessness.) Anamnetic experiments are recollections of "experiences that have opened sources of excitement, from which issue the urge to further philosophical reflection" (1978, 37). Such experiences are those that excite consciousness "to the 'awe' of existence" (p. 36).

Voegelin invites us to participate in these experiences, thereby cultivating for ourselves the fundamental insight upon which *politike episteme* is based—namely, transcendence of self, consciousness of the awe of existence. Voegelin writes of himself: "A philosopher, it appeared, had to engage in an anamnetic exploration of his own consciousness in order to discover its constitution by his own experiences of reality, if he wanted to be critically aware of what he was doing" (1977, 12–13). Conceptual analysis or theorizing about consciousness is inadequate; one must explore "concretely in the constitution of the responding and verifying consciousness. And that concrete consciousness was my own" (p. 12).

Voegelin points out that Plato may have used myth "because he felt that the myth is a more precise instrument for communicating the psychic excitement of the experience of transcendence, more precise than speculation, even though it is an instrument which not everybody can handle safely" (1978, 22). "A mythical symbol," Voegelin says, "is a finite symbol supposed to provide 'transparence' for a transfinite process" (p. 21). Myths are necessary because "processes transcending consciousness are not experienceable from within" (ibid.). We must use symbols that have developed on occasions of finite experience to evoke the transcendent, the opening to further opening, the transfinite awe of existence. Myths, in short, have arisen as a means of expressing the tension within consciousness between the finite and the infinite. Mythical symbols, while likely to be finitized by the self that misunderstands the nature of such symbolic forms, are actually intended to open the soul to the transcendent (the experience of which goes beyond what the self as self will allow).

"To speak of a process as infinite," writes Voegelin, "is tantamount to saying that we have no experience of it 'as a whole' " (1978, 21). Opening ourselves to the infinite whole is precisely what the existence of the self rejects and precisely what myth and philosophy invite. The whole and the self are at odds, of course, for the whole and asymmetrical knowing are incompatible.

Mircea Eliade points out that all myths are in a sense *creation* myths because they evoke the sacred "time" that existed at the beginnings of everything. Even the telling of a myth may be an attempt to evoke the timeless and spaceless beginning (that beginning even before time and space). Voegelin says that "we have no experience whatsoever of a time in which something might begin—for the only time of which we do have experience is the inner experience of the illuminated dimension of consciousness, the process that drops away, at both ends, into inexperienceable darkness" (1978, 21). The sacred, which is creation, is beyond the experience of consciousness; to suggest otherwise would be to *subjectify* (make subject to the self) that which cannot be made subject. That which is subjectified (and thus deformed) is no longer awesome. The very purpose of myth, then, to evoke the opening of the soul to creation, is subverted by the subjectivity of self.

Voegelin explicitly discusses the creation myth of Genesis 1 (1987, 19–20). He points out that its authors faced the task of finding language symbols that would express the experience and structure of the infinite quality of consciousness, ("as an event of participation between partners in the community of being" [1987, 15]), as opposed to consciousness in its finite, "thing" form of selfness. Genesis 1 gives expression to humans participating not as selves but as partners of the wondrous "comprehending reality" (p. 16).

In spatial terms, Voegelin locates this participation, this reality of the "luminosity of consciousness" " 'between' [selfless] human consciousness in bodily existence and reality intended in its [self-ful] mode of thingness" (1987, 16). The "in-between" (Plato's *metaxy*) is meant to show that consciousness in its infinity does not occur in the presence of the self, but rather "in the In-Between of the divine and the human" (1989, 73). The reality of consciousness as infinity is not outside human consciousness (ibid.); for if it were, the self as subject over against object would again have raised its head. Not being outside, it is in the in-between, that is, the context that consciousness participates in (the community of being). Myths give expression to this context and participation in it.[3]

This points to the noumenal, rather than the phenomenal, nature of consciousness (1978, 32). As the thing-in-itself,[4] the noumenal is inaccessible to the mind. The noumenal takes us back to the discussion of the nature of context and God as exemplified by the Kena Upanishad. Consciousness is noumenal. Like myth, it is revelatory. It enables us to live more abundantly, beyond the limits of ego and mind.

GNOSTICISM

Voegelin emphasizes the inescapable nonobjectifiable nature of consciousness in the actual fullness of reality. It is the self that resists the whole of reality. The self by its very nature is and must be an act of "deformation," or deception; its self-deception is

"believing the reality of which it is a part to be an object external to itself that can be mastered by bringing it into the form of a system" (1977, 4). This goes to the very heart of what Voegelin means by gnosticism. The gnostic believes that evil, or the drawbacks of human life, "can be attributed to the fact that the world is intrinsically poorly organized" (1968, 86). With knowledge (gnosis) of the method of altering being, the organization of the world can be altered; salvation from the evil of the world is possible (p. 87).

However, "the specifically human mode of participation in reality" is consciousness (1977, 4). Human consciousness is "an event in the reality of which as a part it partakes" (1978, 4). Thus, human consciousness cannot separate itself out of reality and then see either itself or the rest of reality as objects. Human consciousness remains an event in a context that is both finite and infinite. To make of itself an object over against reality is to not see the whole of its situation; and to not be able to see the whole of its situation is an essential aspect of its situation—the essential infinite quality of consciousness. In believing that the reality in which it exists can be objectified and systematized, the gnostic engages in idolatry, treating "what is but an abstract aspect of some larger whole as though it were the whole itself, or at any rate something concrete" (Ogden 1982, 29).

Moreover, gnosticism is a rejection of philosophy itself, for thinking that such systematic knowledge exists undermines that authentic transcending sense of the awe of existence that constitutes the opening that keeps on opening. Voegelin writes reverently about the "responsive openness to the appeal of reality" and openness to the ceaseless action of consciousness "expanding, ordering, articulating, and correcting itself" (1977, 4). In sum, gnosticism is a denial of the whole of human consciousness, a denial of "the specifically human mode of participation in reality" (ibid.).

CONCLUSION

The essential point is that from the perspective of the self, consciousness in its fullness does not exist. For the self (not the whole person or necessarily the individual but the self), there is no context in which the self partakes, no context that includes the transcending infinite as well as the finite. The self is a case of what Voegelin calls a restriction of the horizon of consciousness (1977, 4).

The importance of this restriction of the horizon of consciousness is that it provides a link between consciousness and the political movements that Voegelin was intent upon understanding: communism, fascism, national socialism, and racism, and also constitutionalism, liberalism, and authoritarianism (1977, 3). These movements are to varying degrees expressions of the self, expressions of the restriction of the horizon of consciousness. Thus, they cannot be understood fully as ideas or as ideology; they must be understood as human actions flowing out of selfness, the denial of consciousness in its fullness.

These movements cannot be fully understood, therefore, by selves—those who as subjects would objectify these movements. Investigators of these movements, in addition to conventional historical and comparative study, must examine their own selves, conduct the anamnestic experiments and philosophical meditations to comes to terms with their own relationships to consciousness, their own modes of participating in reality (see Tulka 1987).[5] Only after having conducted such experiments is the investigator in a position to understand these political movements in their fullness.

A theory of consciousness is not just essential for such understanding of political phenomena. It is unavoidable in the sense that "all philosophizing about consciousness is an event in the consciousness of philosophizing and presupposes this consciousness with its structures" (1978, 33). There is no pure consciousness of philosophizing; such consciousness is that of a human being still within the context that is transcendent, as well as finite. That context constitutes consciousness, which is preferable, Voegelin says, "to a reality cognitively constituted by a transcendental ego" (1977, 12).

Plato makes this point in the *Gorgias* via the symbolism of the Last Judgment (1959, 111). The Judgment is, in effect, the evocation of consciousness or selflessness; for it imagines the observation of one's entire life by an omniscient judge rather than by a self. Consciousness includes the possibility through experience and meditation (especially through myth and philosophy) of entering such a state. The nature of reality (consciousness) is itself the openness of selflessness. There is no place to go with such opening; such opening does not lead anywhere but to further opening, to the awe that kindles the philosophic life, a life of wonder, not a life of answers (except as those "answers" cultivate openness). We may say that such openness is consciousness realizing itself, the space of abeyance of the self of human beings.

What is written here or anywhere by anyone seems subject to the context of consciousness and its structures. This leaves us where we should be—wondering. The kind of wondering in which Voegelin leaves us is the kind that could not have been a party to the destructive mass movements of communism, fascism, and so on. Indeed, the experience of the other and the experience of the self as self are mutually exclusive. "The problem," Voegelin writes, "of the Thou seems to me to resemble that of all other classes of transcendence" (1978, 23). Consciousness of the experience of the other is a "given of experience from which one may start out but behind which one may not retreat. The capacity of transcendence is a fundamental character of consciousness just as much as is illumination; it is given" (ibid.). Consciousness is given, while the self is an attempt to displace consciousness, transcendence, the other, and philosophy.

Is Voegelin pointing to the possibility of a political community based on the opening of the soul to the transcendent source of order? Such a question perhaps misses a more fundamental point. Without the transcending experience of the awe of existence, one is likely to see politics *only* as the expression of the reality of selfness. (Voegelin accuses Hobbes of this [1952, 180]). Such an understanding is flawed. It is not possible fully to grasp the nature of the self from within selfness. Reality includes both self and consciousness where consciousness is understood as the reality that includes more than self. A theory of politics based exclusively on the experience of self, consequently is incomplete and therefore wanting. Inasmuch as the whole of political life includes and reflects consciousness in its finite and infinite character, one must knowingly experience consciousness in its full range of the finite and infinite to be able to speak authentically on the nature of politics. A theory of consciousness equal to the task of being human and conducting political analysis entails not only ideas about consciousness but experiments whereby the investigators examines their own selves and come to appreciate consciousness in its infinity and finiteness.

Finally, as we noted at the outset, the point of political action is not only to accomplish particular common purposes but also "to speak solemnly in everyone's name, in the name of society, about what it holds dear" (Nozick 1989, 289).

Participation in consciousness (selflessness as "participation between partners in the community of being" [Voegelin 1987, 15]) entails profound recognition of the other. Such participation enhances the possibility of more inclusive joint political action. Moreover, such participation is acutely aware of the persevering nature of the self and its political significance. Transcendence (philosophy) results in more of both reality and possibility.

Notes

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1. For secondary sources on Voegelin's theory of consciousness see Cooper 1986; Germino 1982; Havard 1971; Keulman 1990; Porter 1975; Sandoz 1981; Webb 1981; Webb 1988. None of these equally emphasizes the character of the self as an obstacle to the appreciation of consciousness in its fullness.

2. It goes beyond my scope here to reflect upon both the nature of property in connection with the self and liberalism as a social and political expression of selfness.

3. Ellis Sandoz emphasizes (1) that Voegelin's philosophical inquiry is "one of the modest remedies against the disorders of the time" (1987, 4; read *of the self*); (2) that the problems of transcendence were the decisive problems of Voegelin's philosophy, citing his words, "Philosophizing seems to me to be in essence the interpretation of experiences of transcendence" (p. 5).

4. This is not to be confused with the thing over against which the self is a subject.

5. Consider the following exercise: "Note the kind of mental events that come up in the mind and their relationship: images and feelings, intentions, thoughts, judgments, etc. Which elements are dominant and which supportive? How do states such as tension, conflict, boredom, calm, clarity, or appreciation arise? Keep a journal in which you record your discoveries: an annotated catalogue or inventory of the contents of your mind" (Tulka 1987, 15). The design of this exercise is to enable one to be free from unconsciously identifying with the content of the mind, that is, free from the self. The exercise thereby opens up the possibility of participating in the whole of consciousness, rather than in the deformed reality of the self.

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POLITICS AND THE PROBLEM OF TECHNOLOGY: AN ESSAY ON HEIDEGGER AND THE TRADITION OF POLITICAL PHILOSOPHY

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I examine Heidegger's postmodern interpretation of technology, with an eye to exposing its weaknesses. I do this by showing that his view entails an understanding of the tradition of political philosophy that cannot do justice to that tradition's own understanding of the character of technology. I consider first Plato and Aristotle and then Hobbes and Locke in order to suggest that Heidegger's view that modern politics are stamped *by technological metaphysics can be challenged on two related grounds: (1) it assumes incorrectly that the tradition is dominated by dogmatic metaphysics; (2) it prevents us from seeing how some in the tradition both understood and doubted the possibility of a technological stamp. I then suggest an alternative view of technology that might account better for the character of technology and political life.*

Recent thinking about technology and its implications for politics has taken a postmodern turn. In its modern formulation, the danger of technology is the possibility that in the scientific age we will succumb to the consequences of our most impressive intellectual achievements. First, we were poised on the brink of nuclear destruction. Now, as that threat fades, we and the world face new dangers, this time springing from technology used with the best of intentions. Disaster looms because technology enhances our powers of control and because modern science, the theoretical ground of technology, understands human nature to be as manipulable as matter in motion. Under the spell of these forces we risk self-inflicted deformation and brutalization, all in the name of human comfort, freedom from pain, and variety of choice. From this modern point of view technology compels us to remember our essential humanity lest we fail to control the technical means that threaten fundamental values (e.g., Jonas 1984; Kass 1985). The modern age is not just the age of science; it is the age of the potentially beneficent tension between science and humanism. If we understand how science and humanism contend within the progress of reason, it will be possible to exercise responsible political choice today.

The postmodern view is perhaps best expressed by Heidegger's comment that "the essence of technology is by no means anything technological" (Heidegger 1977, 4; 1967, vol. 1, 5). Technology is not the sum of machines and techniques that we must learn to master and use as a neutral set of means lest we forsake our humanity. Technology is not even something we need to "do" anything about. Rather, it is just what we have now become. And although it opens our eyes, it does not compel us to seek out a reinvigorated humanism. On the contrary, technology discloses to us that humanism is the proximate source of technology. Technology is indeed a fateful danger; but it shows us that within the tradition of rationalism, the essences of technology and humanism are the same. As the result of that tradition, our world is now stamped by technology. But the problem of technology has nothing to do with recovering humane standards for political (and technical) choice. That problem is less a challenge to be met and overcome than a sign that we are on the verge of an age in which all of our categories of political choice and evaluation have become exhausted. Whatever it might be that technology compels us to do (especially as regards politics), it will not be to recur to the contending elements of modern rationalism.

I wish to suggest that Heidegger's view of technology is untrustworthy and that an indication of its weakness is that it misinterprets the theoretical tradition that it claims *stamps* the modern era. His view obscures the extent to which the most important thinkers in that tradition distinguished between political life and the activities of thought, or the power of any tradition. Heidegger's view obscures the extent to which these thinkers were themselves aware of the phenomenon of the technological stamp and knew better than Heidegger himself the limited power of any such stamp to form practical life. Moreover, they understood how the stamp is an ineradicable, natural fact of productive life. I argue that we can learn more about technology from the thinkers of the tradition than from Heidegger, and then suggest some lines along which we might think to recover and develop an alternative view of the problem of technology that we face.

NIHILISM AND BEING

For Heidegger, modern science and technology are rooted conjointly in the metaphysical worldview. According to this view, the world is conceived of as a spatial whole made up of three parts. These three parts are the demonstrably knowable and eternal *ground* (objective laws of matter) of every particular entity; all the particular *entities;* and the human *subject* who discovers the objective ground and lives among the various entities. In discovering the ground, hu-

man beings become able completely to manipulate and transform the various things in nature. In doing so they endow the things with values. Science tells us that only humans, not other entities, have value, and that humans *give* the world its meaning or value as their knowing discloses the world's manipulability. Science is thus humanistic to its core. In its light every particular entity stands neutrally (not as nature) between the necessity of its objective ground (matter in motion, extension, etc.) and the freedom of subjective human art, between fact and value. When understood as the indubitable vantage point for universal, scientific (mathematical) knowledge, subjectivity is the certain and fixed beginning point for discovering the objective ground of manipulable things. But when experienced as the animus of the individual soul, subjectivity is merely arbitrary. Thus, facts are taken to be objective, and values are taken to be merely subjective, thus revealing the essential kinship of subjectivity and manipulable entities, both of which have no fixed character or nature. We cannot hope for salvation from Kant, says Heidegger, because Kant's account of subjectivity—as transcendental unity of apperception *and* as the free legislation of the absolute moral law—itself assumes dogmatically a metaphysical conception of the subject. Thus, free subjectivity and the manipulability of entities turn out to be the same in comparison to the necessity of objective ground. In fact, for modern science there are no essential differences between subjectivity, objectivity, freedom, and manipulability. These aspects of "reality" are actually united in a technological understanding of being: subjectivity is the Archimedean point for uncovering objectivity (Descartes); the doctrine of moral freedom dogmatically presumes that very subjectivity (Kant); and the identification of being with the knowable and changeless entity (objectivity) grew out of the problem of fixing stable grounds for the arbitrary manipulations of human art (Plato and Aristotle). Even for pure natural science "to be" is "to be the ground of the manipulable." Science is humanistic and humanism is technological. (See Heidegger [1927] 1972, 89–101, 202–208, 317–21; 1962, 122–34, 246–52, 364–68; 1982, 112–17; Schürmann 1987, 75; Zimmerman 1990, 157–63, 196, 222–23.)

But our view of the world (including human beings) as manipulable, as the object of control, is not itself within our control. Modern technology is rooted in the metaphysical conception of being that began long ago with Plato. Metaphysics conceived of being in terms of one particular kind, or "domain," of being—objects permanently present before knowing. The task of metaphysics was thus epistemology, which aimed to establish the conditions for certain knowledge of the objects located in the external world. But metaphysics was dogmatic because it merely assumed that we have access to a privileged position outside the presuppositions of a given practical world, because it assumed and thus missed the character of "being-in-the-world," and because it assumed that being is an entity or thing. Metaphysics assumed an impossible independence of theory from practice and confused one domain of being with being itself, forgetting that being neither is an entity or thing nor is identifiable with one or another or even all of its domains, which include objects of knowledge, tools, human beings, the earth, the heavens, and the gods.

Under the sway of metaphysics, the domains of being are so conceived as ultimately to produce the domination of all by one—by objective manipulability. Thus, metaphysics comes to a peak in modern technology, for whose conception of being—which Heidegger calls the *enframing* (*Gestell*)—every entity, whether theoretical object, human being, earth, sky, or god, is taken as the manipulable stuff—the *standing reserve* (*Bestand*)—of an endless industrial business transaction (Heidegger 1977, 17–28).[1] In the full-blown age of technology, the phenomena of art, politics, and the gods are flattened in being understood as the objects of scientific knowledge, in the light of which they become merely useful. In such an age genuine creativity, reverence, loyalty, rootedness, and the full possibilities of astonishment and estrangement are obliterated in a cybernetic swirl that spares nothing, that annihilates everything. The age of technology is the age of the last man, for whom the whole of nature and every human being is the standing reserve of a plethora of industries: publishing, war, travel, entertainment, agriculture, concentration camps, education, and so on. And in this age there are no differences among the great competing social and political systems, all of which are bound together in the embrace of global technology.

Yet despite technology's being the dark night of the world, Heidegger tells us that it is a "danger that saves" (1977, 28–35); for the result of technology is nihilism, the frame of mind that forgets completely the differences among the domains of being and between being and its domains. Nihilism, which in its forgetting says only "It is not" (as in positivism and relativism), actually discloses the negative character of being, the fact that being *is not* an entity or category of entities. It thus discloses that being is the temporal configuration of the separate domains of being—requiring their nearness and farness—and that, as such, being is finite and singular. It discloses that the essence of being is the arbitrary occurrence or happening of one or another utterly unique configuration. Metaphysics, itself an occurrence of being and rooted in everyday experience, hides the essence of being as occurrence. It thus represents the withdrawal of being, which nevertheless always has the power of disclosing itself and thus upsetting our everyday and theoretical understanding of the world and orientations within the world. Technology is the metaphysical appearing of being that ultimately discloses the radical finitude of metaphysics itself.

Technology forces us to see that metaphysics and technology are themselves finite being-dispensations. As such, they are not eternal; but neither are they under our control. Modern technology, the *Gestell*, is but *one* way that being is unconcealed; and

while it is necessary as a dispensation of being, its necessity is not that of a species' relation to an unchanging genus. This fact lets us account for the uniqueness of modern technology (for the fact that the Baconian–Cartesian project for the mastery of nature is not *essentially* akin to, say, ancient *techne*), despite the rootedness of technology in the metaphysical tradition running all the way back to Plato. The heterogeneous epochs within the metaphysical tradition are each of them new withdrawals of being that undermine the previous metaphysical ontology. But when grasped in the glare of nihilistic technology, the several metaphysical epochs point not to some common ground of human history but to the radical finitude and singularity of every "tradition," including technology and metaphysics. Modern technology is metaphysical and is for that reason a species, or kind, of metaphysics. But the essence of modern technology is to disclose the historical finitude of the very categories of essence, genus, and species (or kind).

When the finitude of being announces itself out of technological nihilism and the exhaustion of metaphysics, we can prepare ourselves for a new relationship to being. This will be one of expecting altogether unpredictable and indeterminate configurations or play of the domains of being. As we are now in the technological release from metaphysics, we can ready ourselves for wholly new occurrences of the human and the divine, the public and the private, the natural and the artificial. These occurrences (the play of being) will be serious, however, because they will always announce the finitude of being and thus shatter an everday way of thinking that for the most part convinces us that being is not finite and that the world is a three-dimensional, scientifically knowable whole that surrounds us. Such readiness for the new is the saving gift of technology over which we have no power. For Heidegger, the challenge posed by technology is not to rediscover our values or invent new ones. Whatever the politics of the future life will be (it will be neither democracy nor communism, which are technocratic), our genuine stance for now is one of expectation, a readiness that holds itself open for new configurations of being whose character we cannot know or force or control.[2]

Now to speak rather generally, Heidegger's account of technology has had influence far beyond those who would identify themselves as Heideggerians. The most powerful recent (and explicitly Heideggerian) version of this argument is expressed in Reiner Schürmann's deconstructive anarchism; but it certainly influenced Arendt's account of modern democracy and liberalism and could be said to have made its mark on the contemporary critics of "foundationalism," for whom getting a bead on politics requires seeing the tradition's search for metaphysical grounds as it has now revealed itself—as the last repository of vain and disfiguring illusion. But setting aside the important question of Heidegger's contemporary legacy, the political implications of his account of technology are clear and familiar enough. They tell us that modern politics, both liberal and socialist, are in the grip of metaphysics. This grip began with Plato and received its decisive technological form in the seventeenth century, which saw the explicit linking of philosophy and politics to the scientific project for the conquest of nature. Modern life, whose roots reach back to classical antiquity, is thus stamped by the technological frame of mind, which so forms modern politics as to distort and truncate the possibilities of human experience. The result is the totalitarian disaster of modernity, in which labor, citizenship, statesmanship, art, and thought—the essential elements of any possible world—are impoverished, if not obliterated, by the very forces we pursue to cure our telltale malaise. Clear thinking shows that we can only hope for the advent of something altogether new and that with the technological self-destruction of the metaphysical tradition we can experience a liberating transformation of political life.[3]

POLITICS AND PHILOSOPHY

What does it mean to say that nihilism announces the end of technology's (and metaphysic's) stamping of an epoch? How do we recognize the pathology of this stamping so that the freedom conferred by its demise can come to light? We can fathom the matter by considering again Heidegger's description of the *Gestell*, a dispensation of being that answers to the productive impetus of human art. The world of this epoch is modeled after the system of (both capitalist and socialist) industrial production, the aim of which is the provision of unlimited goods for the satisfaction of unlimited needs. It is the world formed by the Baconian and Cartesian project for the mastery of nature and the "relief of man's estate." It is a world, in short, where there are no scarce goods, where absolute plenty makes it possible for social and political life to be ordered as a self-regulating, or cybernetic, system.

But why must such a world flatten the human landscape, reduce human beings to mere means, and banish the gods and every mystery? It must do this because to render common life translucent and perfectly orderly it is not enough just to satisfy pressing material needs but it is also necessary to *overcome the need* for goods that are inherently scarce because they cannot be conceived of as falling within the cycle of production, reproduction, and consumption. These are the *noble and beautiful things*, the goods whose rarity and whose opacity to clear definition give rise to perplexity about justice, to serious political competition, and, ultimately, to faith in the gods. Heidegger's accounts of *Gestell* and *Bestand* are illuminating because they suggest that it is openness to the noble and the beautiful things that constitutes, at least in part, the essence of human experience. When we are revolted by the Heideggerian *Bestand*, we supply evidence that such goods exist, that human being is experienced in terms of them, and thus that technology can overcome scarcity only by denaturing

human life. Moreover, we disclose that the impetus for technological mastery is the desire for justice provoked by the scarcity of such goods, and that the stamp of technology—the domination of utilitarianism, instrumental method, and universal *mathesis*—is the triumph of egalitarian justice: in a world where nothing is scarce because everything can be produced, all can be had and used equally. (It is no accident that Heidegger did not advocate democracy.)

In this light we might question whether the inegalitarian ancients—against their intentions and by way of metaphysics—actually set the stage for such a technological project; for when viewed apart from the Heideggerian interpretation of the tradition, the ancients turn out to have thought more clearly about technology than we are disposed to think and were themselves aware of the link between technology and the tendency of thought to dogmatic metaphysics. To see this, I shall start by turning to Aristotle, who begins the *Politics* with the well-known discussion of the origins of the polis and the distinction between mere productive life and the good life (1252a25–53a1). To appreciate the full meaning of Aristotle's teaching for our problem I shall consider this distinction very carefully, since it is rather less clear than first meets the eye.

At first glance it seems obvious that social life begins with the dividing of labor to meet the simplest and most pressing needs, that is, to provide for mere life. Speaking strictly, however, there is an imaginable alternative to the most rudimentary common life, an alternative considered by Plato (*Republic* 369e–70b), as well as by Aristotle (*Politics* 1253a1–b1; see also Rousseau 1964). This is the life of isolated self-sufficiency. No doubt, for anyone but a god such a life, engaged almost exclusively in attending to the very simplest needs, would be very poor and harsh. But this possibility shows that the really pressing needs are few indeed. And in its light the simplest division of labor comes into being not for the sake of mere life but for the good life. As measured by isolated self-sufficiency, even the very first things produced by society are luxuries that transcend simple need. Thus, even the proximate cause of common life is the desire for goods that lie beyond the most immediate need: human industry is provoked at the very beginning by the desire for the good, as opposed to the merely useful—by the desire for the noble and beautiful things.[4] But while common production is even at its inception drawn forth as production for such goods, it is at that early time close to the all-absorbing leisurelessness of solitary life. The openness to luxury is thus experienced as the press of need. This is, of course, why Aristotle says, with common sense, that political life comes into being first for the sake of mere living rather than for living well.

Yet given the paucity of the very simplest needs, scarcity exists only concurrently with the desire for luxury and the capacity to produce it. Strictly speaking, scarcity could be overcome (and freedom could be possible) not when some level of productive capacity has been reached but only with a limit on the desire for luxurious acquisition. For Aristotle, this limit requires drawing a clear distinction between needlike luxuries and goods that are genuinely noble and beautiful. Therefore, Aristotle follows his account of the origins of the polis with a discussion of the natural limits to acquisition (*Politics* 1256b40–1258a18). For Aristotle, there is such a limit, situated between penury and unlimited acquisitiveness. But this limit is neither easy to see nor realizable by all, because it is in conflict with material production itself.

The reason for this conflict is that if common life comes into being for the sake of the good and not the useful, then the best life would be free not just from the isolate's needs but also from the desire for needlike luxuries that motivates productive life. From the standpoint of such freedom, even the rarest or finest of such luxuries are really not ends (as opposed to useful means), because they are, ultimately, inseparable from the most pressing bodily desires. The best life would not even be the one devoted to the apparently free but really useful activities of politics or moral virtue. Rather, it would be genuinely playful like the life of Socrates, who did not work (though no aristocrat), avoided politics and everything else practical, called the moral virtues vulgar, and eschewed the goods that for almost everyone made life worth living. For Aristotle (*Nicomachean Ethics* 1176a30), as for Plato (*Republic* 500d4–8, 504d3–e3; *Apology* 31a7–33a1), the best life—the one that is an end in itself and useful for nothing—is philosophy, the most uncompromising questioning, wakefulness, and self-knowing. And such a life is unproductive of either useful material things or actions useful for the common good.

By Socrates' own account it was his unstinting questioning of the politicians', the poets', and the manual artisans' claims to practical wisdom (about the noble and good) that got him into trouble (*Apology* 22a–24c). But the leisure of his investigation was at least as controversial as any opinion he might have suggested or criticized. In the final analysis, Socrates' *endless* questioning conflicted with the urgency of all the arts from the productive to the political and poetic. No wonder that he so irritated the practitioners of these arts, to whom he appeared not just unjust and impious but lazy and indifferent to all the goods that for them are the ends of life! It is of utmost importance that in the dialogues we never hear that Socrates, of low birth and poor, performed any productive work.[5] A city of Socrateses is an impossibility; for such leisure as his (one that would attract every moment of waking life) could never be universal, lest production disappear. Consequently, since men live for the good and not the useful and for what is at least possibly within their grasp, most will produce for the sake of needlike luxuries or the performance of useful deeds but will take them as self-sufficient ends in themselves. They will confuse the necessary and sufficient things. And to the extent

that they live for productive leisure, most will view the truly useless life—the life lived beyond the pursuit of useful luxury or practical virtue and exposing the spurious self-sufficiency of such pursuit—with suspicion if not with anger.[6]

For the very sake of production, human beings must tend to confuse their own useful and productive activities with the noble, beautiful, and good things, whose perfection and self-sufficiency are by nature outside the range of the producible and usable as such. And since these good things are not self-evident, there is a powerful tendency for each art or practical activity to see itself as the self-sufficient, ruling end of the productive order. Even so, no productive art (e.g., medicine, military strategy, moneymaking) can cease to be a useful means to something else, and no virtuous deed can be wholly divorced from service to the common good.[7] No claim to being the end of the productive order is beyond controversy. If production is to occur every one of the productive arts and practical activities (including moral virtue, sacerdotal mediation, and political deliberation) must harbor the potential claim to being an end in itself. In other words, since the goods calling forth the production of artificial means are natural ones, their character of being non-reproducible, useless, and indeterminate results in their being scarce. Consequently, there will always be controversy about their distribution. And since the productive must confuse the good and the useful, all material and artificial means can in practice take on the character of the things that are good by nature. Thus, any order of arts, no matter how productive, will be moved by the attractiveness of goods whose character can rub off on even the most common and reproducible of material means, which can become virtually if not actually scarce. Even in a world where more such means could always be produced, as in a technological utopia, it will be possible (indeed, necessary) for some, convinced of their superior dignity, to claim more of them than others. We cannot assume that in a condition of plenty there will be no serious disputes about the distribution of means. Nor can we distinguish in practice between the goods of the body (means) and the goods of the soul (ends), hoping that when the former are plentiful the latter's scarcity will not matter very much. Rather, disputes about ends will become as contentious as those regarding basic needs in less affluent times, and prosperity will require that we treat plentiful means as if they were natural ends. Such will be the case so long as human society differs from the bees (see Aristotle *Politics* 1235a10; Hobbes 1981, chap. 17).

For Plato, the problem of Socrates disclosed the paradox of human productivity: that the productive must despise the uselessness of the self-sufficient ends for which they work. The ultimate consequence of this paradox is that production transpires within an order that is potentially unstable and given to partisan violence. Aristotle saw things much the same way. He argued that while production requires openness to the noble, beautiful, and good things it also involves the confusion of the necessary and the sufficient. For Aristotle, this confusion leads to the possibility of taking life in an explicitly technological sense, where the good is understood to be the endless, needful acquisition of mere means. Human artfulness thus involves the paradox that in order to get beyond mere life there must be those who take the good to be unlimited acquisition and exchange, that is, mere life writ large. This technological view is inseparable from the counterthrust of the noble and beautiful things, whose scarcity and indeterminateness make political life a serious problem (*Politics* 1256b40).[8] For Aristotle, it is simply not possible for practical life to be completely formed by a particular understanding of the good or to be wholly determined by the technological conception of life that can always accompany such an understanding. To put the matter more generally, for Aristotle, it is impossible for life to be so well determined by knowledge (whether theoretical or practical) that the fragility and contentiousness of politics might be overcome. Both Plato and Aristotle judged the complete stamping of life by technology to be impossible, not because they failed to imagine the project for the conquest of nature[9] but because they thought there were always goods in essentially short supply regardless of our productive capacities.

Now it follows that for the ancients, the fragility, contentiousness, and opacity of political life are not evident to most of those who actually live it. For the most part, certain opinion (not to mention faith) binds political life together from day to day; and citizens live as if that opinion were rooted in a universe that is perfectly clear. But since the truth about the whole is not prior to politics (as if one political opinion could be established on demonstrable grounds), the everyday certainty of political opinion is always haunted by its own finitude and openness and by the unstable partisanship latent in any common good. For the ancients, it is not possible to establish the priority of thought to politics (or vice versa), because political life is at once driven by the need to grasp an elusive whole and (for the sake of material production) intolerant of genuine thought, which is the unstinting activity of doubt and questioning.

Moreover, for Plato there is no human inquiry that does not grow out of the urgency of political life.[10] The truth is an issue only for the being who has language and for whom the truth can matter. And it can matter only because productive life is rent by tensions such as those between rich and poor, noble and base, between public and private life, sacred and profane, war and peace, thought and action, law and morality, and so forth. Where there is no production for luxury (as in the imaginary age of Cronus or the City of Sows), there is no politics and hence no philosophy (*Statesman* 271c–73e; *Republic* 369b–76b).[11] For Plato, politics is the horizon through which the whole occurs. Moreover, this horizon is inseparable from the being of the whole in two respects: (1) the whole *is* as what answers to our need to understand

the just, the beautiful, and the good; and (2) the whole is intelligible because the problems to which it answers are finite and related to each other as a whole, because in politics there are only so many problems (see *Apology* 22d–e). This is why, although for Plato truth and politics are inseparable, it does not follow that the whole is contingent through and through and thus an artifact that could have an infinite number of alternative forms.

But that the problems of politics are limited does not mean, for Plato, that the whole can be known with sufficient clarity to answer to the needs of practical life (see *Republic* 500d4–8, 504d4–e3, 507a1–5, 509b2–c11; *Protagoras* 329b1–34c6, 349d2–51b2, 360e6–62a4; see also Aristotle *Generation and Corruption* 315a26–16a14). Problems are not answers. Although the whole occurs through the fabric of political opinion that points to it, the whole is beyond the knowledge to which opinion aspires. First of all, while there are only relatively few political problems, the concrete circumstances out of which they arise are never the same. Consequently, if we had a demonstrable grasp of the whole, it would for that very reason be too fixed to give us answers to the specific problems that prompt our attempts to know the whole in the first place.[12] Thus, to know that the noble is superior to the just would not tell us certainly what is in fact noble or just in a given case or whether in the circumstances we should prefer one to the other. Second, for an end genuinely to command or guide our action, that end must be genuinely independent both of our art and our powers of knowing. The existence and natures of such ends cannot be determined by what can be made and understood, lest they be merely ours and created, not "their own." Thus, the whole composed of them does—but yet cannot—occur *as* what answers to our political perplexities. In its appearing it hides itself. And yet we cannot reason from this mysteriousness to the whole's being purely artificial without dogmatically granting to ourselves what cannot be had (i.e., a privileged, epistemological position outside of the presuppositions inherent in the contingent opinions through which the whole appears), that is, without dogmatically assuming about the nature of being that what cannot be known with demonstrable certainty either cannot be or must be artificial. For Plato, the whole occurs only with human experience. But from this fact we can reason no more to constructivist moral relativism than we can to comprehensive teleology. For Plato, the whole occurs—by way of political experience—as a morally inescapable mystery and yet is intelligible as its independent but elusive being shines through the rational capacity to doubt, through our ability to recognize presupposition and dogma.

But again, all this is so only for the most complete wakefulness and questioning, a possibility illustrated (but not "proved") by the fact of Socrates' life. There is a difference between what the whole is—its mysterious presence before genuine questioning—and how it appears to those through whose partisanship such rare questioning occurs. This partisanship is the inevitable result of human productivity. If production is to happen at all, the partisan, unlike Socrates, takes the whole as both knowable and known—so well known in fact that in its light the controversies (and the finitude and fragility) of political life just disappear. Socrates' life is not wholly *strange* to the productive, for they claim to know the good; but it is intolerable to them because it exposes their ignorance of their own ignorance.[13] For Plato, there is a fundamental tension between genuine thought and the inevitable requirement of production—the partisan's need and claim to know. Epistemology is a fact of productive (technological) life, with which Socrates had nothing to do.[14]

For the ancients, that philosophy occurs only with politics does not mean that thought is subordinate to practice, is merely partisan, and is unable to provide guidance to moral life. Independent thought *is* possible, as is shown by the example of Socrates. The life of such thought is the only one wholly free from partisanship; and as such, it is the ultimate model for moderation, without which statesmanship is impossible. Decent politics (not necessarily a genuine common good) is thus dependent on thought that grasps the whole. But this grasp cannot be identified with the cognitive pretenses of epistemology or metaphysics; and its influence can at best be indirect because it can never provide the certainty demanded by productive life. Politics is never as soft and pliable and playful as is genuine thought; to think otherwise would be to imagine life without material production. But at the same time, thought can never do without the spirit of gravity, for two reasons: (1) its own safety will require that playful thought recognize the partisan and deadly moral seriousness from which it arises; and (2) if it is not to succumb to dogmatism, it must be attuned to all of the phenomena of practical life, including those serious things (like the noble, the beautiful, and the just) that can be seen fully for what they are only in their openness to nature, or the *eide*, or the gods. For Plato, thought understood as unadulterated playfulness would be no different from thought understood as unrelieved seriousness, because both stances reflect the dogmatic partisanship essential to production. For Plato, the life of the mind and moderation transpire precariously between two conceits rooted in productive life: the idea that politics *is* ordered after the model of a *knowable* whole and the idea that it *can be* ordered after the model of a *constructible* whole.[15] Both conceits are ultimately technological and dogmatic, since both aspire to find justice in a perfectly adequate supply of the noble and beautiful things, which are in truth inherently scarce and indeterminate goods. For Plato, as for Aristotle, the desire for the technological (and metaphysical) stamp of knowing and making arises out of the very condition (the scarcity of the noble and beautiful things) that renders such a stamp impossible. The ancients did not dogmatically presume productionist metaphysics (Zimmerman 1990, 157–63, 196, 222–23); but, more important, it is possible to see the character

and limits of technology within their conceptions of being and nature, which do involve the principles of form, ground, and cause.

THE CONQUEST OF NATURE

It may be that classical rationalism does not bear the technological stamp of productionist metaphysics. But such metaphysics could well have been nascent in the ancients, not so much in what they presupposed as in what they did not think sufficiently. It could then be said to have flowered in the modern thinkers, who, while they opposed the ancients in embarking on the technological project for conquering nature, still did so within the categories (form, ground, and cause) of rationalism in general. After all, the crucial modern turn occurred with Bacon and especially (for Heidegger) Descartes, whose project for thought appears in practice in the systems of Hobbes and Locke. In the latter systems we should doubtless see the fundamentals of technological politics: robust utilitarianism, radical individualism, preoccupation with acquisition and production and private property, opposition to social holism and moral virtue, the radical distinction between the state and civil society, aggressive secularism and hostility to the "divine," and the reduction of human personality and community to the typical liberal abstractions. In short, we should see "fallen" everydayness, embedded in spheres of life metaphysically conceived as connected by causal grounds, which could be exploded either by authentic politics or by *Gelassenheit*.[16] Thus, for Hobbes and Locke, commonwealth and citizenship are wholly artificial, intended not to encourage political or moral virtue but rather to reduce the common good to the institutional requirements for the selfish pursuit of long life and demystified, commodious living. These early liberals seem to epitomize the technological hope that politics can be reduced to a machine supporting civil society, that artificial realm where once-conflicting-but-ennobling public ends are wholly privatized and trivialized. Indeed, the character of their politics appears to highlight the technological essence of modern rationalism.

I shall argue that neither Hobbes nor Locke was in the grip of technological thinking, despite appearances to the contrary. Consequently, their politics are not reducible to mechanical technique and in fact require and produce human experience outside the realm of technological, utilitarian calculation. Moreover, they understood the nature and limits of the technology they (especially Locke) did embrace and were aware of the circumstances that led them to their modern avowal of the conquest of nature.

We begin by noting that while Hobbes does contend that politics merely serves civil society, the relation he sees between them is far less technological than first meets the eye. He did not reason from the natural condition (with its free, equal, autonomous individuals moved only by rational self-interest and no partisan or moral conception of the public good) to the spontaneous creation of civil society by all of its members. His political science was directed not to them but, rather, to the few enlightened by the scientific teaching of the state of nature: to him "that is to govern a whole nation" and to the students in the universities, that is, to the clever who would also be the most ambitious (Hobbes 1981, intro., chaps. 21 and 31, review, and concl.; see Tarcov 1984, 42–51). Hobbes knew that at least some individuals in civil society would press controversial political opinions about what have been called the noble and beautiful things. He understood clearly the political importance of opinionated public spiritedness, both as a problem and as an element of a rational political order. Despite Hobbes's clear intention to deflate the character of rank in human life, it is important to recognize just how political, nontechnological, and even noble his understanding of representative government actually is.

For Hobbes, human beings are *not* political animals in the sense that they reach perfection in the activities or virtues of public life. The latter is the clear intention of the doctrine of the state of nature, and any modern liberal must adhere in general to this view. For Hobbes, political life and citizenship are artificial means to the natural common ends of self-preservation and commodious living. In principle, sovereign political power represents individuals not by standing for one or another opinion about the particular ends of life or even by brokering them but, rather, by providing the conditions under which such opinions concern merely private affairs. But even though the sovereign's task is not to determine the citizen's private ends, Hobbes knows perfectly well that any law or act the sovereign judges necessary for the common good of civil peace will tend to favor one particular end rather than another, will answer to some opinion about the goodness or badness of such ends, and will thus benefit some individuals more than others. The task of his political science was to distance political choice from considerations of such ends and benefits, a task accomplished by his distinction between the artificial opinions of the artificial citizen and the natural opinions of the natural individual.

Hobbes argues that if individuals are to escape the prepolitical war of all against all they must submit to absolute sovereign power originating in the consent of each individual. This power is given its particular form (one, few, or many) by the arbitrary decision of the majority of those who consent to leave the natural condition. Each individual, bound to a particular form by the majority, authorizes the sovereign's actions as his own, willed by each as necessary for guaranteeing private contracts and hence for justice and peace. This authorization makes it impossible for the sovereign himself to be accused of an injustice. Therefore, *whatever* the sovereign does or opines in acting to secure peace in a particular instance represents the authorizing individual even if it conflicts with that individual's actual opinion about particular

ends (Hobbes 1981, chaps. 16–19). In describing the natural condition, Hobbes says that because individuals are equal, each takes his own opinions not just as superior to others' but as applicable to them as well. Thus, in the state of nature the *equality* of individuals inclines each one of them to doubt that any others are equally wise, and the bolder few's love of glory and delight in conquest, as much as general competition and diffidence, are the causes of war (chap. 13). Natural individuals always have opinions about the noble and beautiful things, and these opinions tend to be contentious and political. And since citizenship does not transform human character, individuals continue to harbor such opinions after the establishment of civil society.

Now according to the doctrine of representation, when the ends preferred by a natural person conflict with what the sovereign deems necessary for peace, the sovereign's opinions become the individual's *as citizen* because that individual needs peace more than anything else; otherwise, the individual's opinions remain a merely private matter.[17] The sovereign thus represents natural individuals only by re-presenting some of their opinions, by filtering them through the artificial medium of citizenship. The sovereign's opinions thus do not mirror those of the represented, nor are the sovereign's opinions judged by considering the merits of natural individuals' particular conceptions of the good. Rather, their being accepted as arbitrary (as beyond considerations of justice and any particular good other than peace) is itself an important condition for civil peace. The difference between citizen and sovereign (aside from the sovereign's monopoly of coercive power) is that the sovereign's natural and artificial personalities and opinions are coeval, while the citizen is a natural person whose actions and opinions take on the artificial garb of citizenship only on the occasions when they come under the specific restraints of sovereign will and positive law. In a Hobbesian order, the extent of the positive law will be kept to a relative minimum, for the reasons that Hobbes lays out in *Leviathan*. Once politics is understood as grounded in consent and as instrumental to the natural liberty transpiring in civil society, then sovereigns will be interested in limiting what individuals do as citizens, and individuals will do in civil society what is expected of them when they are not behaving as citizens, namely, mind their own business (which is business).

But this does not happen spontaneously; for the political character of the natural person's opinions tends to cover up the scientifically disclosed teaching of the human heart, namely, the harsh truths that are uncovered by reflecting on the state of nature and that compel us to create the artificial citizen and the private realm of civil society: truths such as the natural right to self-preservation, the war of all against all, and the absence of a summum bonum. Consequently, for Hobbes, the will behind public law (i.e., the sovereign's will) must always be evident and awesome. In order for individuals to let sovereign power represent them, they must always be reminded of the harsh, prepolitical necessity requiring the sovereign's deeds and their confinement to minding their own business. Moreover, the sovereign's lessons must appear through his character as the authoritative measure of "opinions and doctrines," both secular and divine, and of "laws of honor" (Hobbes 1981, chap. 18). The sovereign's will stands in for the science that only a few can know. Thus, the sovereign's will (not his argument or virtue) dampens the political self-assertion to which individuals convinced of their superior wisdom and ends will be tempted. Their occasional experience as artificial citizens must continually remind them to love their natural, spontaneous opinions about the noble and beautiful things less than they fear the artificial, represented counterparts of those opinions.

Yet the fearsome authority of the sovereign is not intended simply to force individuals to mind their own business, against their natural inclinations; for in reminding the individuals of their true self-interest, the sovereign is able to enlist the individual as citizen to attend to the harsh necessities of public life and resolve inevitable political controversies, regardless of the inclinations (one could say "policy inclinations") of the individual's conscience. The energy of representation works both ways for Hobbes, from the natural person to the artificial sovereign and then back from the sovereign to the artificial citizen, who is thus not inert. When natural individuals make a covenant, they authorize the sovereign to "use the strength and means of them all, as he shall think expedient, for their peace and common defense" (Hobbes 1981, chap. 18). It is easy to forget that the doctrine of freedom of conscience—first worked out by Hobbes—was intended not only to prevent the voice of God from meddling in political affairs but also to indemnify the individual before God of any iniquity done as a citizen.[18] The citizen can put his shoulder to the sovereign's wheel, live with the majority's will, or accept a compromise of fundamental moral principle because God judges the conscience of the natural soul, not the actions of the artificial citizen.[19] Thus, even in the case of Hobbes, the representative state is a subtle machine intended not only to manage the partisan tensions of political life but also to establish the conditions for engaging the energies of individuals, as citizens, in this management.[20] This is, of course, a far cry from Heideggerian authenticity. But it is less dependent on the saving power of technology and far more engaging of sacrifice and the political virtues—and thus ultimately of the attractiveness of the noble and beautiful—than is granted by the postmodern account of technology. One might say that it is with Hobbes that we first see described the conditions for political participation—and thus for overcoming the utilitarian–technological calculus of interest—in the modern, Christian age.[21]

This moral depth of liberal doctrine is much clearer in Locke, whose intention in the *Second Treatise of Civil Government* was to present a more politicized version of Hobbes's account of government. For

Locke, like Hobbes, individuals create political power to escape the disorder caused by their natural equality. Government is established not to secure virtue or glory or some partisan conception of distributive justice but, rather, to guarantee the sphere of private liberty, or civil society. Again, government arises from the consent of each individual, but its form is determined by the will of the majority. But for Locke, unlike Hobbes, government is best able to ensure the broadest scope and tranquillity of civil society if political power is exercised through complicated and restrictive institutional arrangements.[22] As is well known, Locke departs from Hobbes in arguing for the prepolitical, natural status of private property, for the separation of legislative and executive power, for the placing of legislative power in the hands of an occasional assembly, for the subordination of executive to legislative power, and for the majority's freedom for revolution in the face of a legislative or executive threat to private property or civil peace.

However, there is no doubt that for Locke political power is absolute in the Hobbesian sense that there is no law (or power) to which the ultimate sovereign is subordinate. Though it originates in the consent of each individual, the power to make fundamental law resides in the will of the majority—without which "the mighty Leviathan" of commonwealth, to use Locke's Hobbesian words, could not exist for one moment (Locke 1965, pars. 97–99, 132–33). The purpose of this power—the protection of the "lives, liberties and estates" of those who consent to government—is, of course, that power's limiting condition and the limit of those who act as legislators or executives for the majority. But there is no natural juridical principle that specifies the proper form of legislative power, which is determined by what the community (i.e., the absolute majority) "think[s] good" (Locke 1965, par. 132; see also Hobbes 1981, chap. 19). Nor is there any natural or positive juridical principle that specifies when those who represent the majority by making or executing public law do or do not act for the end of political life. There are no such principles that govern the majority's judgment about these questions, aside from the bare fact of its sovereign interest. Hence, there are none that determine the majority's act of revolution against a legislative or executive power seen to have threatened the public good (Locke 1965, pars. 134–35, 142, 168). For Locke, the legitimacy of legislative and executive acts can ultimately be judged only after the fact, by whether they do or do not provoke revolution. Likewise, an act of revolution is judged legitimate only if it occurs, which proves that it is in fact the act of the original majority whose absolute power animates the representative state.[23]

Yet despite this Hobbesianism, Locke distinguished clearly between absolute and arbitrary power. Political power is arbitrary when it is exercised at pleasure and contrary to the common good (1965, par. 137). But the rule of law is not by itself proof against arbitrariness, because (1) law can itself be arbitrary and (2) so much that is essential to preserving the public good, including the very principle of the rule of law itself, is dependent on the exercise of executive power outside the bounds of law. For Locke, rational government depends not simply on the rule of law, the mechanical separation of power, and the majority's freedom for revolution but also on the reasonableness of the few representatives who make law for the majority, the wise exercise of executive prerogative, and the majority's sobriety and moderation. For Locke, private property is the institution best suited to ensure these virtues. It is not the absolute right of individuals to their legitimately held possessions in the realm of civil society (as it is for libertarians). It is, rather, the morally formative institution that both soothes the ambitious few who rule and girds the dignity and political liberty of the majority (1965, pars. 27, 31, 34–41, 123, 131). Locke's Hobbesianism leads him to conclude that rational politics depends as much on the premodern consideration of the character and ends of those who rule as on the modern and technological mechanisms for restraining the abstract and absolute authority of the majority.

It is true that Locke first describes the right to private property as grounded on the natural right to self-preservation and not on any convention or agreement. But it is important to remember that only the most original private property—the simplest appropriation from nature for the sake of nourishment—is determined wholly by individual acquisition and is absolute. Moreover, in the case of the appropriation of land, the natural law prohibition against waste is supplemented by the additional condition that there be enough land left over for others. Although this condition never applies in the first ages of the world (because of the simplicity of needs and means),[24] it establishes the principle that private property in land is not simply absolute and that its legitimacy depends on the welfare of those other than the proprietor. At the very least, in a condition of scarcity the first acquirer would have no proprietary right because all other men would have the right to take what they need by force. Unequal private property, however, is indeed the result of an agreement, namely, the tacit one required for the invention and use of money. The principle on which the agreement is made is the vast increase in general wealth produced by the incentive of unlimited private acquisition. Thus, unequal acquisition by individuals is justified by an appeal to the common good. Moreover, Locke makes it clear that with the advent of civil society, unequal private property in land is settled by positive law (1965, pars. 25–51).

Hence, when Locke says that securing private property is the end of government, he speaks of property broadly as a means for securing the *common* goods of life, liberty, civil peace, and prosperity (see Mansfield 1979). As a means, it is not an absolute end. Rather, it is measured, managed, and limited by those who govern with an eye to the majority's considered judgment about the common good of civil peace. (There is nothing in Locke's understanding of

property that would prevent a policy of redistributive taxation, so long as it is decided by representation and accepted by the sovereign majority.) For Locke, the governors will be most likely to keep their eyes on the common goods of life, liberty, and estate if they are propertied. And the people (the majority who always have the freedom for revolution) will be most reasonable if it too is "propertied" in a broad sense, that is, if the fundamental cleavage in civil society (between rich and poor) is moderated by the general prosperity resulting from unequal private property. And as regards the individual, private property is an aspect of moral personality that transcends its origin in mere self-preservation. As Locke argues, even though a consenting citizen can be obliged to surrender his life in battle, his inalienable right to self-preservation remains intact in his inviolable property (1965, par. 139). The "personality" appearing in one's property stands above mere life itself and can thus be the medium for overcoming the most immediate self-interest. Far from being the vehicle for licentious selfishness, for Locke, unlimited acquisition in the form of private property actually disciplines the soul.

There is still more to suggest that Locke's politics are far from technological; for he does not think that the liberal order can be constructed mechanically out of separated power, freedom for revolution, and the spontaneous moral and political efficacy of private property and unlimited acquisition. As I have argued elsewhere, Locke's distinction between the rational and industrious, on the one hand, and the quarrelsome and contentious, on the other, cannot be identified simply with the difference between the propertied rich and the propertyless poor (Weinberger 1988a, 1988b). Rather, Locke's account of human production makes it clear that the politically contentious passions and opinions (those having to do with power, honor, and vain ambition, all engendered by the call of the noble and beautiful) spring from the very desires and capacities of mind that give rise to the division of labor and expansive material production. The invention of money depends on the human capacity to confer value on useless or "fantastical and imaginary" things, as does the desire to produce beyond the simplest necessity; and both requirements of production lead to what Locke calls "evil concupiscence" and "vain ambition" (1965, pars. 46, 111, 184). In fact, the very artfulness of the "industrious and rational" is for Locke the ultimate source of political self-assertion. For Locke, the important political distinction is not just between rich and poor but also the Machiavellian one between peoples and princes. This is the distinction between those who humbly mind their own business and those who—moved by the attraction of the noble and the beautiful to contentious opinion, ambition, and the love of glory and honor—do not. The princely types may well stir up the poor, but they are themselves unlikely to be poor or to focus their ambitions simply on the claims or discontents of the poor. These princely types are the *busy heads* that Locke says can be found in any political order devoted to private property, material acquisition, and the freedoms of civil society (1965, par. 230).

They are not, however, an unmitigated evil according to Locke; for they provide the solution to the real problem of limiting government, which is how to engage the people's freedom for revolution. At first glance, the problem seems to be that revolution is so very dangerous. It is thus reasonable to be silent about the *right to revolution* (Locke never uses the term) and perhaps even to deny it altogether. But such caution is not necessary, says Locke, because the people, not being princely, are very slow to be moved. The real problem is not the people's volatility but their inertia, which inclines them to act only when their liberties have already been seized and it is too late to do anything about it. Something must goad the slow people to revolutionary vigilance. The only candidate for this role is the ambition of the well-heeled, busy heads who do not happen to rule (1965, pars. 220, 223–25, 230). But since there is no natural or positive law prescribing its precise conditions, revolution can never be a legal act—in which case the service rendered to the common good by the ambitious few is always in the form of conspiracy against legal—and, prior to the revolution, legitimate—authority. Most such conspiracies (which are always around in civil society) will fail, with the conspirators answering both to "God and Man" (1965, pars. 204–209, 240–42). But if one should succeed, it is only because the conditions were such as to incline the people to be moved on time. These are the conditions that (always after the fact) prove the legitimacy of any revolution. For Locke, the essence of the liberal state is the separation of politics and civil society. But this separation is effected only by engaging and managing the political forces arising from civil society and ultimately from the essence of human production. The secret of modern government is the subtle mixture of separated power, the moral and political institution of private property, and the revolutionary exploitation of conspiratorial ambition: the wise use and abuse of the princely on behalf of the political assertiveness and virtue of the people. Again, we could say that with Locke's account of property and revolution we see a fuller account of the conditions for political virtue and self-transcendence in the modern age.

POLITICS AND RELIGION

From Hobbes and Locke we learn that the modern political order is technological in the sense that it sees political practice as but a means to some transpolitical (hence ultimately private) end. Moreover, we learn from Locke that the liberal order is technological in the further sense that it manipulates those who do not fully know themselves, namely, the ambitious, who act for ends, ultimately private, that they do not intend. Yet despite this technological character of Hobbes and Locke's politics and their explicit opposition to classical political and moral doctrine, in both

can be seen a lesson hearkening back to Platonic political philosophy, namely, that politics is rooted in the inevitable existence of goods—the noble and beautiful things—whose scarcity and indeterminateness are coeval with human artfulness no matter how efficient or productive it might be. The technological impetus is the very source of the tensions of political life; thus, it is never possible for technology by itself, apart from some kind of moral and political virtue, to resolve them.[25]

If we find no complete technological stamp on their politics, we should at least be wary of concluding that these modern founders, who attacked the universals of ancient and scholastic metaphysics, dogmatically assumed a *technological* metaphysics of their own.[26] We might then consider that the contradictions often noted between their politics and their skeptical metaphysics were not unconscious and that their foundationalism (as expressed in the state of nature) did not reflect a dogmatic understanding of being. Indeed, it rather seems possible that like Plato and Aristotle, they understood how human thought transpires within the urgent political problems generated by common production. They knew, likewise, that thought is always tempted to the service of dogmatic partisanship and faith and that the activity of genuine questioning and doubt is fragile and rare. At least in part, it was this knowledge that led them to attack the dogmatic metaphysics and enthusiasms of the schools. It would be reasonable, I think, to read Hobbes and Locke's theoretical works in the light of an astounding and almost-always-ignored remark made by Hobbes: in the *Leviathan*, while summing up his argument against the universals of Aristotelian metaphysics, Hobbes says that Aristotle may have known his "entities and essences" to be "false philosophy" but "writ it as a thing consonant to, and corroborative of [the Greeks'] religion; and fearing the fate of Socrates" (Hobbes 1981, chap. 46). Here Hobbes tells us that Aristotle's "first philosophy" might have been ironic, moved by a self-conscious understanding of the relation between thought and politics—akin to Plato's noble lies and myths. If so, then Hobbes suggests that his own "first philosophy" was directed not at Aristotle's errors and toward an alternative, demonstrable first philosophy but, rather, at an earlier philosophic rhetoric occasioned by the clash between the gods (the guarantors of the just distribution of the noble and beautiful things) and the activity of thought. Given this remark and the subtlety of his and Locke's politics, we have at least to consider that far from having been in the grip of metaphysics and technology, the first liberals turned to the scientific conquest of nature with a clear understanding of that project's rootedness in the problematic character of productive life, with a clear understanding of its theoretical limits and practical dangers and with an explicit concern for what they took to be the peculiarly modern problem of politics: Christianity.

For the ancients, faith is inevitable so long as justice is the overarching political virtue, for only the gods can assure that injustice does not pay. And justice *is* the overarching political virtue because production generates partisan claims about a supposedly common good. But this meant that thought (in its purest form unproductive) is an activity that cannot be taken as just (or good) by any practical standard. Thus, if the life of philosophy is the ultimate measure of good politics (of a virtual, if not a genuine, common good), it could be so only as the distant model of a practical virtue that is itself not simply identical with justice.[27] For the ancients, only such practical virtue could restrain the dangers posed to common life by partisanship and its attendant faith in the gods. Given the fragility, instability, and dubiousness of such virtue, the ancients were pessimistic about the likelihood of decent politics. The founders of the modern liberal tradition sensed a new situation posed by Christianity: because of its universalism, Christian faith was even more dangerous to thought and common life than paganism; and because of its (egalitarian) doctrines of sin and conscience it was resistant to an Aristotelian political science based on the pagan moral virtues. Fully aware of the problem of Socrates and anticipating the extraordinary political power of modern science, they thought it best to replace Socratic irony and Aristotelian politics with dogmatic, scientific rationalism as the nexus between the life of thought and politics.[28] In this crucial respect, at its peaks the modern liberal tradition is conspiratorial rather than epistemological, dogmatic, and technological. The early moderns thought they could do much better than the ancients in managing partisanship and belief. But like the ancients they displaced, the liberal founders did not think it possible or desirable that technology wholly stamp practical life. Of course, they counted on technological progress and the acquisitiveness of civil society. But for this very reason they also counted on *liberal* technological politics, which required its own version of moral virtue and made considerable room for thought within the rigid certainties of the new technological age.

NATURE AND THE LIMITS OF TECHNOLOGY

I do not claim that modern rationalism and politics are simply free from metaphysical and technological dogmatism. Nor do I deny that the turn from ancient to modern thought was of enormous moral and political significance. I do mean to suggest that some architects of modern politics (especially the early liberals who thought most deeply about the Baconian project for mastering nature) understood that neither reason nor any system of technical means can ever *completely* stamp an age or political arrangement. And I do mean to suggest that they shared this grasp of reason and technology with their ancient rivals. The best of the early moderns knew that political life requires some form of practical virtue and that such

virtue is always in tension with the strictest demands of the life of reason. They knew that the dogmatism required for practical life conflicts with the fullest capacity for doubt and questioning and that this conflict would abide in and mark the technological age which political circumstances required them to promote. Thus, I mean to suggest that from these modern architects and their rivals one can distill an alternative to Heidegger's understanding of technology and then find an alternative version of Heidegger's view of how the ancients and the moderns are at once the same (as metaphysicians) and radically different (as regards the modern age of *das Gestell*).

Human experience is characterized by artfulness that is moved by attraction to goods (the noble and beautiful things) that are by nature scarce. Why are they by nature scarce? It is because as the independent and authoritative ends of art—as the objects of human longing—they are ultimately beyond our productive grasp. Moreover, the attempt to identify concrete things, here and now, as unambiguously beautiful and noble—as determinately good—never succeeds altogether. It is always possible to see a problem in every such attempt. Therefore, each such noble and beautiful thing points to what is good in itself and thus beyond any world here and now. And what these things are in themselves and beyond any world cannot be determined wholly by our knowing. But at the same time, the elusiveness of such ends and the urgency of material production combine to make human life impossible if art and convention do not claim to know and possess these ends. Their possession is impossible apart from production for luxury, which itself happens in part through the efforts of those for whom the noble and beautiful things consist in the activity of unlimited acquisition. And as a consequence of the essential scarcity of the natural goods, human life is always riven by the myriad tensions among the many and the few—between democratic and nondemocratic conceptions of the noble and beautiful.

These elements of experience produce (in addition to religious faith) two dogmatic conceptions of the natural whole, which, in each case, is taken to be so clear as to provide guidance about the just distribution of the noble and beautiful things. The first lets us know precisely what the rare noble and beautiful things are and which few should have them; the second lets us know that since the whole is a figment of human convention, there are no noble and beautiful things apart from those we freely create. In both cases, the noble and beautiful things are thought to be in perfectly adequate supply—either because there are just enough to fill a limited need or always enough to fill an unlimited demand. In the first case is the argument from strict teleology for the aristocrat; in the second case is the argument from constructivist relativism for the democratic–egalitarian proponent of the life of unlimited acquisition and exchange.[29] In mistaking the problematic character of the noble and beautiful things, both conceptions reflect the partisan's desire for justice and for overcoming the essential tensions of political life. Both are unphilosophic. But since philosophy is in tension with productive (i.e., technological) life, it is not possible for the two conceptions to become philosophic—to come to see their own limitations and problems. Consequently, an essential task of politics is the management of these two inevitable, irascible, and dogmatic tendencies of human reason. One or the other will always predominate in practical life. In the Christian age (we learn from the first moderns), it will have to be the second. And of course (as in every age), contingent circumstances require that thought practice the rhetoric most suitable to the times.

From this point of view I conclude, against Heidegger, that the problem of technology can be understood best in essentialistic terms—in terms of cause (or ground), genus, and species. Considered generically, the problem of technology identifies the tension between production and the self-sufficient ends that set production in motion and thus the political character of any order of human art; and the problem of technology emerges as the problem of nature, that is, how we are to live with the inevitable political tensions (finite in number and coeval with human life) caused by our technical openness to authoritative goods beyond our artful and cognitive activities (i.e., beyond any given conventional world). Considered specifically, the contemporary problem of technology springs from the decision to use the democratic–egalitarian claims of unlimited acquisition and theoretical constructivism to manage the natural tensions of common life. In this narrower respect, our contemporary problem of technology comes to light as the *exaggerated* conceit that common life can be stamped wholly by reason and by a system of productive means (a conceit used and abetted but never naively believed by the early liberals). The modern problem of technology is the inevitably partisan dream—now heightened by the project of scientific conquest—that the natural fissures of political life can be eliminated by an achievement of human thought and art.

At this point, the obvious objection is that to follow this line of thinking we would have to take the phenomenon of nature seriously; that is, we would have to take seriously the existence of a finite number of essential human problems and the existence of worldly and transworldly objects that call these problems into being. We would have to make a case for things like human nature, a normative highest good, and the Platonic *eide* and Aristotelian causes (perplexing as they are), lest this account of technology be nothing but a story that is at once dogmatic in ignoring Heidegger's powerful account of the worldhood of the world and dogmatically moralistic in simply assuming that there is some natural standard (the life of questioning) by which we can gauge our management of political life. Of course, I cannot refute this objection here. Suffice it to say that Heidegger's powerful attack on the epistemologically privileged position sought by some in the metaphys-

ical tradition (a position outside the assumptions of some contingent, lived world) does not by itself uproot the question of the being of nature. Rather, I would suggest that it liberates this question from the dogmatism and moralism of Enlightenment rationalism, which still lives secretly even in much post-Heideggerian thought.[30] Heidegger helps us reconsider the relations among thinking, knowing, being, art, nature, and politics, without dogmatically reducing them one to the other or presuming the character of being-in-the-world. In this respect, he helps us look for the grounds that are common to the ancients and the moderns, so that we may better understand how they differ. But if we can recognize a categorical and causal account of being that is not dogmatic from the beginning, how are we to choose between it and the Heideggerian alternative? Perhaps we may do so only by asking which one permits the fullest awareness of the character of practical life in all its comedy and gravity. To choose for our position, we would have to show two things: (1) that such awareness *requires* our granting the perplexed and dangerous openness of political life to governing entities whose existence lies beyond any worldly power to know or make—that without such a causal yet skeptical understanding of being, we cannot be fully open to the political phenomena that provoke our questioning wonder; (2) that such phenomena cannot be outstripped (always running ahead of every project to overcome them), so that we cannot escape the need to see them for what they are.

In other words, we would have to show that Heideggerian being, which grants a causeless and "factical" play of its domains, cannot account for the genuine gravity of political life—for how the elements of experience contend against each other, as we see in the challenge of thought to faith, the tension between private and public life, the conflicts between morality and politics, the difference between the good and the just, and so on. We would have to show that such contention is possible only insofar as their elements are related causally and hierarchically, so that each by its very nature claims an authority, beyond the contingencies of any given world, to order the others. And we would have to see that however much the fact of such contention calls forth our efforts to overcome it by way of making and knowing, both making and knowing are even at their best the very source of this contention. Nature, as I propose to think about it, is beyond any project for conquest. Technology could, of course, simply destroy the natural soul by making it either subhuman or godlike; but it could never wholly stamp the *human* species because it cannot supply all of the needs that the soul has spontaneously (or by nature), such as the desire for noble preeminence.[31] Consequently, the harder technology presses, the more intensely we sense a "problem" with it. I am suggesting that the problem of technology is most fully understood when we approach it through the old-fashioned question of natural justice that transcends any given political conventions. I am thus suggesting that no era's thinking and practice is so finite and self-contained that it can be wholly stamped by technology and that we do not have to recur to Heideggerian being to see the limits of the stamp. But I am also suggesting that such direction as nature gives to our groping for justice will never satisfy the demands of everyday politics and morality; for that direction consists in the limited extent to which the widest opening of our eyes can cure the blindnesses of political life.

All this is to say that we can choose against Heidegger only by showing that our view inclines us best to see unblinkingly the fissures within political life and to resist the dogmatic partisanship that is inseparable from *techne* and *poiesis*. Is it possible that for all its extraordinary power, the Heideggerian attempt to grasp the character of thought and art results in an equally extraordinary obtuseness toward the nature of political life? I would argue that both anticipatory resoluteness and *Gelassenheit* do just this: the former tempts us to a spurious unity of such things as work, thought, and war; and the latter tempts us to the spurious disconnectedness of these same phenomena.[32] Is it thus possible that Heideggerian being is itself a danger of technology, which always (like morality) tempts us vainly to try to jump out of our political skins? My argument is that if we own up to what it means to be human, the answer is a provisional *yes*. But if we need further proof that the homely problems of political life just will not go away, that there really are not that many of them, and that we deny this at our peril, perhaps we need only relearn how to look right under our noses.

There is nothing more technological than the idea that a metaphysical tradition—some rational system—could wholly stamp an age and its practical life. This is, however, just what Heidegger's account of technology would have us believe. And yet Heidegger's accounts of *Gestell* and *Bestand* do help us to think about what is so disturbing about modern technology—its tendency to deny the objects of the soul's desire, the noble and beautiful things that are in essence both rare and elusive. In thinking about technology, we discover that not the stamp of metaphysics but the political problems within which thought arises out of production is turned toward a mysterious but necessary whole and is always precarious. Technology is indeed a danger that saves: it compels us to think anew about the meaning of nature. But if, in grasping technology, we owe anything to Heidegger (and we certainly do) it is that Heidegger helps us to see how technology turns us away from Heidegger.[33] I suspect that such a turn will require us to admit that we cannot escape the question of the highest good and that while neither politics nor justice is that highest good, we cannot think it apart from the moral demands for justice that frame political life and any questioning about being and the soul.

Notes

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1. The *Gestell* is not a "concept" but, rather, the particular configuration of the domains of being that frames our age; but I risk distortion here for the sake of brevity. For the argument so far and for what follows, see also Heidegger's essays *Das Ding* ("The Thing") (1971, 165–86; 1967, vol. 2, 37–55), *Wissenschaft und Besinnung* ("Science and Reflection") (1977, 155–82; 1967, vol. 1, 37–62), and *Die Zeit des Weltbildes* ("The Age of the World Picture") (1977, 115–54; 1950, 69–104).

2. For an excellent summary of Heidegger's account of technology and nihilism, see Gillespie 1984, 124–64; see also, among many others, Kolb 1986, 144–200.

3. See Schürmann 1987, 17, where he explains the concept of stamps (*Prägungen*), and Arendt 1958 and 1963. For instance, without being a Heideggerian, Richard Rorty (1989) accepts completely Heidegger's account of the tradition. It would take a long time to account for the Heideggerian influences on the various contemporary schools of thought (both liberal and democratic) that see technology or metaphysics or both as a fundamental problem for modern politics and to relate these influences to the critical movement (i.e., Kant) within the tradition itself.

4. I do not here deny the distinction between political and prepolitical social life (i.e., between village and city). On the contrary, I mean to suggest that by contrast to isolated self-sufficiency all prepolitical social forms are defective as judged by their own principles. In other words, as regards political life Aristotle's teleology can be stated more even forcefully than Aristotle states it. Perhaps he demurred because he knew that what applies to the village can apply to the city, as well.

5. His political service came only when required; and we hear his courage described only in retreat (Plato, *Apology* 23b–c, 30c–33c, 36b–38c; *Symposium* 220d–21c). And, of course, his leisure made him dependent if not on those who work, then on the rich nobles with whom he associated.

6. But at the same time, such a life will never be altogether alien to them.

7. On the debatable status of moral virtue, see *Nicomachean Ethics* 1103a14–b25, 1105a17–b18, 1179c34–81d24; *Politics* 1281a11–84b34, 1323b21-24a35.

8. It is not true that technological conceptions of life and nature are altogether new. On the contrary, Aristotle teaches us that they are coeval with human life.

9. Plato's account of Atlantis in the *Timaeus* and *Critias* and Aristotle's discussion of the possibility of unlimited acquisition and Hippodamus should put this view to rest. Aristotle says that if tools were to move by themselves (on command or by anticipation), then master craftsmen would have no need for subordinates and masters would not need slaves (*Politics* 1253b34–54a1). Aristotle's meaning here is not what Marx took it to be (that the machine would minimize work; Marx 1977, vol. 1, pp. 531–32) because Aristotle speaks not just of machines but of miraculous, "thinking" machines that could be moved by speech or intention alone. But even granted such machines (which now seem possible), they would either still require much human work for their production, maintenance, and improvement or else (were they entirely self-moving) their capacities would raise serious questions about the justice of *their* enslavement. Moreover—and this is the important point—their productivity would not overcome the scarcity of the noble and beautiful things, the desire for which moves production in the first place.

10. This is why he wrote dialogues and why there is no Platonic doctrine presented apart from some consideration of useful myth or political necessity. Nowhere, in fact, does Plato present a demonstrable proof of the existence of the *eide* or of the existence of some transcendent reality that grounds the contingent phenomena of life and language. Plato's dialogues are first and foremost hermeneutical, not epistemological; and they always end in doubt, not wisdom.

11. The City of Sows' predecessor—the City of Utmost Necessity, where labor is first divided—is as imaginary as the age of Cronus, because that city's bare necessity is, in fact, luxurious by comparison to the alternative, rejected by Adeimantus, of isolated self-sufficiency. Thus, the City of Sows is, in fact, the adolescent form of its luxurious (and political) successor, a fact hinted at by the existence, even in the City of Utmost Necessity, of animal flesh (the by-product of the shoemaker's art), apparently introduced only after Glaucon's demand for luxury.

12. Recall Aristotle's perplexing claim that while there is such a thing as the just by nature, it is always changeable (*Nicomachean Ethics* 1134b18–35a15).

13. It is especially intolerable, since such ignorance regards the nature of the gods, who guarantee the justice of living for the productive common good, by whichever partisan conception it is defined (Plato *Apology* 23c2–30c1).

14. This contradicts, I should say, Richard Rorty's claim that Socrates was a metaphysician in the sense of having *assumed* that a "term in his final vocabulary" (i.e., in his account of ends) refers to something that has a real essence (1989, 74). See n. 3.

15. The former is the view of "traditional morality," as exemplified by Socrates' tormentor Anytus; the latter is the view of the sophists and rhetoricians, to whom the powerful of Athens sent their sons for instruction (see Plato's dialogues *Meno* 70a1–71e1, 80d5–81a21, 89d–93a4, 95a2–96e5; *Apology* 18a7–21b, 23c2–24c9; *Sophist* 234a7–36d3; 240d1–41c6, 260d5–61b4; *Republic* 492a1–d1, 595a1–99a4; *Gorgias* 465b6–c7, 482c3–86d1, 494a6–97d8; cf. Aristotle *On Sophistical Refutation*, 183a36–84b9; and *Rhetoric* 1355a3–b34).

16. See, e.g., Heidegger [1927] 1972, 126–30, 175–80; 1962, 163–68, 219–24; 1933; 1976. Falling, however, is not to be understood as an ontic moral condition "caused" by the likes of Hobbes. As Heidegger insists, it is a "definite existential characteristic of Dasein itself" (1962, 220).

17. The liberty of the natural condition is the same in character as the liberty of the subject; it is just that the subject's liberty is narrowed by civil law for the sake of maximizing the scope of private (natural) liberty (see Hobbes 1981, chap. 21).

18. See Hobbes 1981, chaps. 21, 42–43; the sovereign can commit an iniquity—but not an injustice—against a citizen; and "profession of the tongue is but an external thing" (see Orwin 1975).

19. It is only the sovereign for whom natural and artificial personality are inseparable and coeval. Thus, if there is a God, the sovereign takes the biggest risk that his conscience might be wrong. Indeed, he takes the sins of the citizen upon himself!

20. It especially engages the energies of the "enlightened." Hobbes clearly presumed the political ambition of the enlightened few as essential to a rational political order. Given the teaching of the state of nature as he presents it (showing political opinions about the noble and good to be in every case spurious), one suspects that Hobbes thought the enlightened to exhibit their own special lack of self-knowledge. Like Socrates, Hobbes never ran for office or aspired to rule.

21. This is not to say that the likes of Hobbes and Locke agree with the ancients about the character and status of the moral virtues and the noble and beautiful things. They do not; and to think otherwise would lead us astray. But I do mean to suggest that the ancients' "belief" in the existence and intelligibility of such phenomena and the moderns "rejection" of them are much more complicated, subtle, and qualified than is assumed by the critics of technology.

22. Hobbes, of course, preferred absolute monarchy. However, it is often forgotten that for Hobbes all forms of

government (one, few, or many) are equally representative and equally absolute (1981, chap. 19).

23. Like Hobbes, then, Locke's understanding of representative government leaves no room for individual or collective appeals to conscience—or to higher law and natural distributive justice—in evaluating the form or particular policies of an established political authority. Representative political power is limited (kept to its task of preserving the sphere of civil society) by the freedom for revolution, rooted ultimately in the absolute will of the majority. The majority's will determines the individual wills of those who originally authorize it, and that same majority's absolute judgment confronts and limits the particular wills of legislators and executives. But again, there are no juridical principles that govern the exercise of this judgment.

24. I doubt, however, that Locke really thinks it possible that there ever was such a first age.

25. As Hobbes says (in subtle agreement and disagreement with Aristotle), men differ from bees and ants in competing for honor and dignity; in relishing "nothing but what is eminent," which generates the tension between public and private good; in thinking themselves wiser in public business than others; in misrepresenting good and evil; and in creating government (1981, chap. 17).

26. This is the clear implication of Heidegger's account of Hobbes's nominalism (Heidegger 1982, 183–92, 200–208).

27. According to Aristotle, genuine courage requires that the courageous man *not* believe in life (and thus punishment or reward) after death, piety is not one of the virtues, and justice competes with magnanimity for the status of highest virtue. There is, then, a tension between virtue, on the one hand, and faith and justice, on the other, as well as between faith and thought. This is not to say, however, that the virtuous are easy friends of thought or philosophy; for virtue is based not on thought but on habit, and neither Aristotle nor Plato thought the good aristocrats to be any less dogmatic than the faithful and just many (see *Nicomachean Ethics* 1100a10–1101b10, 1103a15–26, 1115a25–33).

28. A further such nexus between thought and politics was the typically modern doctrine that justice is the highest virtue if not, indeed, the only principle of morality.

29. This explains how the aristocratic but constructivist Nietzsche could be assimilated so easily to contemporary egalitarian politics; that is, constructivism is always secretly democratic and egalitarian, so that it is no surprise that Nietzsche finally described himself as a saintly aristocrat, that is, one who cares about, and bears responsibility for, the whole of humanity.

30. Imagine the likes of Socrates or Hobbes or Locke confronting Heidegger or a contemporary advocate of "postmodernism." Would not the former immediately wonder of the latter what kind of partisan, moralistic ax was being ground in the claim that political life would be transformed when "metaphysics" or "foundationalism" were left behind? Would they not wonder whether the real issue at stake was not a naive hopefulness (often hidden beneath realistic claims about the character of power) that political life would give itself over to unity once we gave up the hopeless quest for a demonstrable, unifying ground? And would they not say that this hopefulness would founder on, if it did not exacerbate, the timeless conflicts between the many and the few, the rich and the poor, the noble and the useful, the good and the just?

31. Of course this desire can take technological form (e.g., producing with the least effort the very best robot), but no technology can make preeminence available to all.

32. For uncovering the questions in this and the previous paragraph, I am indebted to Mark Blitz's original and excellent discussions (1981, 169–80, 210–22).

33. Of course, that Heidegger might be wrong does not mean that the tradition as I have interpreted it here must be right. Most contemporary theorists of democracy and liberalism are not Heideggerian, nor are they inclined to think that they depend on taking seriously the questions of nature, the whole, and the highest good. To make my suggestions stick would require showing that we need them to make sense out of contemporary liberal and democratic politics—especially given the recent spate of "antifoundationalist" and (nonmetaphysical) neo-Hegelian defenses of liberalism (e.g., Pinkard 1987, 1988). I think we do need to follow up my suggestions in order both to defend and to see the now-often-denied problems of liberal politics. But these are themselves enormous questions.

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THE PRICE ELASTICITY OF MASS PREFERENCES

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I compare the price elasticity of economic and political preferences. My central hypothesis is that willingness to pay, whether expressed verbally or through cash transactions, is more price-elastic for economic consumption goods than for public goods. I find that increases in price greatly diminish the proportion of people willing to pay for consumer goods, such as housing or hardback books; whereas the proportion willing to pay more in taxes to support a public good, such as environmental protection or shelter for the homeless, is much less responsive to changes in price. I conclude by discussing the theoretical and political implications of willingness to pay for public and private goods.

> Voters and customers are essentially the same people. Mr. Smith buys and votes; he is the same man in the supermarket and in the voting booth.
>
> —Tullock, *The Vote Motive*

One of the most provocative findings in public opinion research is that tangible personal interests are seldom correlated with policy opinions. The opinions of the employed and unemployed differ only slightly on issues such as whether government should guarantee employment or expand public works programs (Sears et al. 1980; Schlozman and Verba 1979). Much the same may be said of the differences between parents and nonparents on the issue of busing (McConahay 1982; Sears, Hensler, and Speer 1979), men and women on women's rights or the Equal Rights Amendment (Mansbridge 1985; Shapiro and Mahajan 1986), and the medically indigent and those with health coverage on the question of national health insurance (Green 1988; Sears et al. 1980). True, homeowners are at times significantly more receptive to property tax reductions than renters (Sears and Citrin 1985), and smokers more hostile to cigarette taxes and smoking restrictions than nonsmokers (Green and Gerken 1989). But such discrepant cases are overwhelmed by evidence showing the limited influence of narrow self-interest on opinions toward policies such as bilingual education (Sears and Huddy 1987), affirmative action (Kinder and Sanders 1990), energy conservation (Sears et al. 1978), escalation of the Vietnam War (Lau, Brown, and Sears 1978), income redistribution (Kluegel and Smith 1982; Verba and Schlozman 1977), and social security (Beck and Dye 1982; Lau and Sears 1981). In sum, although policies are invariably more costly to some individuals than to others, personal costs and benefits are poor predictors of how people wish government to act.

Such findings run counter to the widely held assumption that political preferences are rooted in selfish and materialistic motives, an assumption that turns up in the writings of Aristotle, Locke, Madison, Marx, Schumpeter, and others (see Green 1988; Mansbridge 1990). But what makes these results especially intriguing is that self-interest seems to fare quite well in other disciplines, notably economics. The empirical literature on the behavior of consumers and firms is replete with illustrations of how ordinary people respond to prices in an effort to buy cheap and sell dear (Bolton 1989; Monroe 1979; Tellis 1988). Even scholars who emphasize the moral or affective dimensions of economic behavior (Collard 1978; Etzioni 1986, 1988) acknowledge that as a rule, a rise in price reduces quantity demanded.

However plausible it may be to suppose that direct personal costs have more force in economics than politics, one must be cautious about comparing the empirical work in the two disciplines. There are important methodological differences between economists' and political scientists' ways of assessing the influence of tangible personal costs. Economists tend to examine the extent to which price changes affect aggregate sales, whereas political scientists tend to study how individuals react to changing incentives. Moreover, economists typically study situations in which prices are stated explicitly to consumers, whereas in public opinion research voters are left to figure out for themselves what a policy will mean for their pocketbooks. Thus, in the absence of a direct empirical test, one cannot rule out the possibility that the apparent contrast between economic and political decisionmaking is to some degree artifactual.

I shall attempt a systematic comparison of the price elasticities of economic and political preferences. I begin with a discussion of the definition and measurement of the key terms in this study: *willingness to pay*, *asking price*, and *price elasticity*. Using a combination of survey data collected between 1940 and 1990, experimental evidence about consumer behavior, and records of employee contributions to the United Way, I assess the hypothesis that willingness to pay (whether expressed verbally or through cash transactions) is more price-elastic for economic consumption goods than for public goods. Small increases in price are found to reduce sharply the proportion of people willing to pay for consumer goods. By comparison, the proportion willing to pay more in taxes to shelter the homeless or protect the environment is less responsive to price changes. I consider a number of potential explanations for the contrast between public

and private goods, then conclude by discussing some of the political implications of the electorate's inelastic demand for public goods.

CONCEPTUALIZATION AND MEASUREMENT

Willingness to Pay

Research on the public's willingness to pay for various goods and services spans several disciplines. Market researchers ask people how much they would be willing to pay for new products. Pollsters interested in the popularity of certain policy initiatives test the depth of public support by asking citizens whether they would be willing to pay more in taxes in return for certain government services. Students of consumer behavior measure the price ranges that shoppers find acceptable for various products. Economists, too, have employed survey research methods in order to assess the public's contingent valuation of various public, or nonmarket, goods.

Notwithstanding the frequency with which it is used, *willingness to pay* remains a somewhat ambiguous construct.[1] The fact that someone verbally expresses a willingness to pay a given price for a certain good implies neither that a purchase at that price is imminent nor that the individual intends to make the purchase in the future. In this sense, verbal expressions of willingness to pay are distinct from behavior, as well as from behavioral intention. Nevertheless, willingness to pay is thought to have behavioral implications. In the economic realm, one would expect those who say they would pay $50 for a hand calculator to be more likely to make such a purchase than those who express unwillingness; in the political realm, one would expect those expressedly willing to pay $50 more in taxes for local schools to be more likely to vote in favor of such a ballot measure than the expressedly unwilling.

Just how well behavior may be predicted by willingness to pay is a matter of considerable disagreement. Some scholars regard utterances of this sort with suspicion, arguing that people have strategic incentives for concealing or misrepresenting their true behavioral predispositions (Samuelson 1954). Economists, in particular, opine that in the absence of real budget constraints, talk is cheap, and purported willingness invariably outstrips effective demand. Another common criticism is that people have strategic incentives to understate their willingness to pay "if they believe they will actually have to pay the amount they reveal, and believe also that there is a good chance the good will be provided even if they understate their true [willingness-to-pay] amount" (Mitchell and Carson 1989, 128). In spite of these measurement problems, there seems to be a persistent correlation between these kinds of verbal expressions and actual purchasing behavior (Banks 1950; Gabor 1985; Green and Tull 1978; Udell 1965). Moreover, experimental evidence suggests that people tend to report essentially the same willingness to pay regardless of whether they are required to back up their responses with cash payment (Bohm 1972; Dickie, Fisher, and Gerking 1987).

In order to satisfy both sides of this dispute, I examine actual spending behavior, as well as verbal expressions of willingness to pay. Interestingly enough, the distinction between economic and political decisions is about as clear for cash transactions as it is for verbally expressed willingness to pay. This is not to deny the potential differences between payment and willingness to pay. The point is, rather, that analyses of both dependent variables support the theoretical argument.

Price Elasticity

By *price elasticity*, I mean the rate at which willingness to pay declines as price increases. Placing asking price on the x-axis and the proportion of people willing to pay on the y-axis (Figure 1), one may draw the distinction between *inelastic demand* (the near-horizontal line) and *elastic demand* (the near-vertical line). In the former case, large increases in price produce little change in willingness to pay; in the latter, small price increases produce large changes.[2]

I have chosen to draw curves in Figure 1 rather than straight lines for two reasons. First, I wanted the diagram to reflect the fact that willingness to pay a given price is a dichotomous dependent variable. Since probabilities range between zero and one, it is conventional to conceive of the dependent variable in logistic terms, wherein each unit along the y-axis represents a one-unit change in the log-odds of willingness to pay (Maddala 1988).[3]

Second, intuition suggests that the independent variable (price measured in dollars) will not bear a linear relationship to the dependent variable. To measure price in dollars implies that a $1 change in price has the same impact on willingness to pay regardless of whether this change is from $1 to $2 or from $101 to $102. Previous work on price perceptions and historical patterns of demand shows this assumption to be implausible (Cameron and Huppert 1989; Nicholson 1984, 179–80). This research, as well as the data I shall examine, indicates that the fit between actual and predicted willingness to pay is vastly improved when the natural log of price is used in place of the absolute dollar value. My analyses therefore assume that the log-odds of willingness to pay is a linear function of the log of price. Had I redrawn Figure 1 with logits on the y-axis and the log of price on the x-axis, the two curves would have been straight lines.

How is an analysis of this sort constructed? By way of illustration, suppose n people are asked

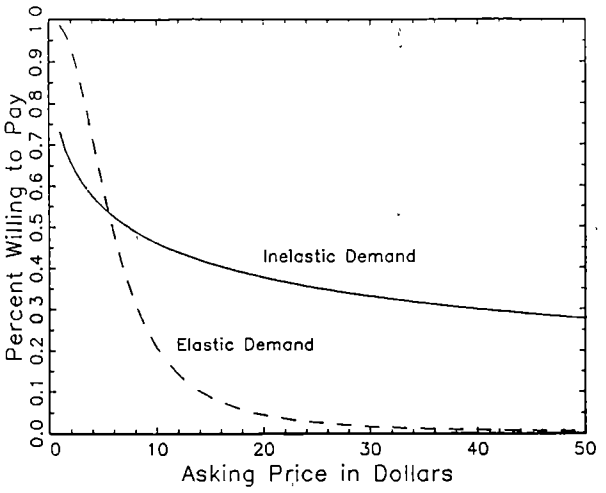
1. Would you be willing to pay $10 for a bundle of good X?

Those who respond *no* are then asked

2. Would you be willing to pay $5 for a bundle of good X?[4]

Figure 1.

Elastic Versus Inelastic Willingness To Pay



The frequency distribution of the responses, where *A*, *B*, and *C* are the numbers of people responding in different ways, is

yes to question 1 (willing to pay \$10)	A
yes to question 2 (willing to pay \$5 but not \$10)	B
no to question 2 (unwilling to pay \$5)	C
Total	*n*

A cross tabulation of price with willingness to pay would look like this:

Willingness to pay	\$5	\$10
Willing	*A+B*	*A*
Unwilling	*C*	*B+C*

Thus, the log-odds ratio is

$$\ln\left[\frac{A \cdot C}{(A+B)\cdot(B+C)}\right]. \qquad (1)$$

Equation 1 has three implications. First, the slope is undefined if either *A* or *C* equals zero. In other words, if the asking prices are too high or too low, it will be impossible to determine the elasticity.[5] Second, if *B* is zero, the slope estimate will be zero. This, too, makes intuitive sense; for if no one settles for an intermediate price, price lacks causal power.[6] As a practical matter, it is important for the survey researcher to select a range of prices that will produce at least some degree of variability in willingness to pay. Finally, it is apparent that for a given *A* and *C*, the slope estimate decreases (becomes more elastic) as *B* increases. Thus, the more people who settle at an intermediate price (ceteris paribus), the greater the price elasticity.

In order to calculate the price elasticity for good X, one divides the log-odds ratio by the change in the natural log of price, in this case .69.[7] Provided the log-odds ratio is not undefined, this elasticity represents the change in the log-odds of willingness to pay that results from a one-unit change in the log of the asking price. Or, to state the result more cautiously, the estimate reflects the best guess of the price elasticity within the range of prices bounded by *A* and *C*.

Three other methodological caveats are in order. For this analysis to be meaningful, respondents cannot be exempt from the asking price. The fact that between 80% and 87% of nonsmokers favor raising cigarette taxes 5, 10, or 20 cents says little about willingness to *pay* (Green and Gerken 1989). More relevant would be the views of smokers, who are subject to the tax. Second, it is important that the distribution of willingness to pay not be truncated or censored by the use of "filter questions" that are themselves measures of willingness to pay. For example, a 1976 General Electric Quarterly Survey asked respondents whether they would be likely to purchase an electric car and if so, how much they would be willing to pay. This procedure leaves unmeasured the willingness to pay of those not likely to buy. It would be hasty to impute to them a willingness to pay of zero, since they may have had in mind an anticipated purchase price when answering the filter question. To discard these observations would almost certainly lead to biased estimates (Maddala 1988). Therefore, I have excluded a handful of survey items that were preceded by filter questions of this kind (see end of Appendix B).

Finally, in order to facilitate a comparison of demand for public and private goods, it is best to minimize extraneous substitution effects. Appliance stores, for example, feature many comparable brands of consumer durables, so that if one brand goes up in price, people will switch to others (Abrams 1964; Jones 1975). In this situation, price elasticity for a particular brand is greater than the elasticity for the product class (e.g., refrigerators) as a whole. Thus, when gathering data on consumer preferences for private goods, I have ignored the literature on market share and restricted my attention to cases in which people are asked whether they would pay for a type of product.

DATA AND MEASURES

The data analyzed in this paper are drawn from three sources: surveys, experiments, and United Way fund-raising records.[8] In each case, people are asked whether they would be willing to pay for a particular good, be it hardcover books, shelter for the homeless, or commercial-free radio.[9] The survey data were obtained from on-line searches of the Harris, Roper, and Inter-University Consortium for Political and Social Research question text data bases and from manual searches of local, state, and national polls archived at the Roper Center. Experimental data were gleaned from secondary sources. United Way records

were made available to me by the payroll departments of three large firms in the greater New Haven area.

Over the past 50 years, pollsters and market researchers have devised several ways to offer people different prices. One is to divide the sample into random subgroups and pose a different price to each subgroup. Another approach is to present all respondents with a single price and then ask a follow-up question at a higher or lower price, as in the good X example. Another is to present respondents with a range of different prices simultaneously and ask which is the highest price they are willing to pay. Finally, one may simply ask respondents how much they are willing to pay, without offering any response categories.

Although equivalent in principle, the psychological effects of each question format are likely to differ. When two randomly assigned groups confront different prices, the effects of price tend to be weaker than when the same people face a series of prices (Ehrenberg and Mills 1990; Kosenko 1989; Monroe 1977; Petroshius and Monroe 1987). In the former case, subjects receive no initial price with which to contrast subsequent prices; and the difference between $20 and $25 may fail to produce variation in willingness to pay. The effect of asking respondents to name their prices probably falls in between these two formats. As long as there is variation in the prices named, willingness to pay will fall with price; there is no such guarantee that this will happen when groups are randomly assigned a single price. On the other hand, in the absence of structured response options, responses may become less reliable, and therefore less price-elastic.

One also needs to guard against framing effects that may distort the results. When respondents are asked a long battery of questions concerning their willingness to pay for various goods, they gradually become more thrifty. Thus, willingness to pay becomes confounded with the extent of one's prior expressions of willingness. Similarly, how people react to prices may be conditioned by the prices they received earlier for previous goods. For these reasons, two cases have been dropped from the analysis.[10]

In sum, various methodological considerations led us to restrict the analysis to just 45 items, covering 38 different kinds of goods. These survey questions are described in Appendix B. Despite my selectiveness, this data set remains methodologically heterogeneous. For this reason, I shall now evaluate the robustness of elasticity estimates across time, question format, and question sequence.

DATA ANALYSIS

The Robustness of the Elasticity Estimates

Before taking up the question of whether political goods are less price-elastic than economic goods, one must first ask whether it is meaningful to speak of enduring "parameters" that represent the price elasticity of willingness to pay for a given public or private good. After all, if the estimated price elasticity for a particular good varies substantially from one setting to the next, the utility of studying patterns in the public's willingness to pay is called into question. On the other hand, if parameter estimates display stability over time, then a minimum condition for proceeding with the analysis has been met.

Three sets of questions are available for across-time comparison.[11] Californians in 1988 and 1989 were exposed to virtually identical questions about paying more in taxes to provide shelter for the homeless. Willingness to pay proved to be significantly greater in 1988 than in 1989 ($p < .05$); yet the elasticities are quite similar across the two samples (−.27 vs. −.35), and the difference between them is not significantly different from zero ($p > .05$). Michigan respondents were asked in 1972 and 1974 about their willingness to pay more in state and local taxes for law enforcement. The only variation in question wording was that 1972 respondents were informed that average per capita expenditures in this area were $36, whereas in 1974 this amount was changed to $45 (see Appendix B). Although respondents were significantly more willing to pay higher taxes in 1974 than in 1972 ($p < .05$), the elasticities (−.65 and −.54) are not statistically distinguishable from one another ($p > .05$). Finally, four national surveys between 1973 and 1987 asked Americans whether they would be willing to pay more in taxes in order to fund artistic and cultural activities. Question wording and order varied somewhat; and again, the mean level of public support changed significantly from one survey to the next. Yet the four elasticities are almost identical (−.53, −.46, −.52, −.54). Pooling the data and comparing constrained and unconstrained likelihoods shows that the small differences between elasticities may be attributed to sampling variability ($p > .05$).

Elasticity estimates also seem to be quite robust across variations in question wording at any given point in time. Kahneman (1986) conducted an experiment in which Ontario respondents were asked about their willingness to pay higher taxes to fight water pollution and preserve fishing in the region. The definition of the region, however, varied from one experimental group to the next. Some respondents were asked about their willingness to pay to preserve fishing in Muskoka; others, Haliburton; others, Ontario as a whole. The three elasticities are quite similar (−1.39, −1.50, −1.47; $p > .05$), despite the fact that the cleanup of all of Ontario was 5–15% more popular than the other cleanup proposals.

These results suggest that the price elasticities associated with particular goods have an enduring quality; for even as goods grow or diminish in attractiveness, their elasticities remain stable over time and amid variations in question wording. Adding to the strength of these results is a recent study of the British public's willingness to pay for British Broadcasting Corporation channel 1 television on a sub-

scription basis, indicating that elasticity estimates also tend to be relatively insensitive to the form in which prices are stated. Respondents to different surveys conducted during 1989 and 1990 were offered prices ranging from £44 to £240 per year on an annual or monthly basis, depending on the survey (Ehrenberg and Mills 1990, 13). These data yield elasticities of −1.70 and −1.76 ($p > .05$) for annual and monthly question formats, respectively. In essence, the shape of the willingness-to-pay distribution remains unchanged across equivalent price ranges expressed in different units, despite the fact that the mean level of willingness to pay is significantly higher ($p < .05$) when prices are asked in terms of monthly rates.

Finally, one may gauge the robustness of price elasticity estimates across variations in question sequence. The results concerning cultural activities give a preliminary indication that variations in question order do not alter the results dramatically. A more rigorous test is made possible by a 1963 National Opinion Research Center split-ballot experiment in which people were asked whether they would be willing to pay for a household device designed "to alert the population in case the United States were to be attacked by an enemy." Respondents were randomly assigned to one of four initial asking prices ranging from $5 to $20. After responding to the first price, subjects were asked to respond to the three other prices; thus, some respondents received the prices in ascending order, others in descending order. As one might expect, random assignment of price generates an elasticity estimate that is somewhat weaker than that produced by an ordered series of price offers (−.76 vs. −1.00; $p < .05$).[12] But (as I shall demonstrate) this range of variation is dwarfed by the variation between the elasticities of consumer goods and public goods. In other words, question format may alter the estimates somewhat, but not enough to affect the overall pattern of results.

The Consistency of Price Elasticities across Subgroups

My procedure for evaluating price elasticity presupposes that the influence of price is constant across all the individuals within a sample. Ignored, therefore, are variations in cost sensitivity that might arise because some people are strapped for cash or committed to a political ideology that stresses fiscal restraint. Before turning to the comparison of public and private goods, I take note of the fact that elasticity estimates seem not to vary markedly from one subgroup to another.

Consider, for example, willingness to pay to support the arts and cultural activities in 1973. As one would expect, the frequency with which people attend artistic performances is a strong predictor of willingness to pay: 62% of those Farrell characterizes as "frequent cultural attenders" were willing to pay $50 more in taxes, as opposed to 17% of "cultural nonattenders" (1975, pp. 6, 112). But despite the sharp difference in *mean* willingness to pay, the elasticities for the two groups were −.54 and −.48, respectively.[13] Very similar results obtain when one stratifies the sample by income, age, education, region, or race. For example, although willingness to pay increases sharply with income, elasticities for those making less than $5 thousand, $5–10 thousand, $10–15 thousand, and over $15 thousand turn out to be quite close (−.49, −.48, −.51, and −.57, respectively) and not significantly different from one another ($p > .05$).

These results are corroborated by other investigations of this sort. In an earlier work, I analyzed willingness to pay for (1) shelters for California's homeless, (2) an expanded subway system in Los Angeles, and (3) reductions in water pollution. In each case I found that elasticity estimates varied little across categories of income, ideological self-designation, and region (Green 1988). Similarly, Ehrenberg and Mills (1990) found that the elasticity of willingness to pay for subscription television varied little among groupings of social class and amount of television viewing; results presented by Cameron and Huppert (1989) indicate that the dispersion of anglers' willingness to pay to support fish hatcheries in the San Francisco Bay did not change when controls were added for income, number of fishing trips in the past year, and boat ownership; and Adam's (1958) study of willingness to pay for clothing and durable goods turned up little variation in elasticity across income and occupational strata. In short, while groups vary with respect to their mean level of willingness to pay, the rate at which willingness to pay changes with price is remarkably consistent across groups. In light of these findings, I feel confident that the strategy of treating samples in an undifferentiated way does not distort my conclusions.

THE CONTRAST BETWEEN POLITICAL AND ECONOMIC GOODS

Are public/political goods more price-elastic than private/economic goods? Let us begin by developing standards by which to distinguish between public and private goods. Four criteria suggest themselves:

1. *Excludability versus equal entitlement.* Who is entitled to use the good? In the field of economics, public goods are defined as those which, once produced, may be consumed by anyone. National defense and wildlife preserves, for example, represent public goods because no citizen may be prevented from consuming them (Nicholson 1984).[14]
2. *Individual versus collective payment.* Is payment to be an individual or collective act? One may distinguish between purchases that are made in concert with others (e.g., through taxes or contributions) and transactions in which the purchase results from an individual's action (e.g., an exchange transaction). The former brings to mind an additional psychologi-

TABLE 1

Classification of Goods along the Public–Private Continuum

Private. Exclusive and personal consumption and individual payment; not associated with the public welfare: toilet soap, subscription television (BBC channel 1), lottery tickets, pay-per-view baseball television, newly introduced communications product, refrigerators, nylon stockings, hardcover books, newspapers, housing, and health insurance.

Semiprivate. Exclusive and personal consumption and individual payment; associated with the public welfare. Exclusive and personal consumption but collective payment; not associated with the public welfare. Or personal consumption and individual payment but semiexcludable. Examples are safety caps on prescription bottles, Swedish television comedy show, and recreational fishing.

Mixed. Exclusive and personal consumption of the product to be taxed but nonexclusive personal consumption of the good funded by the tax; collective payment imposed only on those who consume the product; associated with the public welfare:

cigarette tax designed to reduce a budget deficit
gasoline tax designed to build roads
gasoline tax to reduce dependence on foreign oil
sales tax to fund World War II war effort
civil defense alarm
increase in monthly electricity bills for pollution abatement
increased taxes and consumer prices for clean drinking water
increased gasoline prices to sustain Iraq oil embargo
anglers' support for fish hatcheries and habitat restoration

Semipublic. Nonexclusive consumption and collective payment; associated with the public welfare. However, the good may be consumed personally:

preserving fishing in Ontario lakes
fighting pollution in the state's fresh water lakes
artistic and cultural activities
building a subway to combat pollution and traffic congestion
commercial-free radio
planting trees in a nearby park
fighting crime in one's state
cleaning the environment in one's state
increased family expenses to improve the environment

Public. Nonexclusive consumption and collective payment; associated with the public welfare; unlikely to be consumed personally: shelter for the homeless, reducing the federal deficit, cleaning up the environment, clean air, clean water, and saving endangered species.

cal consideration: To what extent should the collectivity take action on behalf of this objective?

3. *Personal versus social consumption.* Is the good in question of personal use to the person being asked to pay? Or does it primarily meet social needs that are more or less independent of the personal needs of the purchaser? When one pays to supply coffee in one's office or clean up air pollution in one's locality, the benefits of doing so are likely to be felt personally. At the opposite end of the continuum are donations designed to combat starvation in a distant country, a program that is likely to have no tangible impact on the donor.[15]

4. *Public welfare goods versus non–public welfare goods.* What ends are served by the good in question? Even goods that are consumed personally, such as safety caps on prescription bottles, may serve a broader social purpose.

Using these criteria, I classified the 38 types of goods along a five-point scale (See Table 1). On one end of the continuum are goods that may be classified as private by all four criteria; on the other, those which are wholly public.[16]

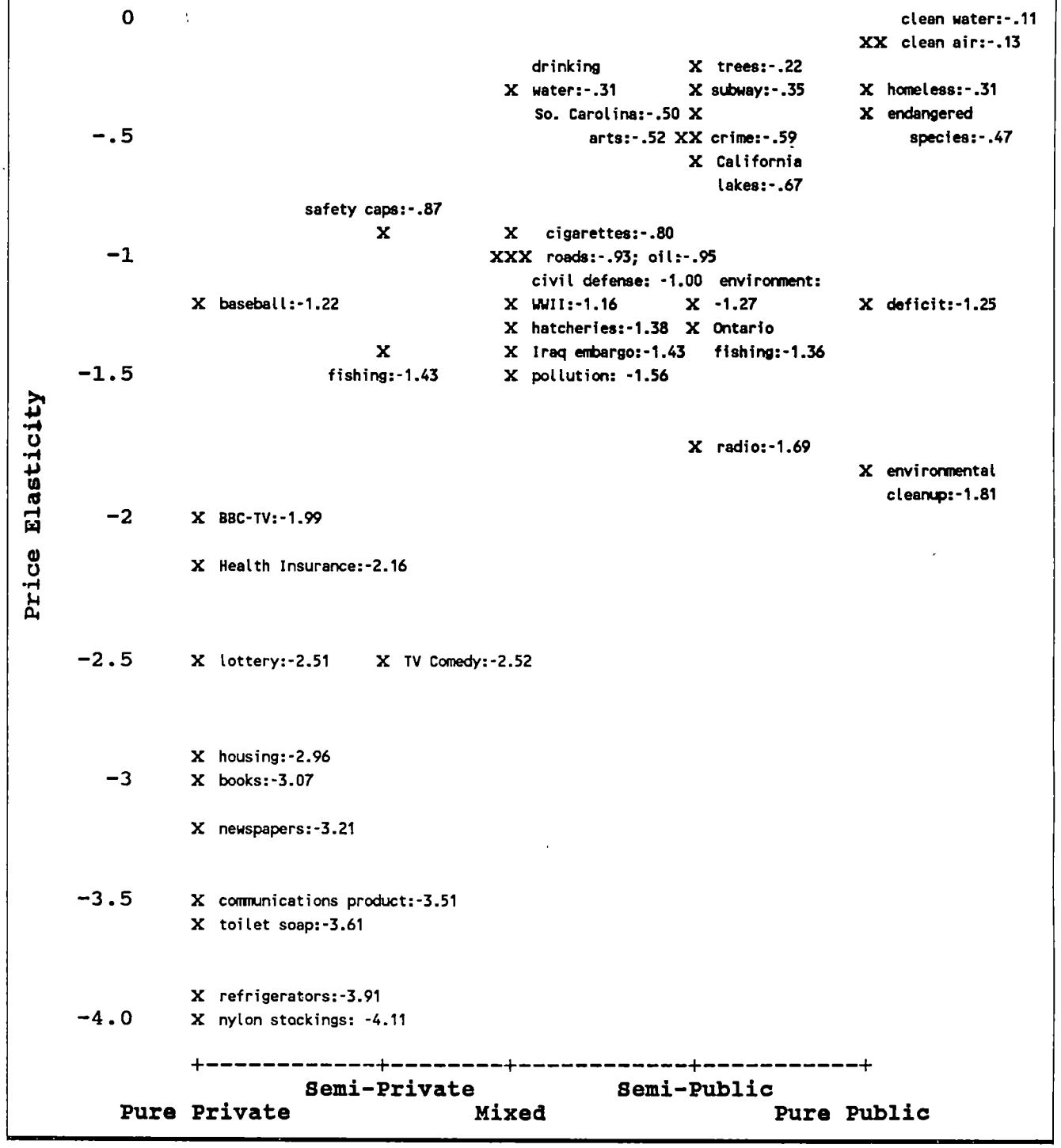
Is there a relationship between the public–private continuum and price elasticity? Price elasticities were calculated for each good. Where duplicate questions existed, the elasticity estimates were derived from analyses in which the data sets were pooled. Figure 2 presents the elasticity estimates in the form of a scatterplot. The strong upward tendency of the points (Spearman's $\rho = .74$) indicates that the elasticity of private/economic goods is considerably greater than the elasticity of public/political goods.[17]

SOME STATISTICAL REFINEMENTS

Although it is gratifying to see the contrast between public and private goods emerge so clearly, the analysis requires some refinement, since the elasticities for certain public goods are so weak as to be anomalous. Take, for example, a good such as shelter for the homeless, which has an elasticity of −.31 and, in 1988, an intercept of 2.3. The hypothesized linear relationship between the log-odds of willingness to pay and the log of price fits the data quite well within the $5–$50 range of prices presented in the survey ($\chi^2_{(2)} = 1.95$, $p = .38$); yet if one were to extrapolate

Figure 2.

Scatterplot of Price Elasticity by Private/Public Continuum



beyond the range of the data, one would draw the implausible conclusion that 54% of the California public were willing to pay a thousand dollars more in taxes.

This kind of anomaly would seem to suggest that elasticities increase at higher price ranges. To test this proposition, I took a closer look at three goods in my data set that were offered at widely varying price ranges: British Broadcasting Corporation subscriptions (a private good), anglers' support for fish hatcheries (a mixed good), and efforts to preserve endangered species (a public good). In each instance, there is strong evidence of acceleration of price elasticity. As shown in Table 2, a significant improvement in fit occurs when one models the process of acceleration by including a quadratic logged price term.[18] Inter-

TABLE 2

The Effect of Price on Willingness to Pay, Accounting for the Acceleration in Price Elasticity

VARIABLE	LOGIT ESTIMATE	STANDARD ERROR
Preserving endangered species[a]		
ln(Price)	.479	.088
ln(Price)^2	−.190	.018
Intercept	−.327	.044
Anglers' support for fish hatcheries[b]		
ln(Price)	.101	.298
ln(Price)^2	−.210	.047
Intercept	1.865	.475
Subscriptions to BBC channel 1[c]		
ln(Price)	.136	.794
ln(Price)^2	−.244	.092
Intercept1	4.190	1.730
Intercept2	4.764	1.720

Note: Estimates were obtained by maximum likelihood. ln(Price) represents the natural log of the asking price. Intercept1 refers to the intercept for cases gathered using a maximum price response format; Intercept2 is the corresponding intercept for cases gathered using a Gabor–Granger method (see Ehrenhart and Mills 1990). L_1 represents the log-likelihood for the quadratic model; L_0, the model without the quadratic term. df = 1, $p < .05$.

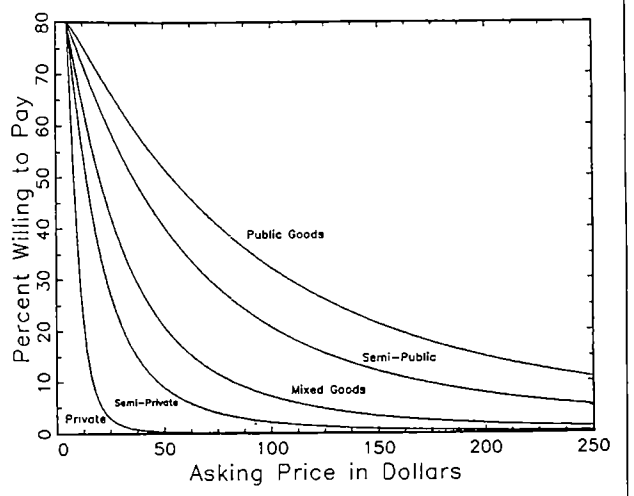
[a] $-2\ln\left(\frac{L_1}{L_0}\right) = 231.9$.

[b] $-2\ln\left(\frac{L_1}{L_0}\right) = 20.8$.

[c] $-2\ln\left(\frac{L_1}{L_0}\right) = 6.3$.

Figure 3.

Willingness-To-Pay Curves for the Median Good in Each of the Five Categories of the Public/Private Continuum



estingly, the rate of acceleration turns out to be quite similar for each of the goods. If anything, the estimates suggest that the rate of increase in elasticity is faster for goods that are private in character (nonsignificant, $p > .05$). The conclusions to be drawn from this digression are threefold. First, once the rate of increase in elasticity is taken into account, anomalies such as the one mentioned disappear. If willingness to pay to shelter the homeless is assumed to follow a trajectory similar to other high-priced goods, the predicted level of support for a thousand-dollar tax would be a more plausible 7%. Second, the notion that higher prices elicit more price-elastic responses accounts for two of the most troublesome outliers in Figure 2, deficit reduction and cleaning up the environment (both relatively price-elastic but classified as pure public goods). Had the prices offered for these goods varied between $10 and $50, rather than between $100 and $500, the elasticity estimates would probably have fallen within the range of −.30 to −.60. Finally, while the elasticity estimates in Figure 2 are to some extent conditional on the range of asking prices presented to respondents, the contrast between public and private goods persists when the rate of acceleration is taken into account.

Figure 3 illustrates the differences among the five categories of public and private goods. In order to facilitate comparison, I have assumed that 80% of the public is willing to pay $5 for each type of good. Each line depicts a trajectory of willingness to pay, assuming the median elasticity for private (−3.07), semiprivate (−1.40), mixed (−1.25), semipublic (−.59), and public goods (−.39). In order to take into account the acceleration in elasticity, I have assumed a quadratic effect of −.20 for each type of good.[19] Notice that by the time the asking price reaches $50, the five lines have diverged considerably: fewer than 1% of the public are willing to pay for the private good, whereas a majority are willing to pay for the public good. When price reaches $250, willingness to pay for the hypothetical private good is all but nonexistent; whereas almost 15% are still willing to pay for the public good. In short, when price increases by an order of magnitude, the result is a decline in the demand for both public and private goods; but the rate of decline is much more precipitous for the latter.

IS THE PRICE–QUALITY RELATIONSHIP A CONFOUNDING FACTOR?

So far, I have considered just one kind of price effect: the negative influence price exerts on demand through the operation of budget constraints. There is, however, a countervailing price effect, which stems from the fact that people sometimes infer a relationship between price and quality (Lichtenstein and Burton 1989). Consumers, for example, may suppose expensive wristwatches to be higher in quality; voters may infer that a $50 tax will rescue more homeless people than a $5 tax. Because this type of inference

tends to reduce the price elasticity of willingness to pay, one must guard against the possibility that the public goods in my sample are more prone to price–quality imputations than the private goods.

Experimental evidence drawn from the contingent valuation literature lends little support to the idea that the perceived link between price and quality is especially strong where public goods are concerned. Brookshire and Coursey (1987) asked one set of respondents how much they would be willing to pay to finance a precisely delineated plan to plant 25 additional trees in a neighboring park. Another set of respondents were asked about the same plan, only with the understanding that the more money raised, the more trees planted. Thus, in the latter case higher expenditures seemed to translate into a higher quality public good, whereas in the former case respondents were offered a "fixed bundle" of public goods. Yet my reanalysis of these data indicates that the price elasticity is not significantly different across the two conditions ($p > .05$). Moreover, willingness to pay for a fixed bundle of trees is *less* responsive to price than most other public goods listed in Figure 2.[20]

Another strike against the price–quality hypothesis is that the effects it ascribes to quality inferences are implausibly large. Consider, for example, what the distribution of willingness to pay to shelter the homeless would have looked like if public goods were in fact as price-elastic as private goods. In 1988, approximately 84% of the California public supported a $5 tax to shelter the homeless; 74% favored a $50 tax. If the elasticity were −1.5 (weak by the standards of private goods), then only 14% of the public should have supported a $50 tax. Clearly, the difference between 74% support and 14% support cannot be ascribed to the greater effectiveness of a program funded by a $50 tax; for (as I noted in discussing Kahneman's [1986] experiment concerning Ontario water pollution) willingness to pay is relatively insensitive to the scope of a proposed program (see also Brookshire and Coursey 1987; Kahneman and Knetch n.d.).

The final word on the influence of price–quality inferences awaits further experimentation. For the moment, I think it unlikely that imputations of quality account for the sizable differences between public and private goods; for if the scope or character of a public program were crucial, then one would expect to see a greater contrast between the elasticities associated with large programs funded by general tax increases (where an increase in price may well correspond to increased provision of a public good) and those that rely on voluntary contributions (where one's own expenditure has no real impact on the dimensions of a public good). I shall demonstrate that this contrast is all but absent.

ATTITUDES VERSUS BEHAVIOR

As I have noted, a certain amount of skepticism surrounds the use of surveys to measure willingness to pay. For this reason, I now take up the question of how the price elasticity of willingness to pay changes when money actually changes hands. A preliminary answer is that the difference is not great. Bohm's (1972) data on willingness to pay to see a comedy special, for instance, show no significant difference ($p > .05$) between the elasticities of verbal expressions and actual payments. Nor are lottery tickets (for which people actually paid cash; Knetch and Sinden 1984) distinctive in terms of price elasticity (see Figure 2). In general, experiments comparing actual payments and verbal expressions of willingness to pay for private and semiprivate goods have drawn the somewhat surprising conclusion that the two have very similar distributions (Dickie, Fisher, and Gerking 1987; Heberlein and Bishop 1986).

Much the same argument may be made for public goods. Data on United Way contributions by employees of three large corporations in the greater New Haven area were obtained.[21] The United Way is an umbrella organization that funds a variety of charitable organizations and programs. It is a pure public good, since payment is collective, consumption is nonexcludable, and few if any contributors benefit directly from its services. Moreover, because payment takes the form of contributions rather than taxes, those who are willing to pay do not have coercive power over others. This is significant in light of the concern that price may be regarded as an indicator of quality. Here, paying more does not mean that the United Way budget will expand proportionally. The United Way services are essentially constant across different prices to the contributor.

Price Elasticity of Contributions

What can the distribution of United Way contributions tell us about the relationship between cost and willingness to pay? The three firms surveyed vary considerably in the strength of their United Way campaigns. In Firm A, 77% of all employees contributed at least $10 to the United Way in 1989. The corresponding figures for Firm B and C were 58% and 24%, respectively. Despite these differences, the price elasticities of United Way contributions turn out to be similar across firms (see Table 3). When we pool the three firms together, we find that the elasticity of contributions is roughly −.3 when the "asking price" is $15, −1.4 when the price is $100, and −2.3 when the price is $500.[22] There is, in other words, a close match between the results obtained from the United Way data and the earlier conclusions based on verbally expressed willingness to pay for pure public goods.

Admittedly, this brief examination of charitable contributions to the United Way cannot be construed as a representative sample of contributing behavior, particularly given the diversity of religious, political, and cultural institutions that raise money. These findings, however, are consistent with studies of donations to the Humane Society (Fraser, Hite, and Sauer 1988), the American Cancer Society (Cialdini

TABLE 3

The Price Elasticity of United Way Contributions

FIRMS	ESTIMATE	STANDARD ERROR
Firm A[a]		
ln(Price)	1.128	.171
ln(Price)^2	−.279	.023
Intercept	.097	.294
Firm B[b]		
ln(Price)	1.232	.501
ln(Price)^2	−.288	.070
Intercept	−.980	.809
Firm C[c]		
ln(Price)	1.354	.430
ln(Price)^2	−.278	.060
Intercept	−2.617	.682
All Firms[d]		
ln(Price)	1.215	.151
ln(Price)^2	−.285	.020
Firm A	−.126	.257
Firm B	−.952	.268
Firm C	−2.212	.256

Note: Estimates were obtained by maximum likelihood. ln(Price) represents the natural log of the contribution. ln(Price)^2 is the square of this variable. Other variables are the intercepts for each of the different corporations. Note that due to the large number of cases, the elasticities of the three firms differ by a statistically significant degree ($p < .05$), although substantively, the trajectories are quite similar.
[a]n = 1,615; log-likelihood = −2,162.1.
[b]n = 342, log-likelihood = −406.1.
[c]n = 700; log-likelihood = −584.6.
[d]n = 2,657; log-likelihood = −3,161.1.

and Schroeder 1976), and political candidates in Ohio (Snyder 1991). These findings are also consistent with preliminary results from my own investigation of contributions to religious organizations, community programs, cultural institutions, and political campaigns. The only discrepant case I have encountered is the pattern of contributions to a local natural history museum, which displayed an elasticity similar to a semiprivate good. But here the money was raised through a membership drive, in which prospective donors were offered a package of benefits (free admission, a newsletter, etc.) in return for their contributions. Apparently, by marketing the consumption-related benefits associated with the museum, the fund-raising campaign increased the price-elasticity of willingness to pay; for its other campaigns, which did not solicit museum memberships, received more widely varying donations. Although the conclusions I draw from this limited sample of contributing behavior are necessarily tentative, the pattern of results is consistent with findings based on survey data.

DISCUSSION

One of the most hallowed principles in microeconomics is the idea that "a rise in price reduces quantity demanded, be it a rise in the market price of eggs, a rise in the 'shadow' price of children, or a rise in the office waiting time for physicians" (Becker 1976, 6–7). The findings presented here do not dispute this claim, for even pure public goods turn out to be price-elastic. But the evidence does suggest the corollary that goods that are evaluated solely in terms of personal consumption are particularly sensitive to price. Microeconomic reasoning is not inapplicable to mass political preferences; but neither is it especially powerful. Unlike the consumer, whose willingness to pay is quickly extinguished by price increases, the voter tends to be much less sensitive to considerations of personal cost.

Consider, for example, the limited influence of cost on public opinion toward redistributive social welfare policies. As I have noted, whether the price is $5 or $50, a sizable majority of Californians support a program designed to shelter the homeless. Analogous results obtain when respondents' social characteristics serve as proxy measures of price. The unemployed have more to gain from a government policy of full employment than those for whom unemployment is an unlikely prospect; moreover, the unemployed have less to lose, since the tax burden would fall more heavily on people with jobs. Yet, cross-sectional surveys of opinion show that the views of the employed and unemployed are not markedly different (Schlozman and Verba 1979); and panel studies indicate that those who experience unemployment or reduced hours during a recession do not become more supportive of policies designed to alleviate unemployment (Sears et al. 1980). Similarly, the medically indigent would benefit from a system of national health insurance at little cost to themselves; their costs would be borne by others. But despite the fact that the indigent and nonindigent would expect to pay different prices for national health insurance (to say nothing of differential benefits), the former are only mildly more supportive of the policy (Green 1988; Sears et al. 1980). In short, the costs of social welfare policies may differ from one social group to the next; but price is seldom a strong correlate of policy support.

If anything, data on the public's willingness to pay provide more evidence of self-interest than many previous studies of mass political attitudes. Considerations of cost, for example, determine whether support for expanding government funding for the arts runs below 45% or upwards of 65%. Not so for opinions towards affirmative action, social security, aid to education, or women's rights (see Citrin and Green 1990; Sears and Funk 1990), which show little covariation with personal costs or benefits. Two factors may account for the apparent contrast. Unlike previous research on the role of self-interest in public opinion, I have analyzed survey questions that state the price of policy support in an explicit fashion. Respondents did not have to figure out for themselves whether their jobs would be threatened by a state budget cut (Sears and Citrin 1985), whether their children would be affected by a desegregation ruling (McConahay 1982), or whether their family might experience a tragic loss at the hands of war

(Lau, Brown, and Sears 1978). Moreover, the fact that price was explicitly mentioned in the survey question meant that respondents were primed to think about considerations of cost—something that survey respondents do not ordinarily do when evaluating policy questions (Green 1988; Lau and Sears 1981; Sears and Lau 1983).[23]

Yet even when costs are made salient and stated in an explicit fashion, the price elasticity of public goods lags far behind the elasticity of commercial goods. What accounts for the contrast between political and economic goods? One explanation might be that people lack a clear sense of how much public goods should cost or, similarly, how much public good can be purchased for a given price (Cox 1986; Sears and Citrin 1985). Uncertainty about the appropriate price of a political good increases the variability in willingness to pay, thereby weakening the price elasticity. The implication is that if citizens had more experience as purchasing agents for public goods, the price elasticity of willingness to pay would increase. One might add that this hypothesis would also predict that economic goods with which the public is unfamiliar will tend to have lower price elasticities. Perhaps this is why a 1959 question concerning pay-per-view cable television produced the weakest price elasticity of the 11 private consumption goods.

Experimental evidence on the role of information shows that intensive exposure to going prices for consumption goods over an extended period of time produces more price-elastic preferences (Diamond and Campbell 1989). Comparable experiments have not been undertaken using nonmarket goods. Interestingly, however, the experiments that have been performed indicate that imparting information about public goods seldom alters the distribution of willingness to pay (Cummings, Brookshire, and Schulze 1986), a conclusion that also emerges from the data presented here. Michigan respondents, for example, were told the per capita cost of law enforcement before being asked their willingness to pay for more; yet the elasticity was just −.59. Information about per capita expenditures on the arts prefaced a series of willingness-to-pay questions; yet the elasticities hovered around −.52. And although Brookshire and Coursey (1987) took pains to inform respondents about the costs and benefits of planting trees in a nearby park, the elasticity was just −.22. While this line of explanation is intuitively appealing, the influence of price-related information on the elasticity of public goods has so far eluded detection.

An alternative argument is that asking price is simply one of several considerations that come to mind when people make decisions (Quandt 1956). Price may, for example, compete with feelings of sympathy or ethical commitments for the attention of the decisionmaker (Etzioni 1988; Sen 1977). Deciding whether to purchase cable television brings to bear relatively few considerations that go beyond price and consumption utility. By contrast, deciding whether to support a tax increase to shelter the homeless involves considerations such as sympathy for the destitute, the appropriate role of government, and perhaps certain racial attitudes as well. As a result, the cognitive weight assigned to price is reduced,[24] while variance stemming from diversity in political predispositions is injected into the distribution of willingness to pay, thereby weakening price elasticity.

The contrast between political and economic goods, according to this argument, hinges on the nature of what is to be decided. Conceivably, certain economic decisions, such as whether to purchase grapes during a farm workers strike, could involve normative considerations that distract attention away from price. By the same token, some political decisions may vary in terms of the potency and number of nonprice considerations that they raise. Over a wide variety of goods, however, this hypothesis suggests that price tends to receive less attention in the ballot booth than in the shopping mall.

A telling indication of how the absence of an ideological or moral dimension to a monetary choice affects the nature of willingness to pay may be found in Lynn's (1990) study of a restaurant that allowed its customers to choose the prices they paid for dinner entrées. At the conclusion of the meal, each customer was presented with a bill that listed four prices: a minimum price and prices 16%, 28%, and 38% higher than the minimum price. While Lynn makes much of the fact that almost half of the customers paid more than the minimum price they were offered, the results may be interpreted somewhat differently. In contrast to the relatively cost-insensitive pattern of contributions to the United Way, donations to the restaurant in Lynn's study displayed an elasticity of −21.8 (SE = 1.8), since just 2% of the customers voluntarily doled out the maximum asking price. Evidently, Adam Smith was right when he noted that economic enterprise could not survive on benevolence alone; for in absence of significant ethical or ideological considerations to call attention away from considerations of cost and utility, the tendency to economize asserts itself.

One further explanation for the apparent contrast between public and private goods has to do with the availability of substitutes. The market economy, one could argue, provides consumers with an array of different products to accommodate a given taste. Substitutes have the effect of winnowing the upper end of the willingness-to-pay distribution; for if the price of a television subscription becomes too high, people may satisfy their thirst for entertainment by watching movies or reading. Less winnowing occurs where public goods are concerned, one might argue, because nonmarket goods are less readily substitutable. What alternatives are available, for example, to those who wish to clean up air pollution in the Grand Canyon but discover that the price of doing so exceeds their willingness to pay?

At present, the role played by substitution effects remains an open empirical question. The literature on willingness to pay offers little guidance here; and my

data are rather ambiguous. Looking back at Figure 2, private goods would appear to be more readily substitutable. On the one hand, consumption goods such as refrigerators or new communications products (for which there are few substitutes) have sharp elasticities, while public goods such as subways or the arts (for which there are several potential substitutes) have weak elasticities. In none of the cases, however, were respondents encouraged to consider alternative means by which they might accomplish their public or private objectives.

The three explanations—price awareness, price salience, and availability of substitutes—are complementary. The next step is to assess the extent to which each factor explains the distinctiveness of public demand for nonmarket goods. If the price salience interpretation is correct, it should follow that the distribution of willingness to pay changes depending on whether respondents are primed to consider the instrumental or symbolic attributes of a particular good. If price awareness is crucial, then providing respondents with information about going prices should make preferences more elastic. And if substitution effects are important, priming respondents to consider a particular good against a backdrop of possible alternatives should enhance elasticity. The relative importance of information, symbolism, and substitutes is not only a matter of theoretical interest; it has important practical implications for those who wish to shape public opinion by enhancing or weakening the elasticity of public demand.

POLITICAL IMPLICATIONS OF THE PUBLIC'S WILLINGNESS TO PAY FOR PUBLIC GOODS

The results presented here may be regarded as something of a departure from previous findings concerning public attitudes toward taxes and spending. Since the advent of survey research during the 1930s, scholars have taken note of the "something for nothing" paradox: the fact that the public's deep-seated hostility toward taxes and "big government" coincides with its desire to see increased government spending on many (if not most) public services (Key 1961; Sears and Citrin 1985; Studenski 1939). The data analyzed here suggest that when forced to confront the trade-off between public goods and taxes, the public *is* in many instances willing to pay for increased spending in specific policy areas. Indeed, many of the surveys seem to indicate that the reservoir of willingness to pay for public goods at times runs quite deep.[25]

The survey context, however, tends to be quite different from the political context. Virtually all of the surveys I have encountered try to "sell" the idea of a tax increase to the respondent. Even the less tendentious questions avoid all mention of considerations that might undermine public support (see Appendix B). Actual campaigns, on the other hand, tend to be two-sided affairs; and the potency of negative symbols such as bureaucratic waste, big government, and undeserving beneficiaries is what often determines the fate of tax-related ballot measures.[26] The connection between survey responses and actual support for tax increases becomes more tenuous as one turns from ballot measures to legislative actions. In the survey context, respondents are asked whether they are willing to pay more to fund a particular public good. Very often, the interviewer makes the explicit stipulation that the funds generated will be used solely for the public good in question. While tax measures that feature this kind of one-to-one correspondence between revenues and expenditures arise from time to time in the form of social security taxes or war-related surcharges, many legislative acts, such as the 1990 federal tax increase, raise revenues for a host of public goods threatened by budgetary shortfalls. Under these circumstances, the level of public willingness to pay is contingent on how the public perceives the good that is to be funded through increased taxes. The public might begrudgingly accept a tax increase in return for "deficit reduction"; but tax increases designed to maintain "government services" are unlikely to have widespread appeal. Unlike law enforcement, health care, or environmental protection, nonspecific references to goods provided by government seldom prevail in trade-offs with "higher taxes" (Sears and Citrin 1985).

Finally, it should be noted that political considerations make it risky for public officials to cash in on the public's willingness to pay for public goods. Naturally, tax increases make no friends among citizens who profess unwillingness to pay the amount that is to be exacted from them. But even taxes that seem to fall below the threshold people are willing to pay may provoke electoral reprisal, since willingness to pay need not imply eagerness to pay. This, in combination with the fact that the public often loses track of the connection between revenues and services, means that public officials risk being remembered primarily as the politician who raised taxes. Small wonder that even when elected officials appear to have a clear survey-based mandate to raise taxes for the purpose of improving education, reducing water pollution, or fighting homelessness, their political instincts make them reluctant to do so.

As a practical matter, then, the percentage of the public willing to pay a given price for a collective good is a politically illusory datum. But the depth, or absolute level, of public support has not been my central concern. (Here, I depart markedly from previous research involving the valuation of public and private goods, which focuses almost exclusively on a population's mean willingness to pay.) Instead, the focus has been the *elasticity* of the public's valuation of collective goods, which is arguably more important in shaping the interplay between public opinion and policymaking. The fact that public opinion changes slowly in response to changing costs affords public officials considerable latitude in implementing policies, particularly when the policies are supported by

strong political attitudes. Enormously expensive undertakings, such as Operation Desert Storm, can be pursued on the assumption that if a majority of the public will swallow a $50 tax hike, a sizable proportion is probably willing to go along with a tax increase of $100.

But if inelastic demand frees the hands of policymakers in certain respects, it ties them in others. In particular, it is difficult for public officials to garner public support for a proposed program by reducing the costs it imposes. According to my estimates, slashing a proposed $100 tax in half tends to boost willingness to pay for a public good by only 5–10%.[27] Unless the supporters of an unpopular public good are willing to scale back their ambitions markedly, they will gravitate toward alternative strategies, such as redefining a good so as to appeal to the ideological or symbolic orientations of the electorate (Edelman 1964).

In conclusion, it may be said that the price elasticity of mass preferences and the nature of public discourse reinforce one another. The weak elasticity of willingness to pay for public goods reflects the extent to which questions of policy draw attention to symbolic or ideological issues. At the same time, the weak elasticity of public goods creates a system of incentives that shunts political discourse away from issues of cost or efficient alternatives, toward symbolic or ideological appeals. A similar reciprocal process seems to operate in the economic realm. The price elasticity of private goods may reflect the relative absence of considerations apart from personal utility; conversely, the potency of price may help make price-based appeals ubiquitous in the marketplace. Thus, not only are economic and political decisions different in character, but the fact that these decisions take place in different environments helps to sustain the schism between the consumer and the consumer's less price-conscious alter ego, the citizen.

APPENDIX A: LIKELIHOOD FUNCTIONS FOR WILLINGNESS-TO-PAY DATA

Maximum likelihood estimation requires one to specify the likelihood function for the observations. For the willingness-to-pay data analyzed here, this likelihood function may take one of two forms, depending on how the price questions are presented. The first likelihood function applies to surveys in which a single price is offered to each of several different subsamples (e.g., the homeless question posed to California respondents in 1989 in which half of the sample was asked whether they would pay $10, half was asked whether they would pay $50). Let w be a random variable representing the maximum amount each person is willing to pay to shelter the homeless. Let $\{p_1, p_2, \ldots, p_k\}$ represent an ordered, nonnegative series of prices to be offered. In this case, $p_1 = \ln(\$10)$ and $p_2 = \ln(\$50)$. Finally, let $I^{no}_{\$10}$ represent the number of people observed who refuse to pay $10; $I^{yes}_{\$10}$ is the number willing to pay $10. A similar notation, $I^{no}_{\$50}$ and $I^{yes}_{\$50}$, applies to the responses of the other half-sample to the asking price of $50.

The likelihood function for the data for an arbitrary probability distribution would be

$$L = \Pr[w < p_1]^{I^{no}_{\$10}} \cdot \Pr[p_1 \leq w]^{I^{yes}_{\$10}} \cdot \Pr[w < p_2]^{I^{no}_{\$50}} \cdot \Pr[p_2 \leq w]^{I^{yes}_{\$50}}.$$

Hence, the log-likelihood would be

$$\ln L = I^{no}_{\$10} \ln \Pr[w < p_1] + I^{yes}_{\$10} \ln \Pr[p_1 \leq w] + I^{no}_{\$50} \ln \Pr[w < p_2] + I^{yes}_{\$50} \ln \Pr[p_2 \leq w].$$

If one assumes (as I do throughout) that willingness to pay is distributed log-logistically, the likelihood function for the data is

$$\ln L = I^{no}_{\$10} \ln \left[\frac{e^{-a-bp_1}}{1+e^{-a-bp_1}}\right] + I^{yes}_{\$10} \ln \left[\frac{1}{1+e^{-a-bp_1}}\right] + I^{no}_{\$50} \ln \left[\frac{e^{-a-bp_2}}{1+e^{-a-bp_2}}\right] + I^{yes}_{\$50} \ln \left[\frac{1}{1+e^{-a-bp_2}}\right]. \quad \text{(A.1)}$$

This functional form extends easily to surveys that divide respondents into k different price subgroups. This likelihood function, incidently, is identical to the one used by statistical packages such as SPSS-X when they run "logit." The values of a and b that maximize this likelihood function are maximum likelihood estimates of the intercept and price elasticity, respectively.

The other likelihood function applies to cases in which a series of prices are offered to a single sample. (See the 1973 Harris questions concerning cultural facilities; the sample was offered an initial price of $50, and those who refused were asked whether they would pay $25.) Let $\{I_1, I_2, \ldots, I_{k+1}\}$ represent the number of respondents who fall in the various intervals between and outside the k price thresholds, so that I_1 people refuse to pay p_1, I_2 refuse to pay p_2 but agree to pay p_1, etc. Now the log-likelihood function becomes

$$\ln L = I_1 \ln \Pr[w < p_1] + I_2 \ln \Pr[p_1 < w < p_2] + I_3 \ln \Pr[p_2 \leq w].$$

Assuming w_i to be distributed log-logistically:

$$\ln L = I_1 \ln \left[\frac{e^{-a-bp_1}}{1+e^{-a-bp_1}}\right] + I_2 \ln \left[\frac{e^{-a-bp_2}}{1+e^{-a-bp_2}} - \frac{e^{-a-bp_1}}{1+e^{-a-bp_1}}\right] + I_3 \ln \left[\frac{1}{1+e^{-a-bp_2}}\right]. \quad \text{(A.2)}$$

As before, maximum likelihood estimates are generated by selecting values of a and b that maximize this likelihood function. Again, this functional form extends easily to the general case in which k prices are offered to a sample. In the text, I refer to the second

maximum likelihood estimator as "constrained logit," so as to distinguish it from the kinds of logit routines that presuppose equation A.1. Note that if the unconstrained likelihood function is used to estimate the price elasticity when constrained logit function is the appropriate likelihood for the data, the estimates are consistent but are inefficient for $k > 2$.

When the individual w are obtained from open-ended questions, one could maximize a likelihood function based on the probability density function for the log-logistic distribution. However, data from such survey measures typically contain zeros and probability spikes around "round" numbers (e.g., $5 or $10). For estimation purposes, continuous data were aggregated into intervals and estimated in a manner consistent with equation A.2.

APPENDIX B: NOTES ON QUESTION WORDING

A *Nylon Stockings 1963.* See Gabor 1985. N = 722.

a *Refrigerators (early 1950s).* See Adam 1958. N = 2143. Prices listed in figure are in thousands of francs.

B *Toilet Soap 1963.* See Gabor 1985. N = 1000 (approximately).

b *New Communication Product 1988.* The exact nature of the product cannot be disclosed. Proprietary survey data N = 511. Respondents were asked how likely they were to make a purchase of this product at $70. Those who did not respond that they would "definitely" buy were offered a price of $40. These data were combined with results from another question, which asked about R's willingness to purchase this good on a monthly basis. Prices here were assigned on a random basis ($3, $5, and $12).

C *Daily Newspaper 1973.* "Would you continue to get a DAILY newspaper if the price went to 15 cents? Would you continue to get a DAILY newspaper if the price went to 20 cents?" American Institute of Public Opinion 4–7 May 1973 (AIPO 870-k). National adult sample, analysis restricted to n = 907 receiving a daily newspaper; personal interview.

c *Hardback Books 1983.* "Suppose there was a book you wanted to have which was *not* going to be coming out in paperback. If the price was $15.95/$17.95/$19.95 how likely is it that you would buy the book—very likely, somewhat likely, not too likely, not at all likely?" Gallup 29 April–7 May 1983 (See Gallup Organization 1985). National adult sample, N = 1,471; personal interview.

D *Housing 1949.* "Which of the groups on this card come closest to the amount you would be willing to pay for a new house (apartment or duplex)?" Prices listed in the figure have been divided by 100. Roper Commercial Poll no. 34, 1949. Louisville adult sample, analysis restricted to N = 135 looking for a place to live; personal interview.

d *Lottery Tickets.* See Knetch and Sinden 1984. N = 64.

E *Health Insurance 1974.* "How much would you be willing to pay a year for health insurance if it covered all of your and your's needs?" Harris Study no. 7484, March 1974. National adult sample restricted to n = 234 who currently pay for health insurance themselves; personal interview. Prices in the figure have been divided by 10.

e *BBC Channel 1 1990.* See Ehrenberg and Mills 1990. N = 213. Gabor/Granger and maximum price formats were used for purposes of statistical analysis because the one-price surveys were separated in time by several months.

F *Baseball Pay-per-View Television 1959.* "If you could watch Dodger and Giant games on pay TV—that is, where you had to pay a certain amount to watch the game—about how many games a season do you think you would watch if they cost 25 cents each. What if they cost 50 cents. If they cost one dollar." Prices listed in the figure are in cents. Field 5902, 9–14 March 1959. California adult sample, analysis restricted to N = 342 baseball fans; personal interview.

f *Swedish Comedy Show 1971.* See Bohm 1972, experiments 1 and 5. N = 62.

— *Fishing Day* (mid-1980s). See Cameron 1988, 363–64. N = 1,033. I did not present the raw data for this analysis. The method of statistical analysis (constrained logit), however, is the same as mine.

G *Safety Caps 1982.* "How much more, if any, would you be willing to pay for an over-the-counter drug that now costs $2.00 if you could purchase it in the future in tamper-proof packaging: 20 cents more or higher, 15 cents more, 5 cents more, or no extra money?" Merit 1–4 November 1982. National adult sample, N = 1,201; telephone interview.

g *Clean Up Pollution 1974–75.* See Eastman, Randall, and Hoffer, 1974–75, Table 1. N = 526. The lowest price range, from 1 cent to one dollar, is treated as 25 cents to one dollar for calculation purposes.

H *Iraq Embargo 1990.* "President Bush's economic sanctions include a ban on Iraqi oil, which could wind up raising the cost of gasoline to $2 a gallon. How much more are you willing to pay for a gallon of gas than you paid last week in order to make the ban work? nothing, 25 cents, 50, 75, or $1 or more." Gordon S. Black Corporation 8 August 1990. National adult sample, N = 561; telephone interview.

h *Fish Hatcheries (mid-1980s).* See Cameron and Huppert 1989. Weighted N = 318.

I *World War II Effort 1943.* "In order to help pay for the war, should the Federal Government put a national sales tax of 10/5/3 cents on

everything you buy, in addition to the taxes now in effect?" American Institute of Public Opinion 27 January 1943 (288k/t). National adult sample, N = 1,472; personal interview.

i *Iowa Gasoline Tax To Improve Roads 1954.* "Some people say we should step up the improvement of our Iowa highways. There is enough money to continue farm-to-market road building, but not enough money to continue the stepped-up improvement of Iowa's primary and main highways. An increase of two cents tax per gallon of gasoline would provide an extra 18 million dollars, enough to carry on the stepped-up rate of improvement of Iowa's main highways. Would you favor or oppose increasing the state gasoline tax 2 cents per gallon to do this? Would you be agreeable to an increase of 1 cent instead of 2 cents per gallon?" Iowa Poll no. 136, September 1954. Iowa adult sample, N = 758; personal interview.

J *Gasoline Tax To Reduce Oil Consumption 1974.* "I'm going to read you some suggestions that people have made about possible steps we could take. For each please tell me whether you think it is something that you personally would be willing to do, or not? . . . Pay 25 cents a gallon more for gasoline, with a gas tax refund on your income tax, if it would cut back our consumption of gasoline and therefore reduce the amount of money we as a nation spend on oil. . . . Pay 10 cents a gallon more for gasoline, with a gas tax refund on your income tax, if it would cut back our consumption of gasoline and therefore reduce the amount of money we as a nation spend on oil." Harris, December 1974. National adult sample, N = 1,337; personal interview.

j *Civil Defense Alarm 1963.* "Actually, the Office of Civil Defense is now developing such a new instrument to alert the population if the United States were to be attacked by an enemy. This is how the device might look. Every American home that has electricity can receive warning through this instrument. When the device is plugged into an outlet, it can be set off by a special signal that goes through the regular community power system. When this happens, the instrument makes a loud buzzing sound that will alert people to turn on their radio for emergency information. The device would be triggered by electricity in a matter of seconds after warning is received. Everything considered, how likely is it that you would get an instrument like this for your home? How about if these instruments were for sale? How likely would you be to buy one of these instruments if the cost were $5/$10/$15/$25?" Note that the sequential price format (see Appendix A) was used for purposes of presentation in Figure 2; N = 672. For the random price format (see Appendix A), N = 1,366. National Opinion Research Center, December 1963 (NORC-SRS-330 Amalgam Survey). National adult sample, N = 1,366; personal interview.

K *Cigarette Tax To Ease Budget Crisis 1987.* "Currently California's tax on cigarettes is 10 [cents] per pack. It has been proposed that this tax be increased to 20/25/30/35 per pack. If the state needed additional money and taxes had to be raised, would you favor or oppose increasing the cigarette tax to 20/25/30/35 per pack?" Field 8702, 3–10 April 1987. California adult sample, analysis restricted to N = 246 smokers; telephone interview.

k *Drinking Water 1982.* "Consumers and taxpayers end up paying the cost of cleaning up water pollution through higher prices on products and higher taxes. I'd like to know how much you might be willing to pay to clean up water pollution. In order to make sure that your drinking water and the streams, lakes and rivers in your area are clean and safe, would you be willing to pay 100 dollars a year more in taxes and higher prices, or not? Well, about how much more per year in taxes and higher prices would you be willing to pay to make sure your drinking water and the lakes and rivers in your area are clean and safe?" Louis Harris and Associates, June–December 1982. National adult sample, N = 2266; telephone interview.

L *Commercial-free Radio 1945.* "Would it be worth it to you to pay a tax of $5 a year to get radio programs without any advertising in them? [If *yes,*] Would it be worth a tax of $10 a year? [If *yes,*] Would it be worth a tax of $25 a year?" NORC, October 1945 (no. 238). National adult sample, N = 2696; personal interview.

l *Muskoka Fishing (mid-1980s).* See Kahneman 1986. N = 167.

M *Haliburton Fishing (mid-1980s).* See Kahneman 1986. N = 164.

m *Ontario Fishing (mid-1980s).* See Kahneman 1986. N = 162.

N *Environment 1970.* "To really improve the environment, would you be willing to accept an increase of $200 per year in your family's total expenses, or not? [If *no,*] Well, would you be willing to accept an increase of $100 per year in your family's total expenses to improve the environment, or not?" Note that since in contrast to k and N, the payment vehicle is ambiguous, I code this as a semipublic good. Harris Study no. 1990, January 1970. National adult sample, n = 1,686; personal interview.

n *Clean Water 1984 (Low Prices).* "Earlier I asked you about Proposition 25, the clean water bond law, which provides funds for water pollution control, water conservation, and water reclamation projects. I would now like you to think about the costs of the program to the state and to you as a taxpayer, should it pass. What if Prop. 25 would cost your household an

additional four dollars each year for the next twenty years. If this were the cost of the program to you, would you vote for or against it? What if Prop. 25 would cost your household an additional $1/2/3 each year for the next twenty years. If this were the cost of the program to you, would you vote for or against it?" Field Poll 8406, 8–12 October 1984. California adult sample, N = 103; telephone interview.

O *Clean Water 1984 (High Prices).* "What if Prop. 25 would cost your household an additional $5/7.50/10/15/25/50 each year for the next twenty years. If this were the cost of the program to you, would you vote for or against it?" Field Poll 8406, 8–12 October 1984. California adult sample, N = 791; telephone interview.

o *Crime 1972.* "Each person in Michigan pays about $36 per year in taxes for police, courts, and prisons. How much more would you be willing to pay in state and local taxes to be used only for law enforcement and justice?" Response categories were "up to" $1, $5, $10, or nothing. It is unclear whether the prices should be treated as upper or lower boundaries. So as to err on the conservative side, we have treated these as upper boundaries: placement in the $5 category is assumed to reveal a willingness to pay of between $1 and $5, and $10 is assumed to reveal a willingness to pay of greater than $5 (since there were no higher response options). Treating these values as lower boundaries yields an elasticity of −.56. Michigan Attitudes toward Crime Study, September 1972. Michigan adult sample, N = 716; personal interview.

P *Crime 1974.* "Each person in Michigan pays about $45 per year in taxes for police, courts, and prisons. How much more would you be willing to pay in state and local taxes to be used only for law enforcement and justice? Response categories were as in o. Michigan Attitudes toward Crime Study, January 1974. Michigan adult sample, N = 797; personal interview.

p *Arts 1973.* "Would you be willing to pay an additional $50 a year in your taxes if the money were used to maintain and operate cultural facilities such as theatre, music and art exhibitions? [If *no* or not sure,] How about a tax rise of $25 to pay for such facilities, would you be willing to pay that much, or not? [If *no* or not sure,] Would you be willing to pay an additional $5 in taxes to make such activities more available?" Harris, January 1973 (see Farrell 1975). National adult sample over 16 years of age, N = 2,785; personal interview.

Q *Cultural Activities 1975.* "Would you be willing to pay $25 more in taxes per year if you knew that the money would be used to support arts and cultural activities and facilities, or would you not be willing to do that? How about a rise of $15—would you be willing to pay $15 more in taxes per year if you knew that the money would be used to support arts and cultural activities and facilities, or would you not be willing to do that? And what about a rise of $5—would you be willing to pay $5 more in taxes per year if you knew that the money would be used to support arts and cultural activities and facilities, or would you not be willing to do that?" Harris, June 1975 (A-H0027). National adult sample, N = 1,462; personal interview.

q *Arts 1984.* "The federal government now pays over $700 per capita for defense, $130 for education, and no more than 70 cents for the arts. Would you be willing to pay . . . $25/$15/$10/$5 more in taxes per year . . . for the arts, or would you not be willing to do that?" Harris 5–25 March 1984. National adult sample, N = 1,504; telephone interview.

R *Arts 1987.* "The federal government now pays over $900 per capita for defense, $140 for education, and no more than 75 cents for the arts. Would you be willing to pay . . . $25/$15/$10/$5 more in taxes per year . . . for the arts, or would you not be willing to do this?" Harris 13 March–6 April 1987 (no. 2436). National adult sample, N = 1,501; telephone interview.

r *South Carolina Environment 1971.* "Would you be willing to see an annual increase of $100 in your taxes if you could be assured that this would prevent future air and water pollution in the state, or not? [If *no,*] Would you be willing to pay $50 more in taxes to prevent air and water pollution? [If *no,*] How about $25—would you be willing to pay that much to get effective pollution control?" Harris no. 2130, August 1971. South Carolina sample, n = 803; personal interview.

S *Subway 1988.* "There has been much talk lately of building a subway in Los Angeles to reduce the traffic congestion and air pollution in that city. Suppose there were a bond issue on the statewide ballot to raise funds for a Los Angeles city subway project and that the cost to your household were an additional $5/$10/$25/$50 per year. If you were voting today, would you vote for or against such a proposal?" Field 8801, February 1988. California adult sample, N = 976; telephone interview.

s *Trees (mid-1980s).* See Brookshire and Coursey 1987. N = 48.

T *Environmental Cleanup 1990.* "Would you be willing to pay $500 more in taxes to clean up the environment if you knew the money was going to be used for environmental cleanup purposes only? Would you be willing to pay $200 more to clean up the environment if you knew the money was going to be used for environmental cleanup purposes only?" Yankelovich, Clancy, Shulman Poll, 27 No-

vember 1990. National Adult sample, N = 1,000; telephone interview.

t *Deficit Reduction 1990.* "In order to reduce the federal budget deficit, would you be willing or not willing to pay $100 more in taxes? Would you be willing or not willing to pay $500 a year more in taxes?" CBS News 7 October 1990. National adult sample, N = 775; telephone interview.

U *Endangered Species 1989.* "How much would you be willing to have your taxes raised each year in order to save endangered wildlife like the condor or the Northern Spotted Owl—or wouldn't you be willing to have your taxes raised at all for that purpose?" Los Angeles Times 17–21 November 1989. National adult sample, weighted N = 2,155; telephone interview.

u *Homelessness 1988.* "California has many homeless and destitute people. Next November there may be a ballot initiative aimed at providing food and shelter for homeless people. What if this proposition were to cost your household an additional $5/$10/$25/$50 in taxes each year? If you were voting, would you vote for or against this initiative?" Field 8801, February 1988. California adult sample, N = 985; telephone interview.

V *Homelessness 1989.* "California has many homeless and destitute people. Suppose there were a ballot initiative aimed at providing food and shelter for homeless people. What if this proposition were to cost your household an additional $10/$50 in taxes each year? If you were voting, would you vote for or against this initiative?" Field 8903, July 1989. California adult sample, N = 853; telephone interview.

v *Clean Air 1965.* "If it would cost each family an extra $100 a year in taxes to have air pollution greatly reduced, would you be willing to accept this expense? How much would you be willing to pay?" Opinion Research Corporation 4 May–1 June 1965. National adult sample, N = 1,064 (half sample); personal interview.

W *Clean Water 1965.* "If it would cost each family an extra $100 a year in taxes to have water pollution greatly reduced, would you be willing to accept this expense? How much would you be willing to pay?" Opinion Research Corporation 4 May–1 June 1965. National adult sample, N = 1,064 (half sample); personal interview.

Five survey questions not included in the analysis are a 1976 General Electric Survey (Study no. 76-04) question about willingness to pay for electric cars, a 1989 Gordon Black survey on willingness to pay more to fight illegal drugs, a pair of 1990 *Los Angeles Times* (study no. 212) questions about willingness to pay for national health insurance and retirement home care, and a 1989 *Los Angeles Times* San Fernando Valley Survey (study no. 189) question concerning charitable contributions to build an "arts park." In each case, the use of filter questions made the data problematic.

When available, weighted survey data were used. Responses of *don't know* or *not sure* were excluded from computation when questions asked respondents to name their highest price. Otherwise, *don't knows* were treated as *not willing* unless ambiguity existed about whether people whose recorded response was *don't know* or *not sure* were in fact asked the willingness-to-pay question. In some cases, *don't know* responses were lumped together with other missing data codes.

The data used in this study, as well as the GAUSS-386 programs used to derive maximum likelihood estimates, are available on request.

Notes

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1. Technical definitions of *willingness to pay* may be found in Brookshire and Coursey 1987 and Mitchell and Carson 1989. These authors use the terms *willingness to pay* and *maximum willingness to pay* interchangeably.

2. Asking price is by no means the sole determinant of willingness to pay. Varying budget constraints and access to substitutes may produce variation in willingness to pay. In addition, people may have different conceptions of what a particular good should cost and who should pay. Generally speaking, the greater the variability in determinants of willingness to pay other than price, the weaker the price elasticity.

3. The logistic is just one of many distributions that might be used to describe the dispersion of willingness to pay. Experimentation with the normal and Weibull distributions, however, produced such similar results that I leave aside the question of which distribution best describes the data. The only other study that employs a comparable statistical approach found the logistic to have somewhat greater predictive accuracy than the normal (Cameron 1988).

4. This procedure assumes that people who are willing to pay $10 are also willing to pay $5. This assumption seems reasonable, although one must be careful to choose plausible prices. Adam (1958) and Gabor (1985) have shown that when the asking price is too low, people become suspicious about the quality of the product.

5. Consider, for example, the case in which people are asked whether they would pay one or two dollars for a gold brick. Presumably, 100% would say *yes* to both, yielding no information about price elasticity.

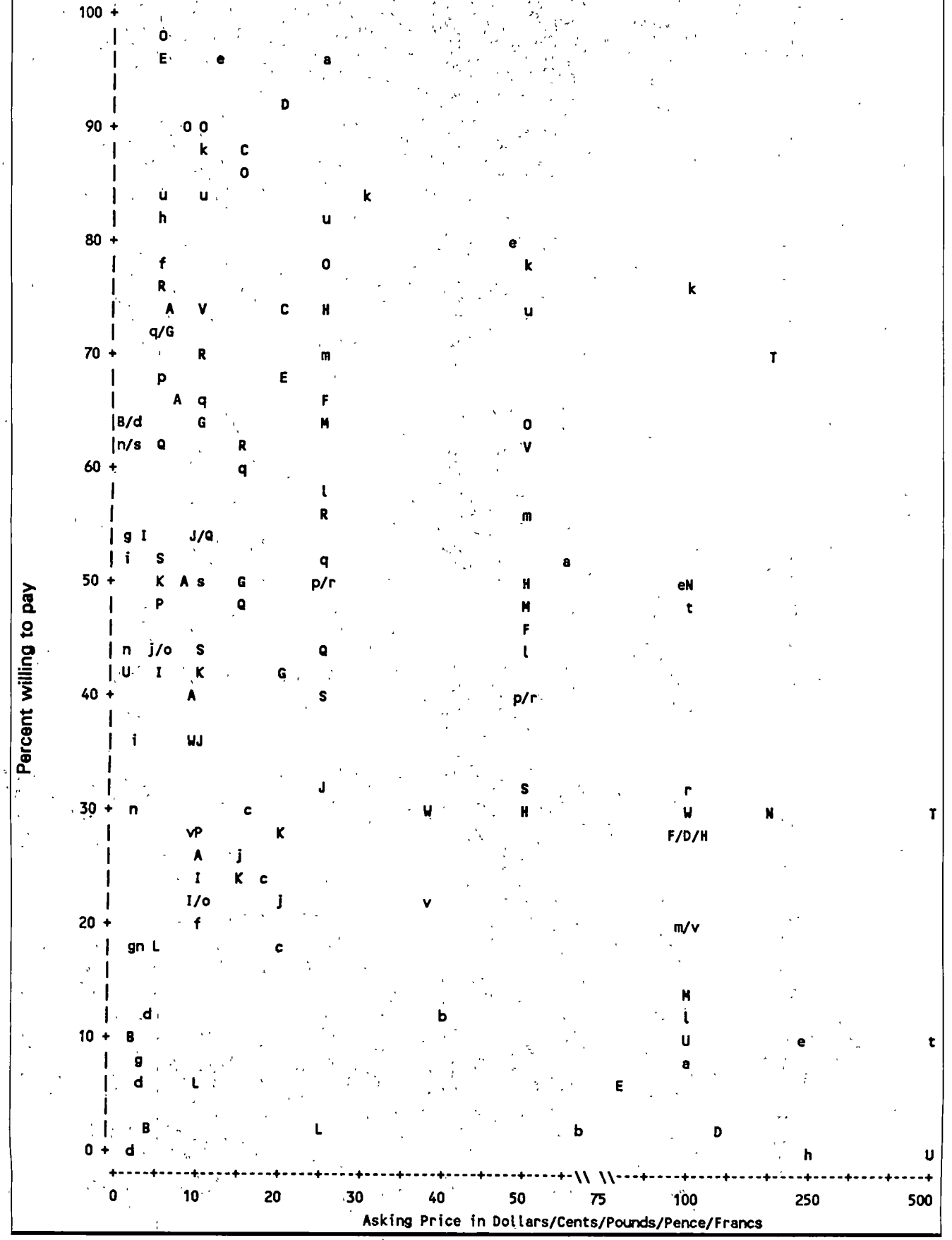
6. Sampling variability, of course, may account for an observed B of zero, especially if the range of prices is narrow. It would not be surprising, for example, to derive a B of zero from a small sample if the first price were $10 and the second $9.95.

7. This kind of closed-form solution is possible only in the special case in which two prices are presented to respondents. In the general case, estimates are obtained by iterative maximization of a likelihood function. Interested readers should consult Appendix B.

8. Certain alternative sources of data are less informative about willingness to pay than one might suppose. Experiments or quasi-experiments that track price changes and sales

Figure B-1.

Willingness To Pay by Asking Price, by Each Survey and Type of Good



of a given product allow one to estimate the price elasticity of aggregated demand, but not the price elasticity of willingness to pay. There are two reasons for this. First, the number of units sold does not necessarily correspond to the number of people who have made a purchase; some people may buy several units at one time (Cramer 1971). Second, those who purchase nothing go uncounted. Thus, one has no way of knowing how the proportion who are willing to pay changes with fluctuations in price. Data on local tax referenda are also unsuitable. While it might at first seem feasible to compare the level of voter support for different local tax increases, this research design has an important defect: the size of the proposed tax increase is likely to be endogenous, since those who desire a tax hike may tailor its size to the level of anticipated support. Further complications arise from local differences in turnout rates, income, and objectives of the tax proposal. The present research sheds light on the dynamics of public support for such tax increases, but voting data themselves are problematic.

9. As shown in Appendix B, a few of the questions I use ask respondents how *likely* they would be to make a purchase at a stated price. The proportion responding *definitely* or *very likely* was used as the proportion willing to pay.

10. The first is a 1988 Gallup poll that asked a series of 13 willingness-to-pay questions (see n. 24). The second is my finding that the elasticity of willingness to pay for bilingual education among Californians was a scant −.12 (Green 1988). The prices offered, however, differed from the prices offered earlier in the survey regarding homelessness and the Los Angeles subway. Respondents who had previously heard prices of $50 responded significantly more favorably toward a subsequent price of $25 than those who had previously heard $10. I dropped this case to avoid biasing the results in favor of my hypothesis concerning public vs. private goods.

11. Of the General Electric polls conducted between 1966 and 1981, 16 contain questions about willingness to pay for various public goods. However, the questions were asked only of respondents who said they would like to "see something done" in their region about each of the public goods. Thus, these data are unsuitable due to truncation bias. Elasticities calculated on these data, incidently, display the same pattern of over-time stability one observes elsewhere.

12. These results are consistent with data presented by Ehrenberg and Mills (1990, 27), which suggest that a series of prices tends to elicit more price-sensitive willingness to pay. Calculations based on their data concerning subscription television suggest that responses to a single price are about 80% as elastic as responses to a series of prices.

13. The elasticities for the two intermediate groups, "moderately frequent cultural attenders" and "infrequent cultural attenders" are −.52 and −.58, respectively.

14. For classification purposes, semiexcludable goods (i.e., goods owned collectively but consumed on a restricted basis, e.g., national parks that charge an admission fee) will be treated as public goods.

15. Complicating matters somewhat are taxes that take the form of surcharges on private goods. Consider the question of whether to raise gasoline taxes in order to reduce U.S. dependence on foreign oil. Here the question of willingness to pay involves both a public objective (limiting dependence), as well as private consumption (the gasoline one buys for personal use). Respondents may interpret this question as either one concerning willingness to pay more for gasoline or one involving willingness to pay more for energy independence.

16. In principle, these four dichotomous classification criteria generate a 16-cell typology. But as Appendix B suggests, the cases tend to cluster together, which rules out a more fine-grained examination of the role of each of the public/private criteria taken separately. I am currently collecting experimental data in an effort to understand better the distinctiveness of public goods.

17. Even more price-elastic than the private goods I included in the analysis are foodstuffs that have comparable substitutes. A February 1971 Social Science Research Council survey (Gabor 1985) found willingness to pay for butter and beans to have elasticities of approximately −10 and −5, respectively. Similarly large elasticities are to be found among Adam's (1958) consumption goods with close substitutes, such as cigarette lighters.

18. Following Cameron and Huppert (1989), I experimented with a Box–Cox transformation of price, but opted for the quadratic specification due to its superior fit to the data. In the quadratic model, the elasticity evaluated at any particular price is equal to the coefficient for ln (price) plus two times the coefficient for ln (price)^2 times ln (price). For example, Table 2 indicates that the elasticity of willingness to pay to preserve wildlife is −.40 when the price is $10.

19. The slopes depicted in Figure 3 seem to grow weaker as price increases; but in terms of log-odds, they are in fact increasing. Note that once one includes a quadratic term it becomes necessary to decide at what price the median slope applies. For the purposes of illustration in Figure 3, this price is $10.

20. Indeed, Figure 2 gives little indication that goods offered in a fixed bundle are significantly more price elastic. Among purely private goods, the cases where quality cannot plausibly vary with price are baseball television, lottery tickets, newspapers, and a communication product; two of these four are the least price-elastic goods in this class. In the middle of the private–public continuum, only one good is offered in the form of a fixed bundle, civil defense alarms, an item that falls in the midrange of elasticities in its class.

21. Requests for data were mailed to eight firms. Three firms, each employing over two hundred people, responded. Firms B and C distribute the standard United Way solicitation cards that ask employees to make either a one-time contribution or choose one of several levels of payroll deduction (employees could also volunteer their own level of payroll contribution). Firm A distributes its own cards: hourly and weekly employees receive cards similar to the standard United Way forms; management and officers receive cards that request larger donations. The data I analyze are actual contributions (as opposed to pledges) made through the firm, whether through cash, check, or payroll deduction.

22. See n. 19. Had I omitted the quadratic term and examined only those prices at or below $50 (relegating all prices over $50 to the *$50 and over* category), the price elasticity of United Way contributions across the three firms would have been −.45 (SE = .02).

23. Both factors—the transparency of self-interest and the saliency of considerations of personal benefits—seem to account for the fact that tax initiatives, smoking restrictions, and rent controls elicit an unusually self-interested public response.

24. Normative considerations may be particularly influential when people are primed to think about the desirability of a good; hence, the unusually weak price elasticities that turn up when respondents are asked their willingness to pay immediately after stating their eagerness to see government spend more. A Gallup survey conducted in 1988, for example, asked respondents whether they would like to see more federal spending on public education. Those who answered *yes* were assigned at random to one of two tax prices: one hundred dollars or two hundred dollars. Despite the rather large sums involved, 59% of the former group and 54% of the latter were willing to pay, for an elasticity of just −.29.

25. Survey data on willingness to pay to reduce the deficit, for example, suggests that the median citizen is willing to pay $94 more in federal taxes. Since the distribution of willingness to pay is skewed, however, average willingness to pay is approximately $1,000, even assuming a rate of acceleration of −.2.

26. Willingness-to-pay surveys have a respectable track record at forecasting referendum results (Mitchell and Carson 1989). But the ballot measures that have been studied (e.g., California's Proposition 25) have been environmental bond issues that faced no organized opposition.

27. Analogous arguments have been made concerning the efficacy of cost reduction in the face of public opposition in cases where the costs are nonmonetary. McConahay (1982), for example, has shown that white opposition to court-ordered school desegregation has little to do with the degree

of inconvenience associated with daily bus rides; he contends that tinkering with bus schedules and the location of bus stops is an ineffective means of currying public favor.

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MEASURING POLITICAL LEGITIMACY

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Political legitimacy is a key concept in both macro and micro theories. Pioneers in survey-based research on alienation and system support envisioned addressing macro questions about legitimacy with the sophisticated empiricism of individual-level methodology but failed; and a succession of innovations in item wording and questionnaire construction only led to an excessive concern with measurement issues at the individual level. I return to an enumeration of the informational requirements for assessing legitimacy in hopes of finding a conceptualization that better utilizes available survey indicators to tap relevant macro dimensions. I specify formal measurement models for both conventional and revised conceptualizations of legitimacy orientations and compare the fit of the two models systematically on data from the U.S. electorate. The revised model appears preferable on both theoretical and empirical grounds.

As one of the central concepts of political science, legitimacy would deserve sustained attention even in the absence of popular risings in Eastern Europe, China, South Africa, and elsewhere. But these events underscore the difficulty of the theoretical and methodological challenges with which legitimacy confronts scholars, and they justify renewed attention to both the potential of survey-based empirical research and the shortfalls of the conventional conceptualization.

Developing more valid measures of legitimacy orientations is not only worthwhile methodologically, it is also needed to improve our ability to distinguish among alternative explanations for important political events. The quandary of would-be reformers in the wake of Eastern Europe's democratic revolutions, whose prospects for improving their polities depend on properly diagnosing and addressing public grievances, illustrates this. At least three general interpretations of events have been offered (see Garton Ash 1990): one highlighting governmental inefficiency and incompetence, especially at economic management, one focusing on ossified and oligarchic Communist parties that stifled policy competition and upward mobility for non–party activists, and one emphasizing structural defects like the exaggerated accretion of central power at the price of regional autonomy.

Western behavioral scientists, whose national studies constitute the laboratories for new survey research, can distinguish the personal and political traits of citizens with greater or less faith in the regime and can usually situate citizen complaints along the continuum from politicians to policies to institutions (Weatherford 1987); but we lack the analytical resources to address systematically the more probing questions about institutional procedures and performance that reformers *must* address. If citizens complain about the government, for instance, are the problems with representational linkages (access and responsiveness) or with the elite policymaking process (procedural regularity and distributive fairness)? Aspects as fundamental as these are not ignored by the conventional approach to the public's legitimacy orientations, but their measurement is scattered and unsystematic. Whether this is because significant institutional reform has seldom been on the agenda or because survey researchers have focused too narrowly on measuring individuals' political traits, the upshot is that little empirical investigation has been devoted to assessing whether and how much such distinctions among aspects of institutional performance undergird expressions of political trust or alienation. I propose a revised conceptualization of the dimensions of legitimacy orientations that centers more directly on assessing these aspects of the public's image of government.

Political legitimacy is too unwieldy and complex a concept to be grappled in a frontal assault, and virtually all the empirical literature follows the tactic of breaking it into component parts. Thus, various lines of research (on alienation, political trust, modes of participation, political efficacy) all partake of a common interest in how citizens evaluate governmental authority. Many tasks can be more effectively completed by dividing the labor in this way, and time and habit invite us to take the current division as given. When we step back from these different literatures to view the global concept they all address, however, we can get a clearer perspective on the advantages and disadvantages of the conventional division.

To summarize briefly, the argument begins by distinguishing two different vantage points in the study of political legitimacy, a macro perspective emphasizing formal system properties, and a micro view emphasizing citizens' attitudes and actions. Although the early practitioners of survey-based studies of system support were motivated by the macro questions of historians and political theorists, as the methodology of survey research developed, the questions asked about legitimacy-like constructs have come to focus excessively on issues of methodology and measurement. This has weakened the ability of empirical researchers to ground individual-level findings in theories about the polity. I propose

an operational means of integrating traditional macro concerns more systematically with the methodology and agenda of behavioral research by conceptualizing the components of legitimacy as part of a multidimensional construct, specifying this dimensional structure as a formal measurement model, and testing its empirical fit in a confirmatory covariance structure analysis of data on the U.S. public's orientations toward the political system. Throughout, the intention is to build on, rather than supplant, the thirty-year tradition of survey-based research on legitimacy orientations.

TWO PERSPECTIVES ON POLITICAL LEGITIMACY

The concept of legitimacy is problematical because it plays such a protean role in accounting for political phenomena, taking on different aspects as it links global attributes of the political system with the orientations of individual citizens. Social scientists deal with many multilevel concepts, of course; but some, like *legitimacy* and *representation,* pose more difficult analytical problems than others. Because these systemic properties have no direct individual-level counterparts, they cannot be readily conceptualized by analogy with social choice phenomena, where the complexities center on choosing the appropriate aggregation rule, given the assumption of methodological individualism. In the case of legitimacy, the proponents of both system-level and individual-level perspectives can reasonably claim pride of place for their view, although they have quite different methodological approaches, take different empirical data as evidence about legitimacy, and orient research toward different sets of theoretical questions.

The View from Above: System-level Aspects of Legitimacy

Historians and political theorists distinguish legitimate from illegitimate governments by focusing on constitutional provisions that establish the opportunity for wide public participation and ensure procedural regularity, especially provisions dealing with majority rule, minority rights, and accountability in regular and frequent elections (Dahl 1956). These features of institutional procedure provide the structures of accountability and establish incentives for the ongoing responsiveness of officials to constituents. Other theorists add criteria against which to judge the content of policy outputs, typically evaluating proposals for altering the distribution of social and economic advantages either against some standard of equality, like Rawls (1971; see Gutman 1980) or against efficiency criteria, like Burns (1963) and Sundquist (1988). The distinction between procedural and distributive justice parallels this one.

These criteria judge government performance by how well institutions and officials discern—or define—and act on the polity's best interests (Pitkin 1967, chap. 7). The literature encompasses important distinctions; but for our purposes, the shared vantage point is more important: this line of research takes for granted the epistemic assumption that an outside observer, relying on fairly gross aggregate evidence, can measure the legitimacy of a political system and rank it in comparison with other systems. Of course, detailed public opinion data are not available in most of the cases historians and political theorists need to address; but it is not just a case of sour grapes that leads to their emphasis on the privileged status of the outside observer. Researchers in this tradition do not ignore public opinion, but they do argue more or less explicitly that the beliefs of citizens at any given time are not essential information for determining the system's legitimacy (see Rogowski 1974, 4–24).[1]

The strength of the traditional approach is its broad theoretical view and its formulation of legitimacy in terms of systemic properties that facilitate comparative analysis. Four attributes stand out in this formulation:

1. *Accountability.* Are rulers accountable to the governed via a process that allows wide, effective participation?
2. *Efficiency.* Is the government set up to accomplish society's ends without undue waste of time or resources?
3. *Procedural fairness.* Is the system structured to ensure that issues are resolved in a regular, predictable way and that access to decisional arenas is open and equal?
4. *Distributive fairness.* Are the advantages and costs allocated by the system distributed equally or else deviations from prima facie equality explicitly justified on grounds that define "fair shares" in terms of some long-run, overarching equality principle?

The weakness of the traditional approach is its tendency to concentrate on formal structures and aggregate processes, and its inadequate recognition of the complementary need to observe the political system's "subjective" aspects. The signal potential of survey methodology is its promise to illuminate the nexus between institutional context and individual psychology.

The View from the Grass Roots: The Relevance of Public Opinion

The development of survey research methodology following World War II provided the tools necessary to pursue this agenda. For many of the pioneers in the development of surveys, the challenge to explain political stability offered an unparalleled opportunity for exploring the capabilities of the new method to illuminate politically significant multilevel concepts such as legitimacy (Verba 1980). Early researchers took their lead from traditional theorists like Tocqueville, whose researches shifted the ground of

debate over political stability from formal constitutional properties to the orientations of individuals. To the allegation, widespread in his time, that democracy would lead to revolution, Tocqueville (1960) insisted instead that the careful observation of public opinion would show that far from being naturally revolutionary, the greater danger democracies face is that their citizens may voluntary withdraw from political life (see also Almond 1980). Tocqueville began his argument from the key distinction between personal and political antecedents of citizens' orientations toward government, and this division remains stalwart in the modern empirical literature.[2]

This approach highlights three aspects of the dynamics of public opinion, in explaining citizens' attitudes and actions vis-à-vis the regime. First, there are factors we would group together as part of the individual's psychological makeup, including the nonpolitical involvements that divert time and attention that might otherwise be spent on politics, along with the personal values underlying the individual's subjective sense of the importance of political participation. Hirschmann (1977), for instance, follows Tocqueville in stressing the effect of commercial and industrial pursuits in mitigating the emotion and diminishing the attention citizens give to political affairs (see Tocqueville 1960, 268–70), while Almond and Verba and later authors emphasize the role of social status and personal values (Almond and Verba, 1963, chaps. 6–9; Hyman 1966; Kluegel and Smith 1986; Mirowsky and Ross 1990). Second, there are interpersonal or social precursors of political involvement, especially the feelings of trust or suspicion that color expectations about other people and the sense of personal competence in dealing with abstract social and political forces. For instance, Noelle-Neumann (1984), like Tocqueville, focuses on the social dynamics that underlie tyranny of the majority (see Tocqueville 1960, 275–76), while others emphasize how social relations can build feelings of political competence (Almond and Verba 1963, chaps. 10, 11; Verba and Nie 1972, chaps. 9–13). Third, there is the public's sense of the capability and limits of the political process. Almond and Verba, for instance, like Tocqueville, envision a developmental process through which citizens' socialization and participation in politics nurture standards by which they judge the conduct of politicians and institutions (Almond and Verba 1963, chaps. 12–14; Tocqueville 1960, 270–77), while others envision a process more akin to consumer satisfaction, with evaluations updated and acted upon more frequently (Hirschmann 1970; Lyons and Lowery 1989; Orbell and Uno 1972).

Just as the macro perspective guides research toward the *context* within which political life takes place, the micro perspective emphasizes the reciprocal paths along which institutions shape public *orientations* and *expectations*, and public opinion then reverberates back to influence the operation of institutional processes. The prominent categories of individual beliefs are

1. *political interest and involvement*—the psychological feeling that political participation is worth the opportunity cost of trading off time and commitment from other occupations
2. *beliefs about interpersonal and social relations relevant to collective action*—expectations about the intentions and trustworthiness of other people
3. *optimism about the responsiveness of the political system*, based on citizens' perceptions of procedural and distributive fairness

It is by now a commonplace to note that the origins of survey research methodology, in sociology and social psychology, inevitably shaped the way theoretical questions were adapted to the new instrumentation. Formative initial research generally followed the sociological tradition in articulating the question of system stability in terms of political alienation, and the originators of key survey items were typically concerned with measuring the social psychological aspects of individual participation.[3] It is logical that the focus on developing measures of individual attitudes and opinions initially took precedence over theory building directed at modeling the linkage between psychological orientations and the political system; but over time, the balance between measurement and theory, at least from the perspective of the system-level aspects of legitimacy, has never been adequately restored.[4]

The Imbalance between Measurement and Theory

There is, of course, no universally appropriate equilibrium between theory building and measurement development. Presumably, any concerted research enterprise passes through multiple stages in which the relative balance shifts in one direction or the other. Observers will differ in their assessments; but most would agree that a research area is excessively measurement-driven if the bulk of activity consists of debate or experimentation involving alternative indicators and the separate measurement innovations rival one another, rather than successively contributing to the ability to explain empirical findings by grounding them in a larger theoretical context. In research on legitimacy, symptoms can be observed in the literatures on political trust, the public's perception of government responsiveness, and (to a lesser extent) efficacy and other aspects of alienation and system support. Typically, they err on the side of deferring excessively to conventional indicators or long-standing measurement models; but the overly modest use of theory even when doubts arise about traditional indicators also signals measurement-driven research.

Repeated use has inevitably had the effect of institutionalizing the conventional measures (Miller, Miller, and Schneider 1980); and some scholars have explicitly opted to take the indicators themselves (rather than their originating theoretical concepts) as the key entities for model building. Wright, for instance, suggests that available indicators should be

considered valid because they are "commonly employed" and there is no need to raise the issue "how best to measure political alienation; the manner in which it is usually measured suffices" (1976, 91, 95). Prominent examples of more moderate versions of the tendency also appear in the literature. Consider, for instance, the controversy over alternative interpretations of the conventional multiitem index of political trust—the one school holding that it indicates public favorability toward the government's policy outputs, the other reading it as an indicator of the popularity of incumbent officials.[5] This is, by any reckoning, an important controversy: the political trust index was introduced with the intention of monitoring the core elements of public support for the regime (Stokes 1962); and the two interpretations differ (on generality and temporal stability) in a way that relates logically to the concept of legitimacy as a systemic property. And yet, if we take a broader perspective on legitimacy and its place in theories of politics, this debate appears to miss the main issues, in two ways: it begins with the measures and tries to fit theoretical inferences to them, rather than the reverse, and it promotes the question of policies-versus-incumbents by construing legitimacy in terms of public approval for governmental outputs, rather than the more theoretically central issues of how citizens evaluate the system's procedural efficiency or distributive fairness.

A more subtle example of the inclination to defer excessively to the conventional image of how theory and measurement should relate appears in two technically sophisticated methodological reviews of research on legitimacy and alienation (Mason, House, and Martin 1985; Parker 1986). Both set out to test the statistical justification for the accepted grouping of questionnaire items into indices, generally by comparing the fit of hierarchically nested models; both demonstrate the weaknesses of the simple factor analysis techniques on which initial clustering was based; and both propose slight modifications of the conventional indices justifying the newly proposed scales essentially on statistical grounds. These evaluations provide useful information; but because they start from the conventional model and limit their tests to that specification, they can yield no theoretically grounded examination of current measurement strategies. The incompleteness of this approach illustrates what Wheaton calls "model inertia": given that many different sets of assumptions can be used to identify a model, achieving a satisfactory statistical fit for a particular specification leaves open the question whether other, quite different, specifications could account for the observed data. The temptation to narrow the analytical purview is especially strong when the model under investigation enjoys conventional status and a long history: "[T]he starting point assumptions used in identifying a model tend to determine the set of restrictions considered in testing a series of hierarchically defined nested models. The effect of the initial model often goes unrecognized. The problem of model inertia, whether it is due to personal commitment or the legitimation of certain specifications in previously published research, is the sense of uniqueness attached to a model that fits" (Wheaton 1988, 196).

Less frequent than instances where indicators are reified in place of the theory are examples in which researchers are appropriately skeptical of particular measures but inadequately aggressive in recurring to theory. Research progress on political legitimacy and its cognates has been stymied in the past; and, almost without exception, the immediate inclination has been to attempt to concoct new questionnaire items to measure the same theoretical entities for which the old measures did a poor job. I do not wish to minimize the genuine dilemma researchers face in a failed replication (the evidence is always interpretable as either weak measurement or poorly specified theory) only to point out the regularity with which innovation has concentrated on measurement at the individual level, rather than returning to the (macro) theory of legitimacy or to a systematic evaluation of the measurement model.[6] Something of the same tendency is visible in the history of failed efforts to disseminate promising measurement developments, where the translation of interesting local innovations to national studies appears to have been defeated typically by the appeal to the historical continuity of the conventional constructs, rather than being joined on theoretical grounds.[7]

The etiology of the imbalance observable between theory and measurement is doubtless somewhat different in each case, but the great preponderance of recent research has been characterized by a common approach. It begins not from the global concepts (legitimacy, alienation) that figure in theories of system stability or political behavior, but, rather, from their component parts as these have been defined by the accretion of conventional indicators. Continuity across the time series is a worthy goal, but it need not dictate that new research efforts must necessarily be bound by the old measurement models. I shall now review the dimensional structure of the conventional indexes and outline an alternative measurement model that specifies more explicitly both the macro and micro aspects of legitimacy.

THE DIMENSIONAL STRUCTURE OF LEGITIMACY ORIENTATIONS

I shall pursue a program of model specification and data analysis that attempts to combine the top–down perspective of system-level theories with the bottom–up perspective of empirical public opinion studies. Although drawing insights from both vantage points, the focus is at neither pole of the continuum but, rather, on the intermediate level between them, the *measurement model*—a middle-range formulation of the linkage between the concept of legitimacy as a multidimensional theoretical entity and the empirical indicators through which we observe citizens' legiti-

macy orientations. Measurement models are especially useful in discussing global concepts where the relationships among component parts are in question, for they provide a means of formulating theoretical statements precisely and unambiguously and of operationalizing and comparing alternative hypotheses about dimensionality. I compare two formulations: a model of the dimensional structure encapsulated in the conventional indexes of legitimacy orientations and a revised model based on the same indicators. With a concept as complex and multifaceted as legitimacy, both models propose complicated structures of relations among multiple components; universal agreement cannot be expected on the details. In order to avoid a comparison that depends on prior complete consensus, the revised model is compared with several alternative specifications of the conventional one.

The key feature of the conventional model is the separation between personal and political correlates of legitimacy orientations; within this general depiction of the concept's dimensionality, two other features are characteristic. First, the components of legitimacy (efficacy, trust, political involvement, etc.) are typically treated as if they were theoretically and statistically separable. The practice of assuming orthogonal components is usually explained on statistical grounds by pointing to the results of a simple factor analysis (see Miller, Miller, and Schneider 1980); but more discriminating statistical analysis shows that this assumption is inconsistent with the observed correlations among indices (Mason, House, and Martin 1985). The assumption is also questionable on theoretical grounds, where the traditional literature envisions the concept as a global variable comprising a nexus of several related components, at least within the "personal" and "political" categories (see Almond and Verba 1963, chaps. 8–12; Easton 1975). Second, survey-based studies typically aim at designing indicators of individuals' feelings of support or rejection of the government as a whole. Outside of the distinctions between policies versus incumbents and national versus local, the conventional indices often combine references to functions and structures that democratic theorists distinguish (e.g., bureaucracy vs. legislature, elections vs. policymaking). My intention is to work toward framing indicators of citizens' evaluations of specific political functions, particularly distinguishing procedural efficiency, equal access, distributive fairness, and accountability.

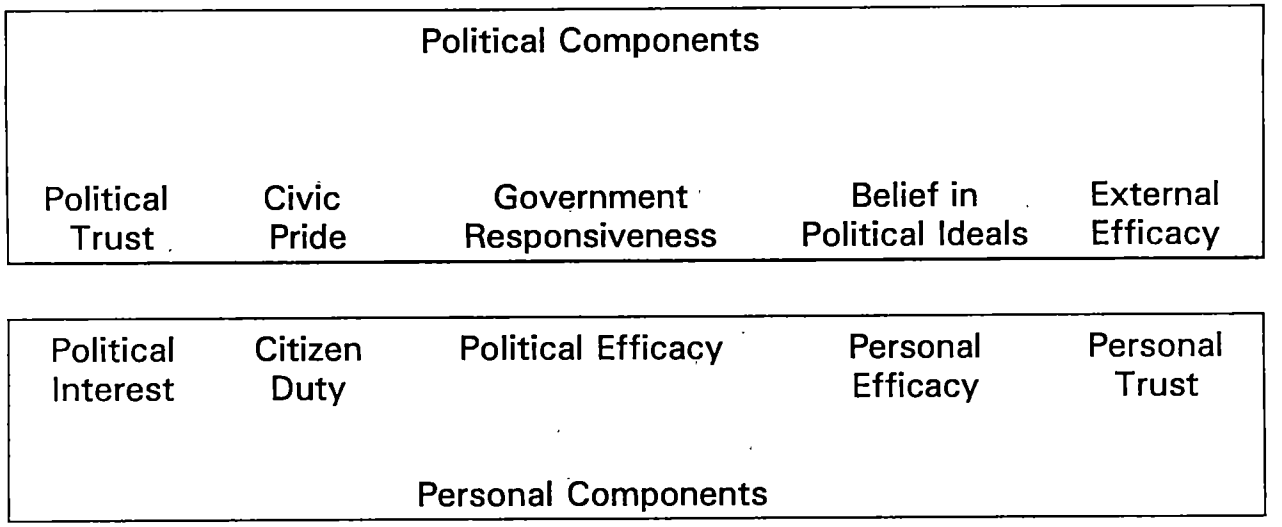
The first step in constructing a measurement model is specifying the way individual items are grouped into indices. Data come from the 1976 National Election Study—one of several from the late 1970s and early 1980s that would serve our purposes adequately, since they contain all the conventional items dating from the previous two decades, as well as some additional items carried over several of the biennial surveys. The relatively large item pool ensures more secure statistical estimation of dimensionality, while the inclusion of the main items forming the time series allows us to give appropriate weight to the claims of continuity. (Appendix A shows the conventional arrangement of legitimacy items.) Since this operationalization of the component dimensions of legitimacy is a familiar one (Miller, Miller, and Schneider 1980), it can serve as a starting point for a discussion of the revised model.

As with the conventional model, the distinction between personal and political sources of legitimacy orientations is central to the revised model. Beyond this similarity, however, the two take different paths in operationalizing the concept's dimensionality. The greater difference is in formulating indices and their interrelations for the political dimensions of legitimacy, where our interest centers on the extent to which the mass public's evaluations of governmental procedures and outputs mirror the distinctions at the heart of macro approaches—accountability, efficiency, and procedural and distributive fairness. These orientations fall naturally into two categories: those having to do with accountability and responsiveness (the linkage processes that relate the government to the citizenry) and those having to do with outputs and policymaking procedures (the performance of governments, once in power). Eventually, appropriate measurement of all these traits will depend on modifying old items and developing new ones; but I concentrate here on reconfiguring extant measures, partly in order to compare the two models in as balanced a way as possible and partly to establish the continuity between the revised model and the time series of conventional indicators.

Accountability is tapped by two of the conventional indices, *government responsiveness* and *external efficacy*. Unfortunately, these indexes combine references to the electoral process with items on officials' conduct as policymakers, distinctive aspects of the governmental process that macro theories differentiate. The revised index *accountability mechanisms* is comprised of items referring explicitly to political parties and elections as linkage mechanisms, calling on respondents to evaluate them in light of their ability to provide access and to hold officials accountable. The institutional mechanisms of accountability are crucial formal structures of legitimate government; but they are effective only to the extent that governing elites acknowledge and act on the institutional incentives intended to motivate responsiveness. Three of the conventional indicators tap citizens' judgments about politicians' actual responsiveness: *political efficacy, external efficacy,* and *government responsiveness*. All these, however, combine this aspect of governmental performance with other features, including personal traits of the respondent and structural traits of the government and the party system. The revised index *officials' attentiveness to constituents* more clearly taps this aspect of the representational linkage. The four items measuring this dimension all refer to the activities of officeholders to maintain attentive channels of communication open to the citizenry: two (*attention* and *don't care*) refer to government and public officials

Figure 1.

The Conventional Model of the Components of Legitimacy Orientations



in general, and two (*congressmen* and *lose touch*) cite Congress members in particular.

Three additional constructs tap citizens' evaluations of the government's outputs and policymaking procedures, distinguishing between procedural efficiency and overall fairness. Procedural efficiency is tapped by three items on the conventional index *political cynicism* but there they are combined with evaluations of the government's overall performance and the fairness with which various interests are represented. The revised index *officials' competence/efficiency* groups these items (*waste, incompetent,* and *crooks*) separately to focus on individuals' judgments of the competence and honesty of officials. The public's general evaluations of governmental fairness are tapped by items included in the conventional *political cynicism* index and by the cluster *belief in political ideals.* The new index *fairness of the political process* makes a separate cluster of items that refer to the policymaking activities of the government in general, rather than the actions of individual officials. Four of these call for respondents to think broadly in terms of institutions (*trust, big interests, represented,* and *fair courts*), and these cue respondents' comparisons against various formulations of the fairness principle; the other two (*blame* and *good politicians*) refer to political leaders in general and explicitly invite respondents to distinguish in their attribution of blame between individual politicians and the government as a whole.[8] The final index among the political or systemic correlates of legitimacy orientations, *civic pride,* replicates the conventional index, clustering two items that refer to approval in a highly general and patriotic way, rather than to the activities of particular institutions or politicians.

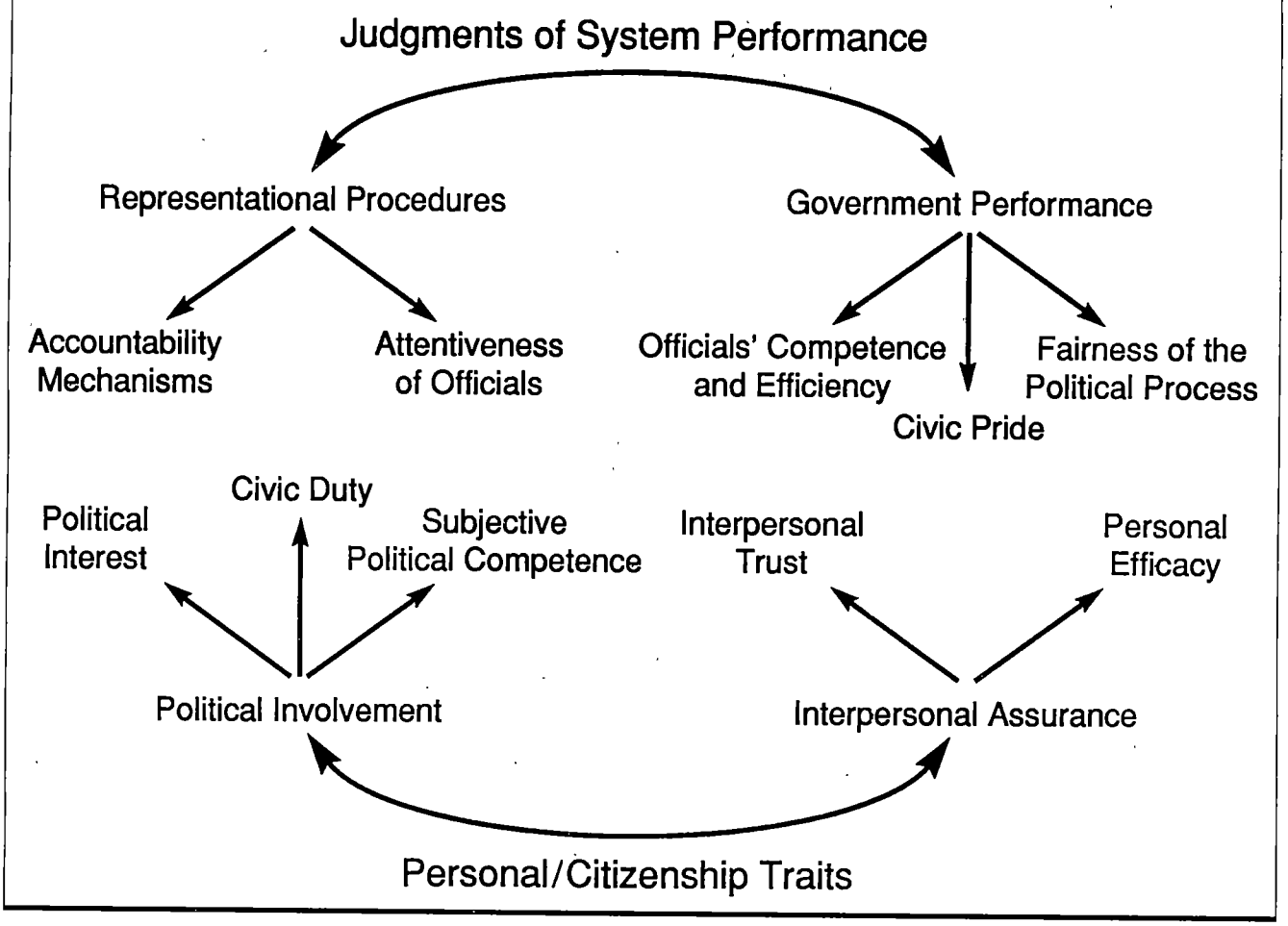
Complementing the political components of legitimacy orientations are the personal and social correlates of citizens' evaluations of the political system. The bulk of empirical research on legitimacylike concepts over the last several decades has been concentrated in developing and improving measures of these aspects; and my revised model carries over the dimensional structure of the conventional approach with fewer changes than were needed with the political correlates. Four indices of the revised model (*political interest, civic duty, personal efficacy,* and *interpersonal trust*) are brought over from the conventional model in identical form. The remaining index, *subjective political competence,* modifies the conventional index *political efficacy* in a minor way, by pruning one item (*don't care*) referring to the conduct of officials, so that the index focuses more precisely on the interaction between the individual and the political world in general. (Appendix B illustrates the revised configuration of indicators and constructs.)

The next step toward integrating political theories of legitimacy into a complete measurement model is to specify the relationships among these constructs explicitly. The conventional model distinguishes between input and output alienation or between the personal and political sources of attitudes toward the regime. This implies that the 10 indicators can be clustered into two broad groupings. The literature is not explicit on this point; but the distinction between personal and political presumably implies that correlations within each cluster are positive, although the use of simple factor analysis results to justify de facto treatment of the 10 components as separate indexes implies that these correlations should be small. Figure 1 illustrates the conventional conceptualization of the dimensions of legitimacy orientations.

The revised model looks to the theoretical precursors of modern survey-based research; and there, one is struck by the parsimony of formulations of the key dimensions of legitimacy. The revised model clusters the 10 first-level indicators into four constructs. Sys-

Figure 2.

The Dimensions of Legitimacy Orientations: The Views from "Above" and "Below"



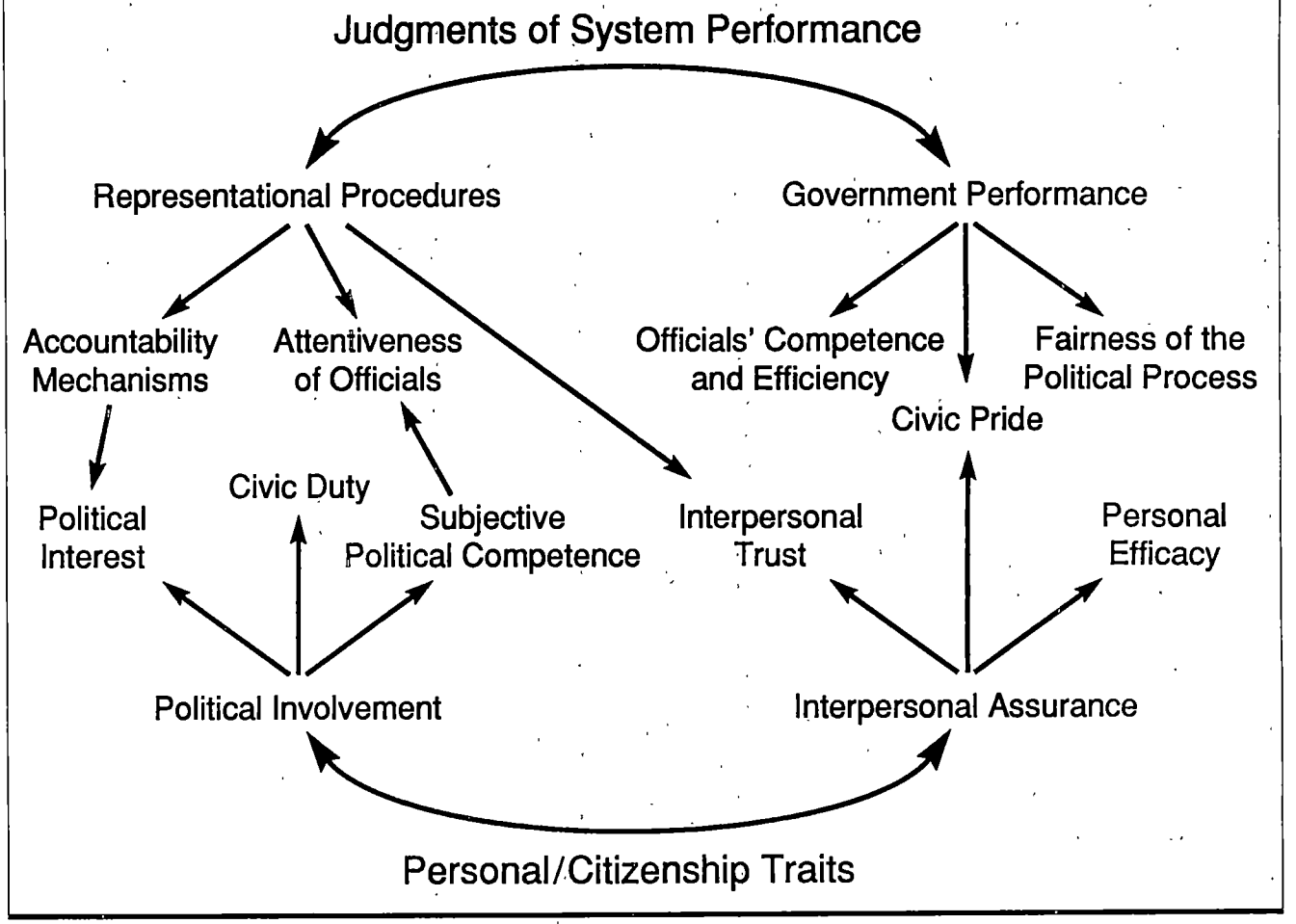
tem-level theories highlight two aspects of constitutional design and institutional performance—one centered on linkage between citizens and government, the other on the conduct and outputs of the governmental process. The indexes *accountability mechanisms* and *officials' attentiveness* tap public evaluations of the quality of representational links, while *officials' competence/efficiency, fairness of the political process,* and *civic pride* tap evaluations of governmental performance. Individual-level theories similarly emphasize two fundamental springs of legitimacy orientations—one centered on personal political involvement, the other on the social sources of interpersonal assurance. The indexes *political interest, civic duty,* and *subjective political competence* tap personal participation, while *interpersonal trust* and *personal efficacy* tap the political correlates of social relations. Figure 2, an interim illustration, shows this hypothesized dimensional structure.

The figure, however, overemphasizes the separateness of macro and micro. Both perspectives (one looking to the performance of the system, the other to the character of citizens) are linked by the paths along which both citizens' feelings about themselves and their fellows generalize to confidence in the political process, and citizens' experiences with the political system condition their optimism about the prospects for successful collective action (see Almond and Verba 1963, 32–36; Barry 1970; Lijphart 1980; Pateman 1971). The links between the personal and political aspects of legitimacy and citizenship orientations are forged by a complex, dynamic process in which different streams of causation predominate at particular points in the life cycle and different causal processes proceed, each at its own characteristic pace.[9] In the cross-sectional data available, we can expect to view only a snapshot of the outcomes of those processes; but while these data cannot unravel the genesis of political support, they can illuminate the connections among its dimensions at a given time.

Four paths link citizenship orientations and evaluations of political system performance. The justification for specifying these draws on the two central themes of the civic culture literature: (1) the role of personal political efficacy and success at social relations in fostering optimism about the political system and (2) the complementary role of the perception that

Figure 3.

The Structures of Relations Among The Dimensions of Legitimacy Orientations



the political system is open and effective in fostering personal political interest and building the foundation for collective action. Two paths specify causal flows from the personal to the political. Individuals who have a strong sense of mastery of the requisites of effective citizenship are more likely to perceive that representational mechanisms work effectively: *subjective political competence* influences evaluations of *officials' attentiveness*. And individuals who feel personally effective in their social relationships will be more likely to view the political system in a positive light: *interpersonal assurance* conditions *civic pride* (see Verba and Nie 1972, chaps. 9–13). It is logical to suppose that in the short run, the causal flow is along such routes, from relatively stable personal traits to more readily fluctuating evaluations of government; but over the long run, the polity also shapes its citizens (see Almond and Verba 1963, 493–497). Two such paths should be observable in the cross section. Citizens who perceive that the institutional mechanisms for maintaining accountability generally work effectively are more likely to take a vigorous interest in political affairs: optimism about *accountability mechanisms* fosters *political interest*. And individuals who view the government as generally representative and responsive are more likely to be optimistic about social relations and the prospects for nonpolitical collective endeavors: the perception of *governmental responsiveness* fosters politically relevant *interpersonal trust*. Figure 3 illustrates these hypotheses, specifying the second-order factor structure of the revised model.

The model summarized in Appendix B and Figure 3 has been justified and explicated by reference to previous theory and empirical literature and by noting the rationale of face validity for grouping observables with similar political content as indicators of the same theoretical construct. These are strong arguments; but on their own they cannot be definitive, for comparable claims of reasonableness and plausibility have been mobilized for the conventional specification. To be persuasive, the argument from theory and face validity must be combined with empirical evaluation. The empirical question is, If the revised conceptualization is preferable on theoretical grounds, does it comport at least as well as the conventional formulation with the way citizens think about the political system?

TABLE 1

Fit Indices for Models of Legitimacy Orientations

	GOODNESS-OF-FIT STATISTICS*					
				INCREMENTAL FIT	OVERALL FIT	
MODEL	CHI-SQUARE	DF	CHI-SQ/DF	BOLLEN'S DELTA	BENTLER-BONNETT COMP. FIT	HOELTER'S CRITICAL N
Null Model	8467.30	630	13.44	—	—	—
*Conventional Model***						
Parallel	6933.9	648	10.70	—	.094	117.23
Tau-equivalent	3795.2	620	6.12	—	.520	205.38
Congeneric	2960.4	594	4.98	.261	.633	252.83
Two common factors	1957.92	584	3.35	.572	.821	376.18
Twenty covariances	1783.82	574	3.11	.624	.846	406.20
2 factors + 4 structural paths	1708.2	580	2.95	.649	.857	428.38
*Revised Model***						
Parallel	8071.0	648	12.46	—	.050	100.71
Tau-equivalent	3922.3	620	6.33	—	.503	198.73
Congeneric	3215.4	594	5.41	.212	.598	232.78
Full Model	1355.52	578	2.35	.768	.901	538.07
Three corr'd errors	1195.77	575	2.08	.815	.921	606.96

* See text for descriptions of fit statistics.

** Successive versions of the conventional model and the revised model specify less restrictive assumptions regarding measurement properties (parallel, tau-equivalent, congeneric) and structural properties.

EVALUATING ALTERNATIVE IMAGES OF LEGITIMACY ORIENTATIONS: THE STATISTICAL EVIDENCE

The appropriate method for comparing alternative hypotheses about dimensionality is covariance structure analysis. Ideally, it would be possible to estimate two measurement models and then to resolve the issue by comparing the magnitude of their respective fit coefficients. For both theoretical and methodological reasons, we can approach, but not achieve, this neat resolution. Theoretical differences over the specification of the two models would make it unlikely that any resolution could claim consensual agreement. Debates occur over specification among adherents to the conventional model, and readers will doubtless suggest amendments to the model proposed here. We can narrow this potential source of indefiniteness by making our assumptions explicit and by following conservative estimation practices,[10] but additional theorizing and testing will be needed before a consensual model is developed. Even with agreement on specification, however, the methodology for comparing structural equation models in situations like the present one cannot yield an unambiguous determination that one or the other should be rejected. Although several complementary goodness-of-fit coefficients are available, no one of them provides a universally appropriate criterion for choosing definitively between nonnested substantive models.[11] My analysis, therefore, utilizes several complementary statistics based on the likelihood ratio test (Bollen 1989, 289–96; Matsueda and Bielby 1986). By comparing the two specifications sequentially at several stages of the model-building process and by using multiple coefficients to assess fit, it is possible to build a sound foundation of cumulated evidence on which to judge the balance of empirical support.

The fit statistics in Table 1, which presents the data for carrying out the relevant comparisons, include indicators of both overall fit and incremental improvement in fit. As measures of overall fit, we compute chi-squared and its associated degrees of freedom, χ^2/df, the Bentler–Bonnett comparative fit index, and Hoelter's Critical N. Chi-squared remains the most commonly used fit coefficient; but its interpretation is ambiguous, since it depends not only on the fit function but also on sample size, being biased against models in larger samples (Long 1983). The ratio χ^2/df has sometimes been advocated to supplement or replace the chi-squared test, since it has an intuitive interpretation in terms of "fit per overidentifying restriction." Although various studies have suggested threshold levels of this coefficient for determining sufficient fit (Wheaton et al 1977 proposes 5; Carmines and MacIver 1981 propose 2), there is no strong rationale for the measure since it maintains chi-squared's dependence on sample size. The comparative fit index (Bentler 1990; Bentler and Bonnett 1980; Bollen 1989, 131–35, 289–92) resembles the adjusted goodness-of-fit index of Joreskog and Sorbom but has the advantage of being a slightly more conservative indicator; its usual threshold for sufficient fit is .9. Most goodness-of-fit measures are

complicated in their construction and interpretation by the need to compensate for the confounding effect of sample size. Rather than correcting the fit function and likelihood ratio for sample size, Hoelter's (1983) Critical N index uses sample size to gauge fit. The problem is that a theoretically sound model is more likely to be rejected in a large sample; Critical N is the sample size at which the model would be accepted at a given confidence level, so that larger values of Critical N are better. Hoelter shows that a reasonable threshold for this measure is 200; I calculate Critical N at $pr(\chi^2) \geq .05$.

To these measures of overall fit, we add a coefficient of incremental fit to assess the gain from successive modification of each of the basic models. Sobel and Bohrnstedt (1984) point out that the baseline for indices of overall fit is a complete null model that is often unrealistically naive in that it takes a more theoretically agnostic position than the literature warrants. This results in a statistical criterion that produces too-ready acceptance of relatively simple models that improve on a situation of complete ignorance and too little capacity to distinguish between sequential refinements that typify a more mature research area. They propose measuring incremental improvements in fit against an "informed null" baseline (specified as a substantively meaningful but restrictive model), given current theory. My analysis employs a sophisticated factor analysis model as the baseline, and Table 1 shows incremental fit measured by Bollen's delta coefficient.[12]

Table 1 displays these fit indexes for a series of less restrictive specifications of each model. The first three are essentially factor analysis models operationalizing successively weaker assumptions about the measurement properties of the indicators (Alwin and Jackson 1979). *Parallel* measurement is the strongest possible measurement model; it assumes that the contribution of the construct to each of its associated indicators is equal and that their reliabilities are the same (i.e., either each measure of a given factor has the same loading or all the loadings equal 1.0 if the variance of the factor is a free parameter, and all the error variances are identical). *Tau-equivalent* measurement retains the assumption of equal factor loadings but allows reliabilities to differ. *Congeneric* measurement allows variation on both properties: the unobserved construct need not contribute identically to each of its measures, nor need the indicators have identical reliabilities. Following Sobel and Bohrnstedt (1984), I specify tau-equivalent measurement as the appropriate informed null baseline.

I first review the statistical evidence for each model separately, then return to their comparison. The upper portion of Table 1 shows goodness-of-fit statistics for the conventional model. The conventional approach has typically treated political trust, political efficacy, governmental responsiveness, and other indices as independent measures of discrete traits; and if this formulation is taken as definitive, then the orthogonal factor structure specified by the congeneric measures model is the appropriate operationalization. Hoelter's Critical N, which comfortably exceeds the threshold value of 200, implies that this model fits the data adequately; but none of the other indices supports this inference.[13] Although a useful starting point, this is arguably too conservative an interpretation of the model for the conventional indices: the assumption of orthogonality has never been universally accepted. At the same time, there is no strong warrant for any particular formulation of how the conventionally measured components are related and hence no consensus as to the route along which the model could be improved by freeing specific sets of restrictions. The general distinction between input and output alienation suggests two clusters of indicators (one for "personal," the other for "political," sources) and thus provides a theoretical rationale for respecifying the model (see Bollen 1989, 296–305). Following this lead, the empirical relations within each cluster could be specified as covariances or as causal paths. The causal hypothesis would be that a single construct gives rise to the five personal traits, while a separate orientation causes variation in the five indicators of political evaluations. Fit statistics for this specification appear in row 4, the conventional model, where 10 structural paths are estimated as free parameters. The correlational variant hypothesizes that the five indicators in each cluster are directly linked but does not assert the operation of a common factor. Within each cluster there are 10 pairwise covariances, and fit statistics for this less restrictive specification are given in row 5.

Table 1 shows goodness-of-fit coefficients for these two specifications. Both formulations are built around the conventional arrangement of items into indices, abandon the assumption of orthogonal indicators, and produce clear improvements in fit, as shown by Bollen's coefficient of incremental fit, which increases from .26 to .6.[14] Either seems a fair representation of the general thrust of the conventional model's conceptualization of the dimensions of legitimacy orientations. Although consensus is lacking on the way personal traits might relate to system orientations (measured by the conventional indicators), a more generous test of the conventional model would allow some structural links to be estimated as free parameters. The final row under the conventional model shows fit statistics for such a model, estimated with four free structural parameters.[15] The overall fit of each of these models would be judged acceptable on the basis of the Critical N index, but they fall short of sufficient fit when compared with the Bentler–Bonnett statistic or the more familiar χ^2/df.

The lower portion of Table 1 arrays corresponding statistics for the revised model. As with the conventional model, we see steady improvement in fit moving from more restrictive to less restrictive factor analysis specifications. Moving from these stock formulations to estimate the full version of theoretically derived model (depicted in Figure 3) results in a pronounced improvement in fit, with Bollen's coefficient of incremental fit increasing from .21 to .77. The

coefficients of overall fit tell the same story: χ^2/df and the Bentler–Bonnett comparative fit index move up to their threshold values for sufficient fit, while Critical N comfortably exceeds its threshold value.

Because the conventional and revised models are not nested, precise comparisons of fit statistics cannot be taken as definitive. Nevertheless, comparability is strengthened by the fact that they are estimated on the same population and specified with approximately equal numbers of free parameters. Without placing too much weight on any one measure of fit, the consistency across all the coefficients is impressive in supporting the empirical superiority of the revised model.

The last row of Table 1 shows goodness-of-fit statistics for a further specification of the revised model, this one freeing covariances across three pairs of error terms. The rationale for testing an explicit model of measurement error returns us to our initial agenda. I have stressed throughout the desirability of formulating a new model of the dimensions of legitimacy orientations that builds on previous work and whose analysis can make use of the available time series of survey data. Building up the revised model from items originally designed to tap the conventional formulation serves this purpose; but it arguably also produces a very conservative test of the empirical support for the revised model, for reasons springing from both the content of the items and the sequence of questions on the survey. Specifically:

> The new indicators are operationalized with items purposely constructed to measure the old indices. Over the years, initial items have been dropped or reworded and new items added, with an eye to improving the scalability and orthogonality of the conventional indices. The extent to which these changes have been successful will register in higher inter-item correlations for the conventional indices.
>
> The data were generated by a survey designed with the conventional indices in mind; specifically, each of the conventional indices is presented to the respondent as a sequence of items, with all the items typically having similar format and wording. The adjacency and similarity of the items increase the likelihood that some of their observed correlation is due to patterned responses induced by the questionnaire instrument (see Sears and Lau 1983).

If such measurement artifacts do inflate correlations among items on the conventional indices, then reconfiguring the items into new indices may worsen the fit of the model, because it moves this artifactual correlation from within indices to between indices. The final row of Table 1 shows goodness-of-fit statistics for the model in Figure 3, now estimated with three error covariances specified as free parameters. The resulting improvement in fit (Bollen's delta increases from .77 to .82) underscores the conservative nature of our test of the revised model's empirical support.[16] Estimated on data that make the statistical test a conservative one, the revised model appears to fit the data well, with a Critical N over 600 and a comparative fit statistic of .921.

CONCLUSION: IMPLICATIONS OF THE REVISED MODEL FOR RESEARCH ON SYSTEM SUPPORT

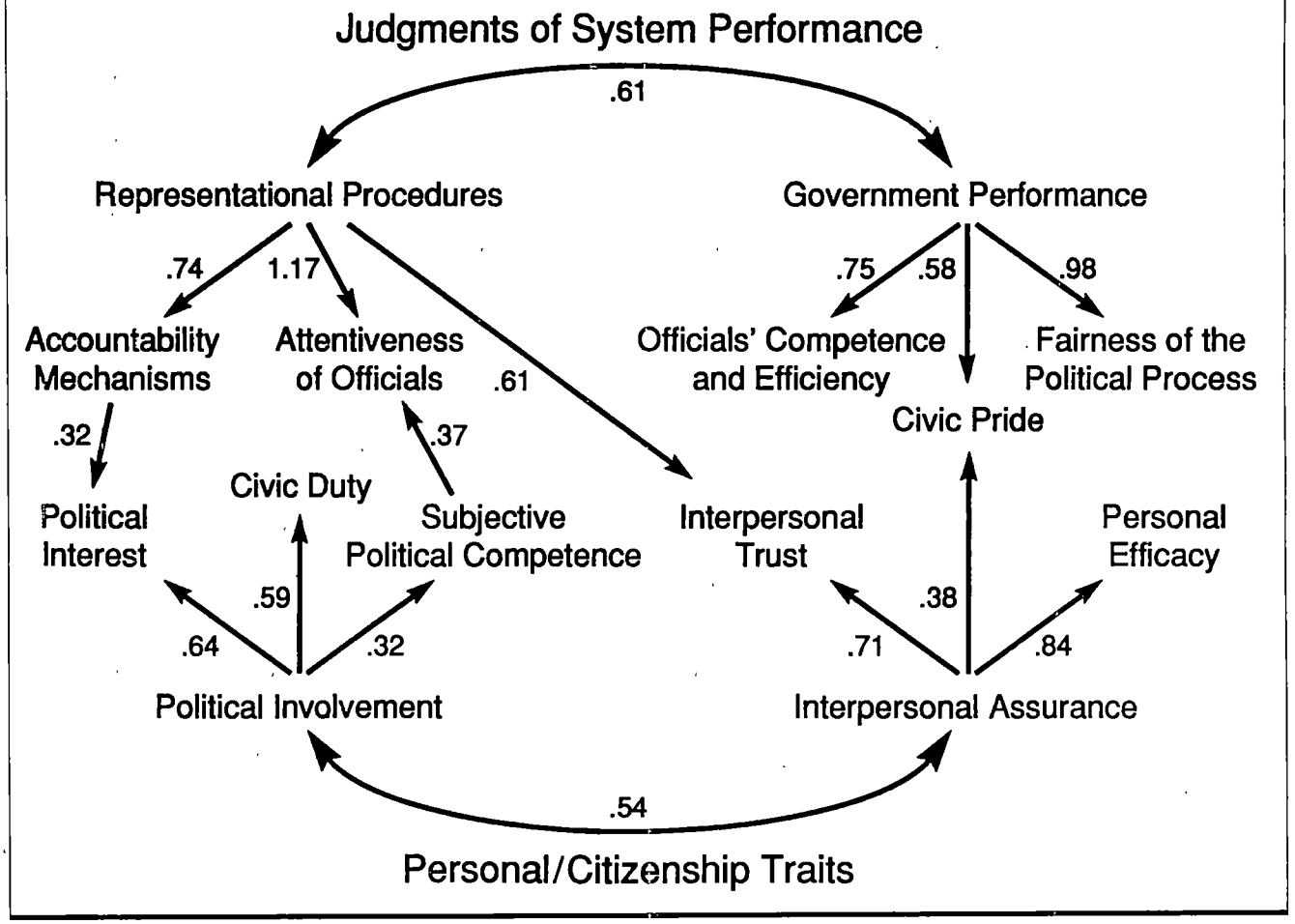
The research agenda guided by the conventional model has produced an admirable track record of findings on the mass public's feelings of trust and favorability toward the government. We can use the conventional indicators, for instance, to discover whether citizen complaints about government center on officials or policies; and we can parse them for detailed information about the social characteristics and political self-images of supporters and skeptics. These are important questions about public opinion, and answering them should eventually help in explaining and predicting political behavior. To date, however, the conventional formulation has not delivered on its promise to connect survey-based research on legitimacy with the macro issues at the heart of theories of system stability.

The reason for this is unlikely to be inadequate inventiveness in formulating specific items on the survey questionnaire, since a host of measurement innovations have sought improvement along that route. I have pursued the alternative strategy, returning to macro theories of political legitimacy for inspiration in reconceptualizing how the multiple components of this global construct fit together and how they might best be measured. When we view the shortfall between survey-based findings and system-level inferences from the perspective of macro theories rather than survey methodology, the resulting model of the dimensions of legitimacy differs correspondingly. The distinction between the revised model and the conventional one is relatively small where the conventional approach is at its strongest—in measuring personal political orientations and mapping the way social relations and interpersonal trust condition political attitudes. The distinction is marked where the conventional model does a poorer job—in targeting the aspects of political legitimacy central to system-level theories, accountability, efficiency, and procedural and distributive fairness.

Figure 3 presents the revised formulation as a covariance structure model. Several differences distinguish it from the conventional model. Most importantly, where the conventional model implies that five separate dimensions comprise the nexus of "political" sources of legitimacy orientations, the revised model groups a modified set of five first-stage indicators into two second-stage clusters, one highlighting representational institutions and practice, the other the policy performance of officials and institutions. The conventional model's five dimensions of personal/citizenship traits are similarly grouped by the revised model into two second-stage clusters—one focusing on political interest and subjective competence, the other on trust and efficacy in social relations. Finally, the new model explicitly specifies two pairs of structural paths spanning the personal and political. Figure 4 shows maximum likelihood

Figure 4.

The Dimensions of Legitimacy Orientations: Parameter Estimates



estimates of the structural parameters of this model, and several comments can be offered on these results.

I begin with the personal and citizenship traits at the bottom of the figure. The second-stage factor *political involvement* gives rise to a trio of indicators of the behavioral and attitudinal correlates of participation. *Political interest* and *civic duty* index active involvement or the feeling of responsibility to participate, and they dominate this factor; *subjective political competence,* which refers not to behavior but to attitudes that indirectly underlie involvement, shows a somewhat weaker but still significant causal relation to the common factor. The second-stage factor *interpersonal assurance* underlies two sets of social experiences that build (or subvert) the confidence to participate in collective political pursuits. *Interpersonal trust* taps the extent to which other people are viewed as reliable and well intentioned, while *personal efficacy* indicates the individual's sense of mastery in organizing and planning his or her life. The two second-stage factors, *political involvement* and *interpersonal assurance* are correlated as part of a general nexus of nonpolitical sources of legitimacy orientations, but the fact that they share only about a quarter of their variance (r = .54) gives empirical support to the specification of separate social and individual components.

Turning to the political sources of legitimacy orientations, the second-stage factor *representational procedures* underlies two indicators of citizens' evaluation of the effectiveness of elite–mass linkages. *Accountability mechanisms* indexes the extent to which the individual sees elections and party competition as institutions that press politicians to pay attention to their constituents, while *attentiveness of officials* taps the public's judgment about whether politicians actually do take proper account of citizens' views. The relative magnitudes of their structural parameters suggests a sensible evaluative rubric that places actions above formal intentions. That the representational connection is a key aspect of citizens' views of the system's legitimacy is underscored by three significant structural paths linking the political and personal, suggesting that (1) personal political interest is sustained in part by the impression that electoral competition heightens accountability; (2) the more politically competent the citizen feels, the more optimistically he or she will view the responsiveness of public officials; and (3) the more effectively the system's representational institutions work to con-

nect citizens meaningfully to the world of politics, the more optimistic they are likely to be about the prospects for collective social efforts.

The second-stage factor *government performance* underlies citizens' responses to three indicators of the quality and fairness of the policy process and its outputs. *Officials' competence* taps the public's judgment about the efficiency and honesty of politicians and bureaucrats, while the more strongly normative index of *fairness of the political process* reflects citizens' sense that the policy process equitably reflects the needs of the unorganized and the organized, private interests and the public interest. *Civic pride* taps a relatively abstract sense of the political system's performance. Given the rather vague way this indicator refers to particular institutional functions, it is likely that individuals' reports only partly reflect their observations of government and are partly the product of long-standing orientations toward politics and society, a hypothesis supported by its relatively weaker connection with the *government performance* factor and by the significant path from *interpersonal assurance*.

As with the individual and social components of the "personal" sources of legitimacy orientations, the two components of their "political" antecedents, *representational procedures* and *governmental performance*, are correlated. But again, the fact that they share just over a third of their variance (r = .61) leaves a good deal of empirical support for the hypothesis that linkage institutions and policymaking performance are distinct objects of evaluation. In sum, the revised model of legitimacy orientations appears preferable to the conventional formulation on theoretical grounds, for its more complete and explicit specification of both macro and micro views of the concept's key dimensions, as well as on empirical grounds, where both the plausibility of its estimated parameters and its consistently better overall fit support its preferability.

The new model has implications both for understanding the interplay between the public and the government and for developing better measures of legitimacy and its cognates. By explicitly importing macro theories of legitimacy into the conceptualization that guides data collection at the individual level, the revised model implies a significant change in the researcher's expectations about the sophistication of ordinary citizens' evaluations of governmental performance. The conventional model directs greatest attention to individuals' reports of their personal feelings about their own participation or about their general favorability toward politicians and policy outputs. The revised model purposely constructs indicators to tap how the public's judgments go beyond such general and personalized ratings to evaluate government's procedures and performances against more impersonal normative standards. By stepping back from the systems theory image of government as a black box exchanging favorable policy outputs for the public's supportive inputs, the revised model presupposes that individuals can, and often do, go well beyond general approval or disapproval in their evaluations of political objects—that they distinguish not only between incumbents and structures but among procedures and policies, fairness, efficiency, and venality. Political theories of legitimacy rest on such subtle distinctions, but they often implicitly assume that expert outside observers are uniquely qualified to draw them. The empirical analysis presented here suggests that citizens, acting as "naive moral philosophers" (Tyler 1984), may be capable of some of the same distinctions.

Finally, several implications follow for the choice of measurement strategy and research design in studies of legitimacy and political system stability. Future research can build on this work in several directions. The model formulated here should be estimated on U.S. data collected at other times, and its applicability assessed outside the national context. But we clearly have only begun to fill out the recipe for closing the gap between theory and operationalization in survey studies of legitimacy. Researchers will need to concentrate on developing measures that tap the new indexes at least as accurately as traditional questionnaire items tapped the conventional indexes. Some promising lines of current research include studies of citizens' responses to their encounters with the bureaucracy, legislators, and the courts (see Jacobson 1987, chap. 5; Katz, Kahn, and Gutek 1975) and studies of citizens' conceptions of social and political justice (Alves and Rossi 1978; Jasso 1980; Jennings 1991; Rainwater 1974; Rasinsky 1989).

Public opinion research on political legitimacy has languished as measurement developments have proceeded independently of the macro context of theory and comparison from which legitimacy concerns gain their significance. The gap between theory and measurement will not be closed until we can estimate models containing good measures of both micro and macro variables. By proposing a new measurement model that more closely aligns survey measures with system properties, I suggest an initial step toward that goal.

APPENDIX A: NES (1976) ITEMS MEASURING LEGITIMACY ORIENTATIONS, CONVENTIONAL INDICES

Numbers in parentheses are factor loadings (lambdas) from the confirmatory covariance structure analysis of the final version of the conventional model.

Political Efficacy

No say. People like me don't have any say about what the government does. (1.0)

Voting. Voting is the only way that people like me can have any say about how the government runs things. (.727)

Complex. Sometimes politics and government seem

so complicated that a person like me can't really understand what's going on. (.505)

Don't care. I don't think public officials care much what people like me think. (.963)

Political Cynicism or Trust

Waste. Do you think that people in government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it? (.539)

Trust. How much of the time do you think you can trust the government in Washington to do what is right—just about all of the time, most of the time, or only some of the time? (.637)

Big interests. Would you say that the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit for all people? (1.0)

Incompetent. Do you feel that almost all the people running the government are smart people who usually know what they are doing or do you think that quite a few don't seem to know what they are doing? (.613)

Crooks. Do you think that quite a few of the people running the government are a little crooked, not very many are, or do you think hardly any of them are crooked at all? (.659)

Trust none. This scale combines responses from three items asking, Which part of the government on this list do you most often trust to do what's right . . . Congress, Supreme Court, President, political parties? Which do you trust next most often? Which do you least often trust? Scoring arrays institutions in terms of their link to "the electoral connection," with low scores indicating greatest trust in the Supreme Court, then the president, Congress, and the political parties. (.591)

Government Responsiveness

Attention. Over the years, how much attention do you feel the government pays to what people think when it decides what to do: good deal, some, or not much? (.994)

Parties. How much do you feel that political parties help to make the government pay attention to what the people think: a good deal, some, or not much? (.960)

Elections. And how much do you feel that having elections makes the government pay attention to what the people think: a good deal, some, or not much? (1.0)

Congressmen. How much attention do you think most congressmen pay to the people who elect them when they decide what to do in Congress: a good deal, some, or not much? (.942)

Personal Efficacy

Expected. When you do make plans ahead, do you usually get to carry out things the way you expect, or do things usually come up to make you change your plans? (.981)

Work out. Have you usually felt pretty sure your life would work out the way you want it to, or have there been times when you haven't been sure about it? (1.0)

Plan life. Do you think it's better to plan your life a good way ahead, or would you say that life is too much a matter of luck to plan ahead very far? (.844)

Run life. Some people feel they can run their lives pretty much the way they want to; others feel the problems of life are sometimes too big for them. Which one are you most like? (.947)

Satisfying life. In general, how satisfying do you find the way you're spending your life these days? Would you call it completely satisfying, pretty satisfying, or not very satisfying? (.494)

Personal Trust

Personal trust. Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people? (.906)

Helpful. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves? (1.0)

Advantage. Do you think that most people would try to take advantage of you if they got the chance, or would they try to be fair? (.866)

Belief in Political Ideals

Represented. How much do you feel that you and your friends are well represented in our political system? (.981)

Fair courts. How much of the time do you think that the courts in the United States guarantee everyone a fair trial? (1.02)

Good politicians. To what extent would you say that the leading politicians in the United States have had good intentions? (1.0)

Blame for corruption. There has been a good deal of talk these days about Watergate, sex scandals, and corruption in the government in Washington. Do you think that these problems are just the result of what individual politicians have done, or is it because there's something more seriously wrong with government in general and the way it operates? (.607)

Political Interest

Follow politics. Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs most of the time, some of the time, only now and then, or hardly at all? (.575)

Campaign. Some people don't pay much attention to the political campaigns. How about you? Would you say that you have been very much interested, somewhat interested, or not much interested in following the political campaigns so far this year? (1.0)

Care who wins. Generally speaking, would you say that you personally care a good deal which party wins the (presidential/congressional) election this fall or that you don't care very much which party wins? (.454)

Political Efficacy (External)

Lose touch. Generally speaking, those we elect to Congress in Washington lose touch with the people pretty quickly. (.805)

Not opinions. Parties are only interested in people's votes but not in their opinions. (1.0)

Citizen Duty

Can't win. It isn't so important to vote when you know your party doesn't have any chance to win. (.685)

Not important. So many other people vote in the national elections that it doesn't matter much to me whether I vote or not. (1.0)

No care. If a person doesn't care how an election comes out, he shouldn't vote in it. (.601)

System Evaluation

Proud. Would you say . . . "I am proud of many things about our form of government" or "I can't find much in our government to be proud of." (1.0)

Need change. Some people believe a change in our whole form of government is needed to solve the problems facing our country, while others feel no real change is necessary. Do you think a big change is needed in our form of government, or should it be kept pretty much as it is? (1.0)

APPENDIX B: NES (1976) ITEMS MEASURING LEGITIMACY ORIENTATIONS, NEW INDICES

Numbers in parentheses are factor loadings (lambdas) from the confirmatory covariance structure analysis of the final version of the revised model.

Political Interest

Care who wins (.416); *campaign* (1.0); *follow politics* (.857); *complex* (.570).

Citizen Duty

Can't win (.631); *not important* (1.0); *no care* (.560).

Personal Efficacy

Plan life (.839); *expected* (1.005); *work out* (1.0); *run life* (.952); *satisfying life* (.506).

Interpersonal Trust

Personal trust (.910); *helpful* (1.0); *advantage* (.872).

Civic Pride

Need change (.481); *proud* (1.0).

Subjective Political Competence

No say (1.0); *voting* (1.085); *complex* (.476).

Accountability Mechanisms/Parties and Elections

Parties (.926); *elections* (1.0); *trust none* (.371); *no say* (.788); *not opinions* (1.217).

Officials' Attentiveness to Constituents

Attention (.581); *congressmen* (.557); *don't care* (1.0); *lose touch* (.732).

Competence/Efficiency of Officials

Waste (.511); *incompetent* (.890); *crooks* (1.0).

Fairness of the Political Process

Trust (.690); *big interests* (1.0); *blame for corruption* (.657); *represented* (.727); *fair courts* (.746); *good politicians* (.726).

Notes

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1. The normative force exerted by the critique in terms of legitimacy is owed in part to the fact that the legitimacy of a regime is somehow "above" or "apart from" transient public attitudes. The evidence typically offered for the statement that public opinion is neither necessary nor sufficient for legitimacy is the fact that from time to time, regimes consensually considered legitimate in the eyes of outside observers are overthrown in an excess of revolutionary fervor, while others that observers would agree are illegitimate persist, relying on force or enculturation.

2. For instance, Almond's adaptation of Parsons and Shils's model focused attention on individual values, self-image, and social relations (Almond 1956; Almond and Verba 1963), while Easton's (1957, 1965) elaboration of the systems theory metaphor emphasized subjective responses to policies. The distinction between "input alienation" and "output alienation" (Gamson 1968, 42) is roughly coterminous with this one, and it stands as one of the relatively few strongly validated propositions in the literature (Aberbach 1969; Abravenel and Busch 1975; Converse 1972; Fraser 1970; Paige 1971; House and Mason 1975; McPherson, Welch, and Clark 1977).

3. In 1952, scales for political efficacy and citizen duty were introduced (Campbell, Gurin, and Miller 1954); and in 1958 an index of "basic evaluative orientations toward the national government" was developed (Stokes 1962, 64). In 1958 and 1964 additional items were added measuring personal efficacy

and interpersonal trust (Rosenberg 1956) and the responsiveness of political institutions. Mason, House, and Martin (1985) review the literature on political alienation.

4. For instance, since the early 1980s, the Board of Overseers for the National Election Studies has curtailed data collection on a growing fraction of the long-standing items measuring citizens' political trust, efficacy, perception of government responsiveness, civic pride, etc. But the mood of these decisions has not been one of accomplishment at successfully completing a research agenda but, rather, frustration at the poor theoretical potential of the conventional conceptualization of legitimacy phenomena and the measurement strategy following therefrom.

5. E.g., Abramson and Aldrich 1982; Abramson and Finifter 1981; Citrin 1974; Citrin and Green 1986; Easton 1975; Erber and Lau 1990; Feldman 1983; Green 1985; Hill 1982; Miller 1974a, 1974b; Muller and Jukam 1977; Sniderman 1981; Sniderman et al. 1975; Weatherford 1984, 1987; Williams 1985.

6. From this perspective, the new items included in the 1987 pilot study constitute a variation on the same theme, for the most part presenting subtle shadings on a familiar battery of questions centered on the policies–incumbents distinction.

7. The Berkeley Social Indicators Project appears to be an example of such a local study piloting several promising measurement ideas. (Sniderman 1981 analyses a portion of the original study.) Some have been adopted, but others appear to have been rejected without much theoretical discussion.

8. It is worth noting that many of the most useful indicators of this dimension have not achieved the status of regular inclusion in NES surveys beyond the early 1980s. This dearth is not as serious as it would be if the NES were the only locus of survey innovation, since researchers in several other centers have carried forward work on related concepts (particularly public conceptions of justice—see Jennings 1991; Rasinski 1987; Tyler 1984). The measurement development work of these and other researchers can be usefully implemented to improve studies of legitimacy.

9. For instance, Almond and Verba (1963), Hyman (1959), Jennings and Niemi (1974), and Sears, Hensler, and Speer (1979) emphasize the influence of socialization and stable personal traits over a relatively long time horizon, while Hirschman (1970), Lyons and Lowery (1989) and Orbell and Uno (1972) emphasize short-run evaluations.

10. Perhaps the most common form of "specification abuse" (Wheaton 1988) is to fit correlated errors on purely statistical, rather than theoretical, grounds. Most reasonable covariance structure models can be fit very snugly to the original data if the researcher is sufficiently generous about freeing correlations between error terms. But correlated errors can mask alternative substantive specifications, hence they should be justified on theoretical grounds. I estimate each of the two models with no correlated errors, freeing covariances between errors in a separate step and explaining the rationale by reference to an explicit hypothesis about sources of measurement error.

11. Model B is *nested* in model A if B contains restrictions on parameters that are free in A but is otherwise identical. Sequential versions of the revised model and of the conventional model are nested, but the revised is not nested in the conventional, or vice versa. Key contributions to the large technical literature on comparing latent variable models include Bentler and Bonnett 1980; Hoelter 1983; Matsueda and Bielby 1986; and Sobel and Bohrnstedt 1985. Wheaton (1988) and Bentler (1990; see also Bentler and Chou 1988) provide useful brief reviews; and Bollen's text gathers several different lines of development into a unified presentation (1989, esp. chaps. 6 and 7).

12. Bollen's modification of the Bentler–Bonnett delta diminishes the coefficient's dependence on sample size and on differences in degrees of freedom between successive versions of the model (Bollen 1989, 271–2).

13. Additional support might be inferred from the ratio χ^2/df, although the value of this index (barely below its more liberal threshold at 5) and its relatively weak statistical rationale suggest that this evidence be treated circumspectly. Mason, House, and Martin (1985; cf. Parker 1986) analyse the orthogonal factors model in detail, finding it inappropriate on several counts.

14. This is consistent with the findings of Mason, House, and Martin (1985).

15. The free paths, chosen for their theoretical plausibility, are from personal and political efficacy to external efficacy, from citizen duty to civic pride, and from personal trust to political trust. Only four free structural paths are estimated in order to maintain comparability with the revised model, where the same number are estimated (since the overall fit of covariance structure models can typically be improved simply by freeing more parameters).

16. While measurement artifact appears the most likely explanation of this improvement in fit, this respecification does not test uniquely for that. Even if the context of measurement inflated inter-item correlations for the conventional indexes, this would not necessarily produce better fit in a complex model estimated by full-information methods. Nor does a significant error covariance necessarily indict measurement artifact of the sort Sears and Lau (1983) find. An error correlation indicates that two measures share variance not accounted for by the latent variables specified in the model. Measurement artifact is a prime source of such shared variance; but it could be due to any other unmeasured and unspecified construct.

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RESEARCH NOTE

MEASURING POLITICAL VIOLENCE AND LAND INEQUALITY IN CENTRAL AMERICA

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The value of cross-national quantitative studies of the relationship between mass political violence and land inequality is challenged along three lines. First, gross and systematic errors in the political violence data of the World Handbook of Political and Social Indicators *(the usual data source for empirical studies) render them worthless for Central America at least and probably much of the Third World as well. Second, conceptualizations of land inequality have been too simplistic to be of much theoretical value. Third, the temporal nature of this relationship has been inadequately considered. Responding to such deficiencies, I elaborate a broader understanding of land inequality and provide a fuller discussion of the temporal nature of its relationship to political violence. Throughout, the five nations of Central America are utilized for appropriate case material.*

Land is the most important resource in rural societies, and access to sufficient land is invariably the fundamental desire of most peasants. Accordingly, it has long been thought that the maldistribution of land is the key determinant of rural unrest, particularly unrest in its most dramatic forms, rebellion and revolution. Land maldistribution can have different manifestations. The most common aspects cited are landholding patterns so concentrated that either (1) many peasants are left with *minifundios* too small for family support (the *minifundización hypothesis*) or (2) that many peasants own no land at all (the *landlessness hypothesis*). In the most current literature the first position is argued by Midlarsky (1988) and the second by Prosterman and Riedinger (1987).

However, Muller and Seligson claim that this conventional wisdom is wrong (1987, 433–34). Their cross-national longitudinal data analysis of 85 states finds both that landlessness is irrelevant as an explanation for political violence and that land inequality (as measured by the Gini index) has only weak predictive value. Instead, they discover a stronger relationship for mass political violence with national income inequality (again measured by the Gini index).[1] They conclude, therefore, that agrarian inequality is relevant to mass political violence "only to the extent that it is associated with inequality in the nationwide distribution of income" (p. 443).

This controversy has important policy ramifications, as the various authors point out. Land redistribution (and associated support programs) is often promoted as a prophylactic against rural disorder and instability.[2] Muller and Seligson's sophisticated analysis appears compelling, thereby questioning the adequacy and wisdom of land redistribution. However, in reality, their study is flawed because of the meaningless data used to measure their dependent variable, mass political violence. The same flaw also negates Midlarsky's analysis, as in fact it does most empirical data analyses on this subject.[3] Their shared deficiency is their reliance on the grossly inaccurate data set provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). In addition, I shall also argue that previous studies are inadequate in their conceptualization of the independent variable. Land inequality cannot be adequately captured in a single dimension no matter how sophisticated the measure but must be understood instead as multidimensional in nature.[4] It is my argument, then, that the empirical relationship between land inequality and political violence remains undetermined.

MEASURING MASS POLITICAL VIOLENCE

> February [1977] An army base is set up near San Juan Cotzal [Guatemala]. Since 1976, killings have included 68 cooperative leaders in the Ixcán, 40 community leaders in Chajul, 28 in Cotzal, and 32 in Nebaj. In one community, the presidents of Catholic Action, Caritas and the local cooperative and community development committee, as well as five sacristans and four bilingual teachers, have been killed. (Davis and Hodson 1982, 47)
>
> Guatemalan deaths from political violence, 1976–77, according to the *World Handbook*: 0.

The most complete and most widely used cross-national longitudinal data set on political protest and violence is provided by the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). A prodigious undertaking, providing annual events data for 1948–77, it is not good enough. The data are mortally flawed for Central America and (I suspect) for the rest of Latin America and the Third World more generally.

The primary source for the data set is the *New York Times Index*, supplemented for Latin America by *Keesing's Contemporary Archives*. Taylor and Jodice point out (p. 12) that *Keesing's* draws from nine basic sources (all European, with the exception of the *New York Times*), supplemented, for Latin America, with

six additional sources—again, not located in Latin America itself.

Taylor and Jodice give some consideration to the accuracy of their data set, but this attention is minimal.[5] They do mention the alternative of utilizing national newspapers, which would, of course, be inordinately expensive (1983, 11, 181). Their defense rests largely on a study by Jackman and Boyd (1979) of the costs and benefits of using multiple sources to measure political conflict. Jackman and Boyd's sample is 30 African countries during the 1960–67 period.[6] Jackman and Boyd conclude that their "results provide good support of [the 1972 *Handbook*'s] claim that increasing coverage beyond two sources is not likely to result in substantial changes in the 'overall picture'" (p. 457).

Well, yes and no. It depends on whether *overall picture* refers to the accurate measurement of what happened in African (or Central American) country X or to the score obtained from using additional sources of the type already used for these data sets. Jackman and Boyd's "multiple sources" for Africa are (in addition to the primary source of the *New York Times Index*), *Keesing's*, *Facts on File*, *Africa Diary*, and *African Recorder*. The latter two are claimed as "regional sources," but both are published in New Dehli. None of their "multiple sources" are African.[7] It is not surprising, therefore, that Jackman and Boyd discover "that the cost–benefit ratio from using multiple sources may be unnecessarily high" (1979, 456).[8]

Obviously, cost factors cannot be ignored; it is certain that to incorporate truly regional—if not national—sources into the *Handbook* would present tremendous, and perhaps insurmountable, difficulties. However, the validity of the data set rests not on financial considerations but on accuracy. In response to criticisms of "imprecision" in the 1972 edition of this data set (as well as in others), Gurr has argued that "the kinds of inferences drawn in quantitative macropolitics are not likely to be affected by anything less than gross and systematic error" (1974, 250). And that is precisely the charge. If valid, this claim has serious implications. As Lichbach has noted, the *World Handbook*'s indicator has been settled upon by scholars as the common dependent variable in cross-national statistical studies of political conflict (1989, 451).

> On November 23, 1974, National Guard troops with tanks and bazookas attacked a group of peasants in La Cayetana, San Vicente, [El Salvador,] killing six and arresting twenty-six (later thirteen were found to have disappeared).[9] These peasants had occupied sixty *manzanas* of idle land belonging to an absentee owner, after repeated attempts to rent the land had failed. . . . By some accounts, the military met . . . and decided that to allow the land occupation would be setting a dangerous precedent, and so they decided to attack the area. (Berryman 1984, 110)
>
> *New York Times* and *Keesing's*: event not reported.

The *Handbook* grossly understates Central American deaths from political violence. For example, standard estimates for deaths and disappearances from political violence in Guatemala for 1966–68 are 3 thousand to 8 thousand, with another 15 thousand for 1970–75 (Brockett 1988, 106–7); the *Handbook* gives only 207 deaths for the overall period.[10] Similarly, for Nicaragua, it has been estimated that over a thousand peasants were killed during the 33 months of martial law beginning at the end of 1974 (Brockett 1988, 168); the *Handbook* gives only 59 deaths for the encompassing five-year period. Although the death toll in El Salvador was far less for the period under examination than in the first two countries, those acquainted with Salvadoran events will agree that the 22 deaths reported by the *Handbook* for 1973–77 substantially underreports the tragic reality.[11] Even allowing for wild exaggerations in the "standard" estimates for these countries, the *Handbook* is clearly in gross error.[12]

Probably the most striking anomaly in the data set is that the *Handbook* reports political deaths in Honduras during the mid-1970s as twice as numerous as in either El Salvador or Guatemala! In reality, Honduras would rank fourth with Guatemala far ahead for all five. The unavoidable conclusion, then, is not just that the *Handbook* scores seriously underreport various countries but, more seriously, that a more accurate reporting would alter both the rankings between countries and the magnitude of the intervals between country scores, thereby substantially altering quantitative analyses utilizing this data set.

There are undoubtedly systematic factors at work producing the gross errors in the data reported by the *Handbook*'s sources. Political violence directed at peasants in remote sections of countries without permanently stationed *New York Times* correspondents is much less likely to be reported than urban violence in countries with permanent correspondents. Simply said, remote rural disappearances are harder to tabulate than assassinations in city streets. Other factors are at work, as well. The *Handbook*'s Honduran data for 1975 are surprisingly complete.[13] This was probably a coincidence, however, resulting from a systematic bias. The Honduran "bananagate" scandal of 1975 reached right into Wall Street, generating unparalleled coverage of Honduran affairs in the U.S. press.[14] As a result, the six people killed in June of that year at a peasant training center received mention by the *New York Times*, although the six peasants killed in 1972 in La Tlanquera were missed, their deaths having preceded the scandal.

Finally, there can be discrepancies between the *New York Times Index* and the news story itself:

> While a worldwide television audience saw El Salvador's sunny beaches before the "Miss Universe" finals July 19 [1975], off-camera heavily armed troops were called out to halt demonstrations by students protesting the Government's expenditure of $4-million on the contest. . . .
>
> Then on July 30 about 3,000 students demonstrating in San Salvador against repression of . . . earlier marches were stopped by machine-gun and automatic-rifle fire from soldiers.
>
> According to the military Government, which con-

TABLE 1

Land Inequality and Disruption Potential

MEASURES OF LAND INEQUALITY	COSTA RICA	EL SALVADOR	GUATEMALA	HONDURAS	NICARAGUA
1. Gini index of land concentration	.81	.81	.82	.78	.80
2. Smallholders[a] as % of all operators	43	74	87	64	51
3. Average size of smallholder farm (hectares)	1.7	1.2	1.8	1.9	2.6
4. Minifundización score[b]	25	62	48	34	20
5. % of all land in largest farms[c]	54	64	69	56	54
6. Landless score	20	36	32	32	27
7. Inequality score[d]	99	162	149	122	101
8. % rural population	59	62	66	72	51
9. Relative rural disruption potential[e]	59	100	99	87	52

Sources:
Row 1: Muller and Seligson 1987, Table A-1 for early 1970s except Guatemala (1964) and Nicaragua (1963).
Rows 2–3: Muller, Seligson, and Fu 1989. Table 2 for early 1970s except Guatemala (1964) and Nicaragua (1963); substituting data for Guatemala 1979 and Nicaragua 1971 made little difference.
Row 5: Brockett 1988, Table 4.1; also United Nations, Food and Agriculture Organization 1981, Table 3.3; for further discussion, see my n. 20.
Row 6: Prosterman and Riedinger's data for early 1970s (as reported by Muller and Seligson 1987, Table A-1) does not include Honduras or Nicaragua; Prosterman and Riedinger's later efforts (1987, Table 2) are for the early 1980s (except for El Salvador and Nicaragua, which are for the period just prior to their reforms). The data used here are an estimate for the mid-1970s, using the midpoint of the two data sets (and the mid-point for the 1987 data set where a range was given). Mid-1970 scores for Honduras and Nicaragua were estimated by reducing the score for the later date by the same amount as the average of El Salvador and Guatemala (Costa Rica is less comparable).
Row 8: United Nations 1976, 171–72 for early 1970s.
Notes:
[a] Smallholders are those with five hectares or less.
[b] Row 2 divided by row 3.
[c] Amount of total land held by largest 3.5–4% of farms in early 1970s (1979 for Guatemala). Cut-off sizes in hectares: Costa Rica, 200; El Salvador, 20; Guatemala, 22.4/44.8; Honduras, 50; Nicaragua, 345/690.
[d] Sum of rows 4–6.
[e] Row 7 times row 8.

tended that the march was part of a "Communist plot," *one person was killed,* five wounded and 11 arrested. But according to the students, *at least 12 persons were killed,* 20 wounded and 40 arrested. Witnesses said that about 50 persons, some apparently dead and others bleeding, had been taken away in army ambulances and trucks.[15]

The index lists only the one death claimed by the Government in the July 30 shootings, ignoring the higher number reported by witnesses.

Nonetheless, if the Central American data for 1948–77 were to be corrected, the political violence scores of Honduras and El Salvador would probably group together when placed alongside the much higher score of Guatemala. This raises at least two major theoretical questions: (1) What is the relationship, if any, between the political violence rates of the past and the future? (the *Handbook*'s 30-year data set ends in 1977—just when violence in El Salvador escalated to the tragic level of Guatemala)[16] and (2) What do these country groupings mean for the purported relationship between land inequality and mass political violence? Was the agrarian structure of El Salvador more similar to that of Honduras than to Guatemala prior to 1977? Did El Salvador's rural inequality then change, precipitating the violence that has now persisted for more than a decade?

MEASURING LAND INEQUALITY

Land inequality is often just measured by the Gini index,[17] which is so simplistic and misleading that it is of limited utility for meaningful cross-national comparisons. Indeed, as Table 1 indicates, the Gini land inequality scores for the Central American countries are virtually identical. For such reasons, Midlarsky (1988, 1989) argues, instead, for a measure of "patterned inequality," by which he means a comparison of the pattern of concentration of land holdings between smallholders and largeholders. In turn, however, Muller, Seligson, and Fu convincingly demonstrate that Midlarsky's measure is "interesting conceptually but so flawed in construction as to be of little utility" (1989, 586).[18] They create instead an "index of bifurcated inequality." This measure is constructed from the proportion of small farms and from the average sizes of small farms compared to large farms.

Both sets of authors are attempting to measure what might be termed *minifundización,* that is, to devise a measure that captures the discontent of the smallholder without enough land to support a family yet confronts other farms that are of vast and "unjust" size. The Muller, Seligson, and Fu approach appears adequate for this purpose on the lower end

of the land distribution scale. However, it would seem that what is most relevant to the smallholder about large estates is not their average size but rather the extent to which they dominate the local land supply. Beyond a certain size, an "unjustly" huge farm is just that—a large farm unjustly dominating the land supply, be it four hundred or four thousand hectares in size. Furthermore, the subdivision of large farms within the elite (by inheritance, sale, or subterfuge to avoid land reform laws) has no effect on land inequality from the perspective of the smallholder but could have a substantial impact on the bifurcation measure if the subdivisions were sufficiently prevalent. Accordingly, the share of total agricultural land dominated by the largest farms would be a better indicator of the role of the upper end of the land distribution scale in creating land inequality and peasant discontent than is the average size of "large" farms.[19]

Unlike the Gini index, these measures indicate substantial differences between the Central American countries. As row 2 of the table shows, *minifundios* are much more prevalent in Guatemala and El Salvador—in fact, twice as prevalent in the former than in Costa Rica. Similarly, the countries vary in the average size of the smallholder's land. Row 4 reports a score created by combining these two measures, which demonstrates *minifundización* to be the worst in El Salvador, followed by Guatemala and then Honduras, with Costa Rica and then Nicaragua grouped at the bottom. At the top end of the land distribution scale, essentially the same rankings obtain: Guatemala is closely followed by El Salvador, with the greatest concentration of land in the largest estates, and Nicaragua and Costa Rica with the least.[20]

Land distribution data are important indicators of rural inequality but are insufficient by themselves because of the increasing prevalence of landlessness among peasants. In fact, the landless of Central America constitute about one-third of the total population (prior to the agrarian reforms of the 1980s), with the exception of Costa Rica. Prosterman and Riedinger (1987, 10–11, 25) define landlessness as the lack of "ownership or ownership-like rights" among "those who cultivate the land." The landless include tenants, since they lack secure use of the land and face landlord extractions, amounts over which they usually have little control. The figures reported in Table 1, however, mask significant differences in the situation of the landless. Tenants vary widely in the security of their tenancy and the income-generating potential of "their" land. It is probable, for example, that the higher landless score of Honduras is less a source of intense discontent than the lower score of Guatemala, where the seasonal migration of Indians from the highlands to work on plantations was the largest migratory-labor-force-as-a-percentage-of-total-population in the world during the 1970s—and under extraordinarily oppressive circumstances (Paige 1975, 361).

These various indicators can be combined into an overall measure of land inequality. Row 7 reports such an inequality score, which combines, with equal weight, the three dimensions: *minifundización*, land concentration in the largest estates, and landlessness. By this measure, the worst land inequality in the early 1970s in Central America was found in El Salvador and then Guatemala, with a larger gap between them and Honduras, followed (by a large gap, again) with Nicaragua and Costa Rica. If land inequality translates into political action and conflict, it might be argued that the relative size of the rural population could be an important factor. Accordingly, row 9 takes this factor into consideration, giving a score for "the relative rural disruption potential" (with largely the same country rankings but a closing of the gap between the top three countries). Regardless of which score is used, the essential point is that a multidimensional measurement of land inequality demonstrates substantial differences between the five countries of Central America.

Devising adequate measures of land inequality, however, takes us only a short distance, since it is unlikely that there is any direct relationship between land inequality and political violence. A major weakness of statistical analyses of comparative political violence is that they are largely devoid of any theoretical link between macro-economic processes and structures and either individual or collective behavior.[21] For example, much of the case literature suggests that changes for the worse in economic status are more likely to lead to discontent and violence than would a constant misery.[22] It is conceivable, then, that countries and regions with high but stable levels of land inequality might be less prone to rural unrest than would others with lower levels of land inequality but where the economic security of the peasantry is markedly deteriorating.

BRINGING IN SOCIAL COMPLEXITY

To make the point more concretely, imagine two countries with similar land distribution patterns and landlessness rates. In country A, the agrarian system has remained fairly stable for decades; but high population growth rates have steadily driven up the levels of landlessness. Country B also experiences population growth; but, in addition, the commercialization of agriculture has rapidly transformed its agrarian system. Profit-motivated commercial farmers in country B use their superior resources (monetary and coercive) to dispossess peasants of lands to which they had long enjoyed ownershiplike use and to break long-term tenancy relationships. In both countries there will be mass poverty, but deprivation alone seldom leads directly to mass mobilization. In country B, which is the superior approximation of the Central American reality, discontent has a target.[23] It is not so much poverty alone, Prosterman and Riedinger point out, "as it is *blamable* poverty that seems to serve as a predictor of violence" (1987, 9). When the disadvantaged live in dangerous political environments, assertion of their demands is risky. To

overcome this discouraging environment, mass collective action usually requires "not only a sense of misery but also a sense of outrage and injustice" (Huntington and Nelson 1976, 102).[24] A good illustration of this point is provided by White's explanation of peasant mobilization in Honduras:

> The brutality of many of these evictions [of semisubsistence farmers by larger landholders] proved to be the catalyst in breaking down the friendly dependence on helpful patrons and developed a profoundly emotional opposition. The evictions were the sudden, sharp deprivation which moved campesinos to risk their lives in organizing to counter rural elites and protest before government authorities. . . . It was the small farmers who became the leaders in the national mobilization of campesinos. The evictions or other means of blocking access to land to which campesinos thought they had a legitimate right also became the basis for the land occupations or recuperations (1977, 181–82).

A final difficulty in assessing the relationship between land inequality and mass political mobilization and violence is the temporal dimension of this relationship. Do changes in the level of rural discontent resulting from growing inequality promptly cause corresponding changes in the level of mobilization and/or violence or might that effect be delayed? More concretely, high levels of peasant mobilization and resulting rural conflict were reached in Honduras in the late 1960s, about half a decade prior to similar mobilization and conflict in Guatemala, itself preceding those of El Salvador and then Nicaragua.[25] To what extent do these country rankings correlate with differences in levels of land inequality?

Although critical, the temporal dimension of the relationship between land inequality and political violence has received little theoretical attention. Midlarsky, for example, ignores the question except for a passing reference to the "generally slow" development of antiregime organizations (1986, 498–99).[26] If there is a long lag between the generation of inequality-based discontent and of social mobilization (and, therefore, political conflict and violence), then the correlation between inequality and violence is confounded (and many data analyses, as well).[27] If we postulate that there is a long lag, that is, that country C and country D have the same level of inequality, but that the inequality is of more recent origin in country D, then it follows that we would expect lower levels of violence in country D. However, why should we even hypothesize a temporal gap between the origins of discontent and of political mobilization and/or violence? And might whatever gap that does exist vary between countries because of the influence of other variables?

To return to the example of Central America and the development of high levels of peasant mobilization in Honduras prior to other countries—in actuality, the postwar agrarian transformation accelerated *last* in Honduras; that is to say, in Honduras there was little delay in time between the generation of discontent based on increasing land inequality and peasant mobilization and political conflict—certainly less of a gap than in the other countries. In contrast, El Salvador entered the postwar period with the most unequal agrarian system of the five; but its rural mobilization lagged behind that of Honduras, and its political violence rate was far below that of Guatemala until the end of the 1970s.

The countries of Central America, then, provide wide variations in the temporal relationship between land inequality and violence. The critical intervening variable that explains these variations is the political context. Largely ignored in earlier research, the critical conditioning role of political variables has fortunately received growing emphasis in recent studies.[28] Although too broad a subject to include here, the inclusion of the political context would explain the confusing relationships between land inequality and political violence found in the Central American cases, such as the very different relationship between the two variables for El Salvador and Honduras, as well as high enough levels of political violence in Nicaragua in the last third of the 1970s to place it with El Salvador and Guatemala, when its land inequality score was more like that of Costa Rica.[29]

CONCLUSION

This research note has discussed a number of hypotheses concerning the relationship between land inequality and mass political violence. Empirical social science teaches well the importance of rigorous hypothesis testing with the appropriate evidence. Regrettably, it sometimes unintentionally reminds us of the necessity of ensuring the validity of the empirical data utilized for such testing. If the *World Handbook*'s data set provided reasonably accurate measurements of the cross-national levels of mass political violence, it would allow for the meaningful testing of the relationships I have examined. However, the *World Handbook* cannot be used for this purpose. The data for at least Central America are grossly inaccurate. Since the data set is not valid for this region, the burden of proof falls on future users to demonstrate that it is sufficiently accurate for the regions of their study. The appropriateness of this data set for cross-national studies that wish to generalize globally, however, is another matter. Since at least part of the *World Handbook*'s data is the product of gross and systematic error, valid cross-national statistical tests of global hypotheses and theoretical models concerning political violence must await the construction of better data sets.

The compilation of the *World Handbook*'s data set on political protest and violence was an extraordinary undertaking and accomplishment; but by the standard of accuracy, it was an unjustifiable shortcut for Central America and (probably) for the rest of the Third World. The construction of reasonably accurate data sets for these countries will probably have to be done on the basis of a substantial division of labor, with small regions researched by different scholars. Instructive for these future efforts would be Tarrow's

(1989) use of national newspapers for his study of Italy and Tilly, Tilly, and Tilly's (1975) use of truly multiple sources in their comparative study of France, Italy, and Germany. Of course, as we move from institutionalized democracies to most Third World countries, censorship becomes a major obstacle to the development of accurate data sets.[30] A notable attempt to address this problem for a Central American country through the use of both national and international sources is the chronology of political violence in the Guatemalan highlands for 1976–82 compiled by Davis and Hodson (1982). They utilized five Guatemalan and four Mexican newspapers; one Costa Rican newspaper; North American newspapers and magazines; and reports from human rights, governmental, and religious organizations.

Future scholarship also must take social complexity into account better than heretofore. First, land inequality is multidimensional, involving, at a minimum, landlessness, insufficient land, and land concentration. Second, a more general concept of *rural inequality* would add to these dimensions other factors, such as income inequality and the security of tenancy and paid labor arrangements. Third, subjective experience is critical to mobilization. Similar levels of inequality can correlate with various levels of political mobilization and violence because of differences in the perception of the sources of that inequality. The available case material suggests that the most explosive situations arise when peasants believe they have been "unjustly" dispossessed of land. Finally, there is no direct relationship between rural discontent based on the perception of unjust inequality with levels of peasant mobilization, political conflict, and political violence. Instead, this relationship is mediated by the always-changing political context.

Notes

An earlier version of this study was presented at the fifteenth international congress of the Latin American Studies Association, Miami, 1989. I thank Mitchell Seligson for his helpful comments.

1. Midlarsky (1988, 503–4) appropriately objects that the Gini index is an invalid measure of what I have called here the *minifundización* hypothesis, of which his "patterned inequality" hypothesis is an example. Additional objections to the Gini index are provided by Prosterman and Riedinger (1987, 24–25). Midlarsky's debate with Muller and Seligson continued in more recent publications.

2. The underlying arguments vary from the inherent value of stability, to reform as preemptive of more radical movements and changes, to stability as the by-product of doing what is just (i.e., reallocating to peasants what they deserve).

3. These two studies are among the best of those that have examined the relationship between economic inequality and political conflict. For critical reviews of this literature, both highlighting the disappointing lack of theoretical progress, see Zimmerman 1980 and, almost a decade later, Lichbach 1989.

4. Although the focus here will remain on land inequality, a fuller account of the sources of rural violence would include other dimensions of rural inequality, such as income. There is, of course, a thriving literature on the socioeconomic determinants of rural collective action, among them McClintock 1984, Migdal 1984, Paige 1975, Popkin 1979, Scott 1976, Tutino 1986, and Wolf 1969.

5. Earlier editions of the *Handbook* were more tentative about the data set's accuracy (Russett et al. 1964, 97–98; Taylor and Hudson 1972, 64–65, 417–423).

6. Jackman (1978) uses this data for a quantitative analysis of the determinants of African coups.

7. The great preponderance of the *African Recorder*'s sources are Indian and Western European (particularly British). A quick check of the first four issues of 1962 did find some reports from Africa-based newspapers, all from Nairobi (except one from Dar es Salaam). The first four issues of 1988 were even more European-based. In a sample of six scattered 1961 issues of the *Africa Diary* close to half of the sources were African, but 61% of these were from two newspapers, one in Accra, the other in Nairobi. In addition, with the exception of Ghana and Kenya, coverage ran heavily to official travels and government announcements. For both sources for this analysis, Northern Africa and South Africa were ignored.

8. An additional deficiency of Jackman and Boyd's study is that they test only whether source differences affect hypothesis testing, but, examination at this gross level of aggregation could mask wide variations in country scores between sources. Precisely this problem had been demonstrated earlier by Doran, Pendley, and Antunes (1973); but Jackman and Boyd do not address this aspect of the earlier study. The Jackman and Boyd study neither compares its five sources for variations in their annual scores for a particular country nor, more importantly, does it compare differences between these five sources and other, more authoritative, sources. For example, while the relevant *Handbook* score mirrors the Burundi massacre of 1972, its report of 106 deaths in Burundi for 1963–67 obviously misses the massacre of some 2,500 to 5,000 Hutu in 1965 (Kuper 1981, 63).

9. The names of the murdered and of 10 of the disappeared can be found in the congressional testimony of Fabio Castillo, former president of the National University of El Salvador (U.S. House, Committee on International Relations 1976, 41).

10. A small story reporting the Amnesty International charge of some twenty thousand deaths and disappearances in Guatemala during the prior decade appeared in the *New York Times* on 12 December 1976. Although it is obvious that this kind of data could not be included in the events data set, it is curious that it did not lead to a reexamination of the validity of the Guatemalan data compiled for the *Handbook*.

11. To his credit, Midlarsky (1988) realizes that there are serious problems with the Central American data. His "solution," however, is inadequate. He uses the entire 1948–77 data set, dividing the Latin American countries into quintiles. However, because "El Salvador and Nicaragua . ... are known to have experienced intense violence in this period," they are artificially moved into the top quintile (El Salvador all the way from the bottom). Guatemala, where the most deaths actually occurred, is left in the third quintile (when it belongs in the first); nor are the scores of any of the other countries reconsidered. In response to the excellent critique by Muller, Seligson, and Fu (1989) of his handling of the dependent variable, Midlarsky (1989, 589) refers to the "arguments developed" in note 7 of his 1988 article. These "arguments" however, consist solely of a quick reference to LaFeber's *Inevitable Revolutions* (1983). The cited page in LaFeber makes no reference to El Salvador at all; its discussion of Nicaragua is in terms of the tens of thousands of deaths in the war against Somoza, almost all of which occurred after 1977, that is, after the end date of the data set.

12. If the data are erroneous for one region, similar problems would presumably be found elsewhere as well. Just a quick glance at the annual country scores shows the following examples of underreporting of political deaths: Chile 1973, 537 deaths; Brazil 1968–72, 36; and Uruguay 1973–77, 14.

13. The data for 1976, however, are another matter. The *Handbook* reports 15 Honduran deaths for 1976. The only relevant *New York Times* story (24 July, 1976) lists 14 deaths—but of Salvadoran soldiers killed in a border conflict with Honduran armed forces. The *Keesing's* edition of 30 April,

1976 (p. 27708) does list 15 peasants killed in Honduras; however, the deaths are reported as having occurred on 8 November 1975 (and are already included in the *Handbook* total for 1975). In fact, the process by which *Keesing's* was utilized by the compilers of the *Handbook*'s scores is a mystery. For example, the *Handbook* reports 22 deaths for Guatemala in 1973; the *New York Times*, 17; so *Keesing's* (6–12 August, 1973, p. 26025) total of 22 must have been utilized. Yet in the following year *Keesing's* (18–24 March, 1974, p. 26419) mentions 8 Guatemalan deaths but the *Handbook* only lists 1. Similarly, the *Handbook*'s total for El Salvador during 1974–75 is only 3 political killings but *Keesing's* (10 September, 1976, p. 27938) lists 26 killings just for November 1974–September 1975.

14. The *New York Times'* first 1975 story on Honduras appeared on 10 April with the U.S. Security and Exchange Commission's charge that United Brands (the successor to United Fruit) had paid a $1.25-million bribe to the Honduran president to obtain favorable tax treatment on its banana exports. In April and May, the *Times* ran some 24 stories related to Honduras, all involving the scandal and its aftermath. The rest of the year had 17 more stories, including several concerning the escalating rural unrest in the country. During the previous three years the *Times* had run only nine stories on Honduras, none of which involved the considerable unrest in the countryside. Meanwhile in 1975, the rest of Central America was largely ignored in the *Times*. For El Salvador there were but three stories and for Guatemala only one. Costa Rica did have nine articles; but five were scandals related to the United States, and the remainder were short pieces. Similarly, four of Nicaragua's seven stories ran in January following the audacious Christmas party attack by the Sandinistas; and two of the remainder concerned the new U.S. ambassador.

15. "Unrest Growing in El Salvador" *New York Times*, 10 August 1975 (emphasis added). Other sources give a higher death toll—for example, Berryman at least 20 dead (1984, 111); Montgomery at least 37 dead and "several dozen" more disappeared (1982, 89), Webre at least 37 dead and "many more" disappeared (1979, 189).

16. One presumes that it was this juxtaposition that led Midlarsky (1988) to arbitrarily alter El Salvador's place in his ranking of the Latin American countries on his political violence scale.

17. Muller and Seligson, for example, use the Gini index for measuring inequality in the distribution of land, which is then combined with the proportion of the labor force employed in the agricultural sector to yield an "agrarian inequality" score (1987, 435).

18. Indeed, this statement can be aptly applied to the whole study. Most of its weaknesses are identified by Muller, Seligson, and Fu (1989). Although still critical, Lichbach sees more merit in Midlarsky's work, praising it as "the most sophisticated and relevant formal models of the [economic inequality–political conflict] nexus" (1989, 452).

19. Another advantage is that this approach avoids the impossible task of deciding what constitutes a "large" farm. For a discussion of these difficulties, see Muller, Seligson, and Fu 1989, 580–81.

20. The decision of how to operationalize "the largest farms" was an artifact of the data sources. The available land distribution data are reported by size categories, whose number varies between countries but usually around 10–12. The procedure was to find breaks between categories that yielded approximately the same cutoff point for "the largest farms" for each country (which turned out to be between the largest 3½–4% of all farms). This could be done for three of the countries, but the percentage of land held by the largest farms had to be extrapolated for two (Guatemala and Nicaragua). As note c to Table 1 indicates, the cutoff point in hectares varied substantially between countries—another manifestation of the large difference between them in the extent of land concentration.

21. Lichbach discusses this deficiency insightfully (1989, 448–55).

22. Eckstein is a recent example of the argument that "economic relationships, especially changing economic relationships [are] the principal cause of protest and pressure for change" (1989, 4; also 16–17).

23. Midlarsky accounts for *minifundización* almost entirely in terms of population growth (1988, 494–95). He gives virtually no consideration to the other factors discussed for country B, which scholars have identified as crucial to accounting for the political conflict in the region (Brockett 1988; Williams 1986).

24. Similarly, see Gurr 1970, 155–231; Jenkins 1985, 5–6; McAdam 1982, 34; Moore 1978, 459–71; and Scott 1977, 236.

25. These generalizations are not based on a quantitative ranking but, rather, a familiarity with the relevant literature (see Brockett 1988). This ranking disregards state terrorism directed at largely unorganized peasants as part of counter-insurgency campaigns (i.e., Guatemala in the mid-1960s and Nicaragua in the mid-1970s).

26. This hypothesis has no bearing on Midlarsky's empirical test, which completely overlooks the temporal relationship of his variables, thereby confusing the causal connection between them. His dependent variable is the total number of deaths from political violence for 1948–77 (as compiled by the *Political Handbook*). His independent variable is constructed from the earliest data available on land concentration patterns. For Costa Rica, the date of the independent variable data comes after virtually all of the political killings; for Honduras and Nicaragua, it is approximately simultaneous with the date of the midpoint of the killings; while for El Salvador and Guatemala, the date of the independent variable data precedes most of the killings. What, then, is the theoretical relationship between the two variables? Muller and Seligson (1987) are better; their dependent variable is for 1973–77, and they try to get their independent variable data from as close to 1970 as possible.

27. In a commentary on an earlier study by Muller, Weede notes the absence in the literature of any "theoretical defense of lagging or not lagging" (1986, 438). Also see the more general, and still relevant, theoretical critique by Gurr and Duvall (1973, 136).

28. In addition to Muller and Seligson 1987, among the best examples are McClintock 1984 and Midlarsky and Roberts 1985.

29. For an explanation of these differences, see Brockett 1991.

30. Illustrative of the problem is Lindenberg's discovery concerning social discontent coverage in Central American newspapers: "Precisely at the moments when historical sources indicate that there was the most discontent, evidence of the volume of that discontent disappears in countries whose newspapers were censored." (1990, 405).

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CONTROVERSY

POLITICAL OBLIGATION, FREEDOM AND FEMINISM

How should feminist theorists approach the analysis of political obligation and freedom? In her article in the December 1989 issue of this Review, *Nancy Hirschmann proposed a fundamental reconstruction of these concepts. Carole Pateman agrees that liberal obligation theory inadequately treats the coercion of women and other groups under the guise of voluntary obligations. But she argues that Hirschmann goes too far, particularly in rejecting the priority of freedom. Hirschmann explicates their disagreements and her approach.*

Sinopoli and Hirschmann's (1991) controversy in the *Review* unfortunately fails to confront Hirschmann's (1989) major claim that the idea of voluntary, or self-assumed, obligation and the priority thus given to freedom is a serious problem for feminists. She concludes that obligation must be reformulated in terms of involuntary, nonconsensual, or "given" social relations and put into a context of connection and responsibility.

I agree with Hirschmann about the inadequacy of contemporary theories of political obligation and their patriarchal character and endorse her call for a new approach and new questions, but I want to take issue with her discussion of political obligation and her conclusion. I will not examine object relations theory, although I do not believe that this provides an appropriate new approach (see e.g. Young 1984) because, as I shall indicate, the case can be made that standard arguments about political obligation are patriarchal from a consideration of classic and contemporary arguments.

OBLIGATION

Hirschmann argues that critics (including myself) of what she calls "consent theory" have failed to look deeply enough at the well-known problems with the theory; the symptoms, not the causes have been addressed. The critics take it for granted that obligation is self-assumed, and they have not asked why abstract individualism is so central to consent theory.

A major problem in Hirschmann's discussion is that she assimilates *obligation* to *consent*. She states, for example, that I argue for a "fully consistent consent theory" (1989, 1228) when I took considerable pains to distinguish my argument about self-assumed obligation from arguments about consent (Pateman 1985). I may not have succeeded but Hirschmann does not show this. The idea of self-assumed obligation is important because to enter an obligation—to commit oneself—is to create a new social relationship. The idea is also very radical, because it depends on the assumption that individuals are free and equal to each other. The only way, therefore, that this assumption can be maintained and legitimate government created is through free agreement, or the voluntary creation of such an arrangement. The idea of self-assumed obligation thus turned the justification of modern government, whether in the state, the plantation, or the workplace or of men over women, into a general and permanent problem.

In contrast, consent must be given *to* something. It is precisely "consent theory" that treats obligation as already established or given. Indeed, at one point Hirschmann writes of "the necessity of consent to obligation" (1989, 1234). In the case of theories about original contracts, consent is (said to be) given by the next generation of citizens to the political obligation created through the original agreement. In contemporary theory, as in classic contract theory, the problem is taken to be to show how consent is given or, why there are reasons for obedience, rather than to show how political obligation is created and recreated or whether it exists. The prevailing assumptions by political theorists are that political obligation exists and consent is always given, thus, a variety of hypotheses are advanced to explain how this takes place; that is, self-assumed obligation is turned into what I called hypothetical voluntarism. The *problem* about political obligation is conjured away (see Pateman 1985). Ironically, Hirschmann presents as a feminist alternative the "given" obligation that is central to almost all contemporary consent theory.

Hirschmann might argue that even hypothetical voluntarism is still voluntarism, and that is the problem. Hypothetical voluntarism still is an example of the "fear and refusal" of consent theory to which she refers in her reply to Sinopoli, to admit "nonconsensual obligations." She argues that consent theory has segregated nonconsensual obligations in the private sphere, assigned them to various groups, "women, workers, slaves", and then denied those groups a political voice (Sinopoli and Hirschmann 1991, 228). I am not clear whether Hirschmann is arguing here that consent theory ignores these groups or that they actually have nonconsensual obligations. Such groups have been incorporated into voluntarist "consent theory." Men become "workers" by entering into, or consenting to, the employment contract, women become "wives" by entering into the marriage contract, and it has even been argued by some classic contract theorists and defenders of slavery in the Old South that "slaves" are constituted through a contract. To be sure, socialists and feminists have argued that the marriage and employment contracts are not valid contracts—that workers and wives, like slaves, are coerced. Such an argument implies that

consequently there are no "obligations", merely enforced obedience. However, the status of Hirschmann's nonconsensual obligations is not clear. She mentions "unfair bargaining positions," for example (1989, 1239), but what does this imply for her major claim that obligations are given and nonconsensual? Are they valid? Her answer in the case of mothers' given obligation to care for their children (the focus of her argument) is equivocal (p. 1240). But this avoids the question whether "obligation" is the appropriate term, in this case, or in the case of workers and slaves, or whether other terms should be used.

Hirschmann notes that she does not discuss "duty" (ibid., 1243); but she does not mention "ought" or "obedience" either. "Nonconsensual obligation" could refer, depending on the context, to a variety of social relations. For instance, such "obligations" might be an example of enforced obedience. Or "duty" might be the relevant concept: we do not choose our duties, they come with our involuntary stations in life. Again, "nonconsensual obligation" could refer to actions that we ought to perform or from which we should forbear or point to the fact that there are others, at home or far away, for whom we should care. We may be forced to do something, or morally we ought to do it—but we do not have an *obligation* to do so unless we have made an explicit commitment. In colloquial speech "obligation" is often used broadly to cover "ought" and "duty", but that is no reason for political theorists to fail to distinguish them. To assume an obligation takes an individual beyond a personal moral judgment (I ought to do [not do] X) to a public commitment to action (forbearance) and into a new relationship and responsibility.

ABSTRACT INDIVIDUALISM

Hirschmann's argument presupposes that the concept of self-assumed obligation depends on abstract individualism. On the contrary, abstract individualism prevents an adequate understanding of obligation and, in its most radical form (as Hobbes demonstrated), makes obligation impossible. Individuals seen in abstraction from each other and their social relations can necessarily make judgments only from a subjective, self-interested perspective, and obligation must be seen as a series of discrete acts. Thus, abstract individualism generates an endemic problem about entering and keeping commitments, best known in the formulation of the "problem" of promising.

To assume an obligation is to commit oneself (promise). But why should promises be kept? From an abstractly individualist viewpoint there is no good reason at all; self-interest will dictate that any promise might be broken. From an alternative perspective that might be called social individuality, there is no general problem about obligation (which is not to say that no questions arise about assuming and fulfilling obligations). Self-assumed obligation is a social practice and individuals can create obligations only because any single promise presupposes that obligations bind. To say "I promise" or the equivalent would be meaningless if there were not an intersubjective understanding that to say these words is to commit oneself (see Pateman 1985, ch. 2).[1] The practice of self-assumed obligation therefore entails the social relations, connection, responsibility, and, I would add trust, stressed by Hirschmann.

This argument does not answer Hirschmann's question why abstract individualism is important in theories of political obligation. Instead, it illustrates the kind of problems generated by widespread adherence to this standpoint. However, like Sinopoli (Sinopoli and Hirschmann 1991, 222), I am not clear exactly what kind of question Hirschmann is asking—whether about cause or meaning. She charges that critics of consent theory have not investigated the cause of abstract individualism. Her own argument about mother-only childrearing, and masculine "reactive autonomy" appears to be causal. Hirschmann claims that reactive autonomy "justifies—indeed, creates and perpetuates—the radical and abstract individualism of liberal democratic theory, the market model of society, a theory that obligation can exist only by virtue of voluntary assumption" (1989, 1237).

One difficulty with this approach is that some great critics of abstract individualism—namely, Rousseau, Hegel, and Marx—either explicitly excluded women from citizenship or failed to break free from patriarchal theory.[2] More importantly, if Hirschmann wishes to give an account of patriarchy as "constituted by the cultural, social, political, sexual, economic and interpersonal structures of the modern liberal era" (Sinopoli and Hirschmann 1991, 226), then I find it hard to see why a causal question about abstract individualism is appropriate or why mothers' caring for their children should bear the weight of explanation.

Indeed, in her reply to Sinopoli Hirschmann appears to draw back from causal argument, treating object relations theory as interpretive (ibid.,) and stating that she is "build[ing] an account" that understands "a world of meaning that allows only certain ideas and views to emerge" (p. 229). Thus, she seems to be asking how "the individual" is interpreted, or what meaning is given to the "individual" and the "self" in the modern liberal era. An elucidation of this meaning(s) does not require a discussion of mothering. To imagine that individuals could be seen as if they were separate from their relations and institutions was a brillant feat of theoretical innovation. This development occurred as the "old world" of tradition and natural, ascribed hierarchy was giving way to the "new world" of universalism, equality, and freedom, the world of the market and constitutional government. For the first time "individuals" appeared, outside of any network of duties and obedience; to be a "free individual" meant that the old constraints were swept away.

Theorists were faced with the new, general problem about power and government generated by the

premise of individual freedom and equality. If the problem was to be circumvented and individuals were to (be said to) consent to modern government—or to make the point another way—if new constraints were to be instituted, the advantages of (modern) government had to be made clear. Here, of course, theorists used the device of the state of nature in which its inhabitants were pictured as abstracted, to a greater or lesser degree, from the bonds of social life (from the "old world") and subject to insecurity or even war. Hence, a "new world" was needed. Reason then dictated that they should enter into the original contract and continue, as subsequent generations should also continue, to give consent. Abstract individualism remains influential, I would suggest, because it enables theorists to avoid asking critical questions about social structures and to assume that political obligation is "given." All that remains is to show how consent can be said to take place.

Stories about original contracts describe the creation of a new, free political order, or civil society, in which individuals become citizens. The "individuals" in question, as Hirschmann stresses, are masculine. Sexual difference, as constructed by the classic contract theorists, is a political difference—the difference between freedom and subordination. Their picture of manhood and womanhood is a political construct, albeit that they use the language of nature. Only men, therefore, can create the new body politic and ties of political obligation. They establish the modern, "free" patriarchal order. (For details of the preceding argument, see Pateman 1988). The classic contract theorists had reason to attack the divine right of kings, despotism in the state, and claims by patriarchal theorists that kings and fathers were one and the same (the "old world"). They had no wish, however, to overthrow men's jurisdiction over women; rather, they had a reason to modernize patriarchy because they had vital interests at stake *as men*.

Does this mean that women are, therefore, excluded from the new political order? Hirschmann's answer appears to be that they are, but the implications of her argument for women's citizenship are unclear.

CITIZENSHIP

The conventional subject matter under the heading *political obligation* is the relation between citizens and state. Hirschmann (1989) discusses some contemporary theorists of political obligation, and subsequently states, "My focus is political obligation" (Sinopoli and Hirschmann 1991, 228). Her argument about women, though, is not about women's obligation as citizens but women's obligation in the family, especially as mothers. Feminists may well argue that this is a political question, or that women's public standing as citizens cannot be understood without taking account of their private status, but Hirschmann does not spell out how the two are connected. She remarks, "the point of my original article [was to] highlight women's systematic exclusion from political obligation theory" (p. 229; see also p. 226), but what is the connection between theory, women's duties as mothers, and their citizenship in practice? She also comments that she is not "suggesting that we model political obligations on the relationship between mother and child" (1989, 1242), but this still leaves open the question of how motherhood; citizenship; and women's political obligation, duty, or obedience are connected.

An emphasis on exclusion obscures the fact that the political *inclusion* of women is at issue too. Classic contract theory is about women's inclusion into the political order as men's subordinates, as well as their exclusion from citizenship. Exclusion from citizenship did not mean that women were excluded as political subjects or that they had no political duties, even though women's inclusion is not discussed by political theorists. Contemporary theorists assume that their arguments can apply to women or men (many of them now carefully add "she" even where completely inappropriate), but they do not ask whether men's and women's political incorporation has taken the same form or, now that women have won virtual equality in formal rights with men, whether the meaning of citizenship is the same for both sexes.

For instance, theorists of political obligation never look to see if there has been a vehicle for women's political inclusion that corresponds to that of "soldier" or "worker" for men. In some recent work, I have begun to explore "motherhood" as a political status and as a means of political incorporation from arguments about Republican motherhood in the United States to arguments and policies about population decline, eugenics and abortion and controversies about the welfare state (see Pateman 1992). Political theorists claim that the ultimate duty of citizens is to be willing, if the state so demands, to risk or give up their lives as soldiers. They do not stop to consider the significance that until very recently only men have been called as soldiers, or to consider whether an analagous duty to risk their lives has been demanded of women—in bearing babies to provide the population and future citizens of the state. "Motherhood", in this sense, does not mean relations between mothers and children; rather, it refers to the political tasks, service, and contribution of women that can be compared to the political contribution of men as workers or soldiers. The paradox is that motherhood has been constructed as quintessentially private, so that women's political duty of motherhood has been seen as opposed to citizenship. This construction has been challenged by feminists, many of whom have demanded, at least since the 1790s, that motherhood, as a service to the state, should be part of citizenship.

Hirschmann states that the "contingent gender bias of voluntarist obligation"—that is, cultural bias and practices that deny women opportunities for consent, and exclude women from standard means of consent (1989, 1229)—treats women as "incapable of consent." Even if a capacity for consent is granted,

women are excluded because their experiences are denied (p. 1240). Certainly, women's experiences are denied political significance by political theorists; the problem is that this denial takes place in a context where women are included in (not excluded from) the "standard" practices of consent, whether these are part of marriage, sexual relations, voting, or other activities typically held to indicate consent.[3]

FREEDOM

Hirschmann argues that women's exclusion derives from the priority given to negative liberty or reactive autonomy in obligation theory. A feminist theory, therefore, would not give freedom priority (1989, 1241). Such a conclusion depends on the identification of freedom with absolute, negative freedom and a view of positive liberty as coercive (p. 1234).[4] Hirschmann writes, for example, that consent (i.e., obligation) "rests on a concept of liberty as absence of restraint" (ibid.). This argument entails that any bond created when an individual assumes an obligation is a restraint on freedom, but, as I have argued, this is to misunderstand obligation—and its connection to freedom.

Sinopoli criticises Hirschmann for an overly atomistic reading of Locke (Sinopoli and Hirschmann 1991, 224), but the criticism of her reading of the classic texts can be taken further. She writes of "the absolute natural freedom of the state of nature outlined by the social contract theorists" (1989, 1234), but this is true only of Hobbes. All the other classic contract theorists picture the natural condition as one of bonds and relations between the inhabitants, including conjugal and familial ties. Hirschmann also claims that since freedom is the "hallmark of humanity," women are not seen as human; that if they were, they would have a right to freedom (1989, 1236). I would not deny that doubts have been cast on women's human status by extreme misogynists and patriarchalists, but the classic contract theorists are hardly guilty of this. On the contrary, not only does Hobbes's portrayal of the natural condition include the striking presupposition that natural freedom and equality are the birthright of women as well as men—in the state of nature women are lords (see Pateman 1991)—but the other contract theorists have to admit women's freedom at the same time that they deny it.[5] In their states of nature, unlike Hobbes's, marriage exists and this "natural" relation is carried over into civil society. They all agree that marriage is based on a contract (this is true even of Hegel) and women are parties to this contract. Therefore, it follows that women share in freedom and the human status signified by "consent", notwithstanding that their "natural" subordination to men is institutionalized through the original contract (see Pateman 1988).

Hobbes showed that an abstract, absolute conception of freedom is no freedom at all. Self-assumed obligation presupposes a very different idea of freedom, a social conception in which freedom inheres in a certain form of sociopolitical relations (relations that include "oughts"). The idea of "obligation" enables individuals to do more than take these relations as given; it allows them both to reinforce interdependencies where appropriate and to create new bonds. Obligation is thus a means through which individuals and associations can exercise, maintain, and sometimes, depending on the character and content of the bonds, enhance their freedom.

Hirschmann states that "the purpose of a feminist approach to obligation is not to bind us more tightly to the state or to relations" (1989, 1242), but her argument about given "obligation" points in that direction. Liberal theory is not alone in giving priority to freedom; socialist theory has also shared that goal. Liberalism and socialism, however, have been patriarchal in theory and practice. Feminists need to do a good deal of theoretical reformulation, and I endeavored to contribute to that task in *The Sexual Contract*, where I looked at the construction of various dichotomies, notably public/private, that have been central to both the understanding of their subject matter by "political" theorists and the practice of contract involving property in the person. Feminists have no need to jetison the concept of obligation or, indeed, freedom, (there are a variety of nonnegative views available on which to draw), but theoretical work goes only so far; the structure of social and political institutions also has to be confronted. Contemporary theories of political obligation typically assume that a free political order exists, but this is not self-evidently the case.

Feminists would be ill advised to give up the priority of freedom, for no one else has given much heed to the problem of men's power and women's subordination. This is not to suggest that feminists should not be concerned with care, which Hirschmann sees as the feminist priority, but to draw attention to two questions. The first is whether the voice of care is always that of the powerless (see Sinopoli and Hirschmann 1991, 226). The powerful talk of *noblesse oblige*, and of the caring duties of "good masters" of all kinds, and are very accomplished at caring for each other. The second question is whether a political order in which all are cared for can be created if freedom for all is relinquished as a priority.

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I am grateful to Carole Pateman for providing this opportunity to expand on several complex points that had to be compressed in my original article (Hirschmann 1989) but that I take up in my book *Rethinking Obligation: A Feminist Method for Political Theory* (1992). My argument raised many controversial issues, not all of which could be addressed within the confines of a single article. Though I hope that my

new book addresses these issues satisfactorily, Pateman's commentary raises important ideas that merit discussion in these pages.

There are many similarities between Pateman's work and my own. Indeed, many of the arguments she makes in her critique are at least guiding assumptions, if not explicit aspects, of my own argument. We are both concerned with political obligation, the social contract, and state legitimacy as they relate to the historical, political, ethical, and philosophical position of women. We both believe that there is a severe disjunction in liberal obligation theory between the language of voluntarism and the reality of coercion for women and other groups. We both emphasize participatory democracy as an important feature of a theory of political obligation. Finally, we both see a problematic relationship between the individual and society in modern obligation theory.

But whereas Pateman seems to want to refine that relationship, I am interested in questioning our understanding of it altogether by considering the epistemological framework that determines the meaning of the terms involved. This leads (as Pateman indicates) to disagreements between us about the priority of freedom and the meaning of consent. But these, I believe, are by-products of our strongest disagreement—over how best to conduct feminist analyses of political theory. Whereas Pateman's critique of patriarchy seeks to alter the particular construction of gender relationships within a liberal epistemological framework, I seek to analyze the framework itself and to identify the bias that exists in the epistemological underpinnings that define the modern world. The problematic relationships between individual and community, freedom and authority, and male power and female subjugation are not to be addressed by reorganizing the pieces on the board. Rather, the board itself must be reassessed and reconfigured before any such reorganization will have the desired effect.

CHOICE, CONSENT, AND GIVEN OBLIGATION

Pateman presents my argument as follows: women have traditionally been considered naturally bound to care for husbands, children, and others. But rather than seeing this historical bondage as problematic, we need to accept it as a new feminist starting point for thinking about politics. In short, she believes, my feminist theory of obligation could be taken to glorify women's oppression and exploitation and could give rise to a sort of feminist totalitarianism where individual difference and choice are suppressed in the interests of care.

But Pateman's misconstrual is obvious. In seeking to construct an alternate vision of obligation based on a feminist standpoint of connection and mutual recognition, I refer to women's history of care not as a simplistic endorsement of women's lack of choice but to understand why it is that *women* have historically lacked choice while men have defined and exercised freedom in the modern era. Furthermore, in the process, I show that this dichotomy between free men and bonded women is a constitutive element of modern understandings of the terms *freedom* and *obligation*; they demand women's continued exclusion from the class of "free" political actors, by definition.

As I particularly stressed, political obligation on the social contract model is flawed not merely because it gives an important role to individual choice per se, but rather for its artificial and oversimplified reification of choice, as well as of human relationship (Sinopoli and Hirschmann 1991, esp. 230). This dichotomy then carries over into the rest of human social relations: men are naturally free, women naturally unfree. But in order for women to gain greater freedom and choice, men must recognize their share in these unchosen obligations; and we must all recognize the role of such "private" activities in constituting the public realm.

Liberal obligation theory *tacitly* admits the importance of the nonconsensual obligations represented by women's experience. But it publicly denigrates them; and that is one of the purposes of its particularly modern version of the public/private split. That they *seem* unimportant to liberal theory stems not from their inherent value but from their bifurcated representation. By taking all of the aspects of life that cannot be *honestly* described as contractual, assigning them to women (even under the name of "duties," as if that answered, rather than begged, the question of choice), and then overtly disvaluing them, liberal theory can ensure that these aspects are taken care of without having to recognize the contradictions they pose for the theory. This, then, leaves men *ostensibly* free in the public sphere to choose (allegedly) all of their connections, relationships, and obligations.

Pateman (1979, 1985) is rightly critical of consent theory for its disingenuous elitism. She is attracted to a Rousseauist participatory democracy because it provides for more genuine individual participation and choice, since citizens have a role in deciding the content of their obligations—i.e. what they will consent *to*. But within the existing epistemological framework, this focus on equal citizens freely choosing offers a consistent rendering of the liberal ideal.[6] A totally voluntarist theory of obligation is possible only because of the existence of a class of people who take care of the nonconsensual obligations that characterize human life. Women's subjugation has endured precisely because of theorists' refusal to incorporate the aspects of life typified in women's experience; it is because of the theoretical contradictions they pose that women must be relegated to the private sphere and then devalued.

Pateman's desire to create the conditions that can provide for better choice is in itself admirable. I, too, am interested in creating such conditions, for women's choicelessness in history is pernicious and presents a serious contradiction for modern (liberal) politics. But the argument cannot stop there; to focus *only* on choice leads one into the trap of taking a reified masculinist experience as the norm that

women should follow. Pateman does this, for instance, in calling the liberal abstraction of individuals from relationships and institutions "brilliant" (p. 180); for in reality, it was brilliant only for (white, bourgeois) men, since this notion of freedom and individualism vitally depended on the subjugation of large groups of other people, particularly women. That consent theory excludes women is not just happenstance (any more than it is conscious conspiracy) but a function of the epistemological structure of liberalism. It is only because liberalism has the public language of contract and can reify and separate noncontractual reality from the public discourse of political theory that the ideology is sustainable (Sinopoli and Hirschmann 1991, 228). And this emphasis on the public is possible only because of a repressed private realm, that is, a realm of noncontractual obligations assigned to women and then repressed or conveniently forgotten when liberal obligation theory is written. But when something is repressed, it is still present even though such presence is consciously denied.

This certainly does not mean that feminists need to glorify women's exploitation as caretakers or that we cannot critically assess and reject patriarchy. But even granting that feminists do not have obligations to the existing patriarchal context does not mean that women—any more than men—can break free of all contexts to drift in abstract freedom. Indeed, their greatest obligation may be to forge a new context—which in many ways is what feminism is all about.

Thus, my account of given obligation is hardly "the 'given' obligation that is central to almost all contemporary consent theory" (p. 179). The givenness of obligation in consent theory is covert, hidden, and even subverted to the end of an overt and explicit public ideology of choice and freedom.[7] My work suggests that theorists need to attend to the aspects of human existence that liberalism limits to the private sphere in order thereby to learn more about all kinds of obligation (including particularly political obligation), because "private" obligations constitute the public. The dichotomy between public and private obligation itself needs to be questioned. Pateman claims to challenge that dichotomy, but her remarks above actually endorse it. What could it mean to talk about "women's obligations as citizens" (which she opposes to "women's obligation in the family" [p. 181]) in the sixteenth century, for instance? Such political obligations, like women's private obligations, were of obedience only—moreover, of obedience *within the private sphere*, to husbands and fathers. Pateman oversimplifies the distinctions between obligation, duty, and obedience by ignoring how these terms are shuffled about and defined to suit patriarchal theoretical needs. To say that women were "included" in consent theory is thus misleading. It is more accurate to say that they are subsumed—and consumed—by it. Their agency is not only made invisible, but effectively disabled as well.

THE PRIORITY OF FREEDOM

Hence, at least *one* "connection between theory, women's duties as mothers, and their citizenship in practice" (as Pateman calls for, p. 181) is clear. Consent theorists do not want to explore the implications of "private" relationships for public ones, because this would require them to compromise the natural liberty they wish to claim for men. As I have argued twice already, theorists like Locke do not present as starkly negative a conception of liberty as Hobbes (Hirschmann 1989, 1233–34; Sinopoli and Hirschmann 1991, 228); yet the foundation of their consent theory is importantly negative. Pateman's claim that only Hobbes talks of "the absolute natural freedom of the state of nature" (p. 182) is far from true. Indeed, even though Rousseau embraces positive liberty in his ideal social contract, he is very similar to Hobbes in his description of the early stages of the state of nature, in that natural freedom is limited only by physical laws of nature (such as gravity) and eventually, as people come into more frequent contact with one another, by the relative strength of other individuals.

Locke himself casts the laws of nature as "limits" to natural freedom, which might suggest positive liberty (Hirschmann 1989, 1233–34). But while such laws are moral maxims (e.g., "Don't take more than you can use, because God did not give the earth for us to spoil"), they are also rational conclusions. For example, taking more than you can use wastes your labor (although once money is developed, it is rational to acquire more than you can use. Macpherson 1962). But for Locke, it is as wrong to talk about rationality "limiting" your freedom as it is to talk about gravity doing so: Why would anyone want to work for an entire day picking apples when half of them will spoil? God did not make men so imperfect. Or to the extent that he did and individual men violate or cannot access their God-given rationality, the social contract provides the remedy to make men do what they should want to do if they were rational enough. Hence, people who are not rational enough to see the need to consent to the social contract are said to do so tacitly. Locke's natural freedom is thus just as absolute as Hobbes's, even if the parameters of that freedom shift, and he binds people by as much of a "rational fiat" as does Hobbes (Hirschmann 1989, 1239). If the laws of nature were only moral maxims and Locke defined *freedom* positively, then why would we have to consent to government in the first place? Why would Locke not simply tell us what we should and should not do?[8]

Pateman's parallel claim that Hobbes is a sexual egalitarian is similarly problematic. While Hobbes argues that men and women are equal in the state of nature, women suddenly drop out of Hobbes's discourse in the formation of civil society. He does not even include them in his description of the family, let alone discuss their role as citizens (Hobbes 1968, 253). This contradiction reveals that his social contract is possible only if women become disempowered. In

order to make sense of the fact that only men participate in the social contract, Pateman argues that the patriarchal nuclear family must predate the social contract (1988, 49); but she also notes the difficulty of this position. If women's childrearing created a temporary strategic disadvantage that enabled men to subjugate them, it is more likely to assume that infants would be abandoned by women. Faced with the choice of keeping a child and being subservient to a man or abandoning the child and maintaining her ability to fight men on an equal basis, Hobbesian woman should choose the latter.

Indeed, it would have made much more sense for Hobbes to declare that women consent equally to the social contract and then have the sovereign create patriarchy by commanding women to obey their husbands. But while this would be more consistent with *natural* sexual equality, it would also, paradoxically, make the structural sexism of Hobbes's *social* contract more apparent. (For a fuller discussion, see Hirschmann 1992, chap. 1 but also Di Stefano 1983). In light of this, Pateman's observation that Hobbes was raised by women is rather curious, because it supports my argument. As Di Stefano (1991) argues, Hobbes's life resonates strongly with an object-relational reading of his theory. It is, at least in part, because of the power of the mother that natural women appear monstrous; and it is because of the need to "escape the female" that women must disappear from Hobbes's account of the social contract and become subordinate to men. The social contract and absolute authority of the sovereign represent the turn to the father in the safe passage he offers away from nature and the power of the mother to "nourish, or expose" the child (Hobbes 1968, 254; see also Flax 1983).

Hence, it is difficult to understand what Pateman can mean by saying that "the classic contract theorists are hardly guilty" of saying that women are less than human, something she reserves for "extreme misogynists and patriarchalists" (p. 182); for if women's powerlessness is a necessary prescription for men's activity in politics, and public and political activity are defined as the essentially human, then are women not prohibited from engaging in the activities that make them human? Are they not so prohibited on the basis that they are not, in fact, human? Women's humanity could not be completely obliterated, of course; if men are to attain recognition, women must retain enough humanity to give this recognition at least a minimal value.[9] But this is tacit at best; they are certainly less than *fully* human. Again, there is a discordance between the overt, conscious statements of liberal obligation theorists and the meaning that underlies those statements; while liberalism tacitly utilizes a notion of different degrees of humanity, it explicitly employs an either/or typology: humans participate, women do not.

Rousseau is only the most explicit example of this phenomenon. There is a superficial way in which Rousseau seems to recognize the humanity of women: Sophie's role in the home is claimed to be as essential to the successful working of the social contract as Emile's participation in the assembly. But the difference in their roles also reveals that Sophie is unilaterally Emile's inferior from a moral—not to mention a political—perspective, primarily because of women's animal sexuality (Rousseau 1979). Rousseau clearly saw that the domestic sphere was important; but in the process of its dichotomization from the public, women *are* dehumanized. Rousseau's reassurance that women are "separate but equal" belies the reality that draws him to this construction of public and private.

This logic underlies other social contract theorists as well, and accounts for the continued disempowerment and nonrecognition of women (see Hirschmann 1992, chaps. 1–2). Hobbes postulates sexual equality in the state of nature but then introduces a radical disjuncture in the effort to disempower women in the social contract. Similarly, Locke, while making certain gestures toward sexual equality (Sinopoli and Hirschmann 1991, 228), nevertheless requires women's obedience to husbands to make his theory work. In defining the public as the realm of the essentially human and the private as tacitly important but overtly unimportant, women are by definition less than human because they are less than men.

The issue of sexual equality is directly tied to the priority of freedom; for as long as freedom is prior to other sorts of concepts, women will continue to be considered men's inferiors. Politically, the centrality of freedom allows men to continue to shun their nonconsensual obligations in the name of freedom. It leaves intact a public ideology that somehow, women are less human than men because they cannot protect their freedom by asserting themselves, standing up for themselves, by breaking free of their ties. Instead, as Gilligan's studies argue, women's experience suggests a different reality (Gilligan 1982; Gilligan, Lyons, and Hanmer 1990). It can reveal a moral schema that gives priority to aspects of human life that liberalism either makes invisible or allows the powerful to repress when convenient.

Hence, I agree with Pateman that liberal theorists "have to admit women's freedom at the same time that they deny it" (p. 000). Our difference seems to be that she wants to focus on the admission, while I seek to understand the denial. I take my approach because in understanding the denial, we unearth the theories' contradictions so that we may attempt to resolve them through a new theory. Certainly, "a new story about freedom is urgently needed" (Pateman 1988, 232–33); and feminists should be engaged in its telling (Hirschmann 1990). But freedom, from a feminist standpoint, must undergo a radical shift; it must be reconceptualized as part of other concepts, such as obligation, equality, and justice. It entails the priority of connection and relationship, enabling us to see that the well-being of others may enhance one's own capacity to act. In the process of this telling, it becomes clear that freedom is decentered: theoretically, it cannot sustain the hegemonic centrality that it occupies in liberal theory and simultaneously work

interactively with these other concepts. This does not mean that freedom is not vital or should be jetisoned. Indeed, I clearly reject such a view (Hirschmann 1989, 1242; Sinopoli and Hirschmann 1991, 229–30). Rather, to paraphrase Pateman's closing sentence, the question is whether a political order in which all are *truly* free can be created if care is relegated to the shadowy corners of a devalued and segregated private sphere.

EPISTEMOLOGY

While I have argued that Pateman's earlier work extends liberalism even as it claims to challenge it, I have read her later work as agreeing with my arguments. In *The Sexual Contract*, she holds that "in modern civil society individual freedom is unconstrained" for men only because of the "mastery and obedience" of women and that accordingly, "[a] free order cannot be a contractual order" (1988, 232). She problematizes the meaning of *individual* and links this to patriarchal dualities: "When feminists forget that, though acceptance of the 'individual' may be politically necessary, so also is rejection, they acquiesce in the patriarchal construction of womanhood" (p. 14). And, of course, she has just mentioned "social individuality." In such passages, she seems to be working toward the transcendance of duality and toward the feminist perspective of connection and relationship that I develop through my feminist analysis of obligation.

But these apparent similarities mask a deeper division between us which Pateman may or may not recognize but certainly does not discuss, a division over the epistemological issues that lie at the heart of my argument. Neither Pateman nor Sinopoli have acknowledged my key claim that structural gender bias is structural because it is epistemological. This certainly resulted in Sinopoli's missing the point of my article, and I believe it affects Pateman's reading as well. Pateman holds that the approach of object relations is unnecessary because "the case can be made that standard arguments about political obligation are patriarchal from a consideration of classic and contemporary arguments" (p. 179).[10] By this, I assume she means that political obligation theory holds internal contradictions that do not make sense of women's exclusion from political power. I agree that such a case can be made. She herself makes that case admirably; and I engage in a similar analysis (Hirschmann 1992, chaps. 1–2). But that hardly precludes the epistemological argument. Indeed, it emphasizes the need for it, because it highlights the question, Why is it that such contradictions have been historically tolerated in theorists' efforts to exclude women from political power? How and why has liberalism sustained itself in the face of these contradictions?

Pateman says that "the classic contract theorists had reason to attack the divine right of kings. . . . they had no wish, however, to overthrow men's jurisdiction over women . . . because they had vital interests at stake *as men*" (p. 181). She thus suggests a conspiracy: men simply have not wanted to give up power. While this may be true in many ways, it does not go far enough; or perhaps it goes too far in attributing such motives to men. As I have emphasized in my two earlier accounts, men suffer from the reified and abstract presentation of obligation almost as much as women, though their position of power has allowed them to circumvent many of its disadvantages. My approach, in contrast, shows why men might be unwilling to give up such power; why such unwillingness might exist without consciousness of that power (or indeed, of that unwillingness); and thus also why it might endure even in the face of feminist arguments showing the hypocrasy and self-contradiction of liberalism. My approach also indicates how such power can translate into particular constructions of the political and how social relations can be structured to support that vision of politics.

Attention to such epistemological issues is what gets feminists beyond "Men are the problem, feminism the solution." By understanding the sexism of liberal epistemology, we can see that both men and women are caught in a particular construction of reality that is simultaneously a product and a constrictor of human action. If the entrenchment in liberal theory of the public/private dichotomy guarantees the obliteration of women's experience from our ordinary-language understanding of political obligation, and if gender bias relates to the very epistemological framework that defines the conceptual terminology in which theories operate, then any analysis of "the problem of women" within the boundaries of existing theories of obligation will only be able to go so far. Such analyses will have only limited depth by definition, because our conceptual vocabulary is itself biased.

A consideration of epistemology helps clarify the connections between choice, relationship, and obligation. Whatever women's history of coercion under patriarchy has been, contemporary women do have more choices than ever before. These choices present perplexing and often intractable dilemmas, but they mark women's ability to assume obligations as they see fit and to act as free agents. Indeed, these choices are in part due to liberalism—not the natural evolution of liberal theory itself but *feminist* efforts to push the logic of liberalism to its extreme conclusion (Hirschmann 1989, 1240).

Yet even if we say that "women" have more choices, the question as to *which* women really have those choices and what those choices *are* remains crucial. The feminist literature that informs my own argument suggests that to the degree that such choice is genuine, women's experience reveals that choice, though important, is not enough. It would seem that the original situation imposed on women by their nonconsensual obligations reflects particular aspects of *human* existence that should not be simply abandoned as relics of a patriarchal past but explored and examined as material for new feminist understandings of human relationship. Such examination is

necessary to change the patriarchal structures within which women are fated never to have as much choice as men.

This might seem circular; but it is so only from the standpoint of liberalism's dedication to the dichotomy between free men and subservient women. Taking obligation as a *starting point* is very different from taking it as an *end point;* and this is the significance of taking given obligation as an epistemological and methodological claim (see Hirschmann 1989, 1243). Treating given obligation as an end point results in the preservation of the oppressive status quo that Pateman rightly fears. But as a starting point, from the perspective of women's experience, nonconsensual aspects of human relationships are vital to our humanity. Rather than segregate them in the private sphere and consign them to nonpolitical female nonactors, feminism requires that men, as well as women, recognize and fulfill these given obligations. Only then can both women and men take part in greater and more genuine choice.

In the process, of course, the dichotomy between choice (freedom) and choicelessness (bondage) is transformed into a relationship between the two, since it is revealed that many aspects of choice are contextual and intimately tied up with things about which we have no choice. Feminists cannot end the dualities of "natural/civil, private/public, women/individual" that Pateman rightly locates at the heart of the patriarchy of liberal theory (1988, 232) without also challenging dualities between voluntaristic obligations and "natural" duties, choice and choicelessness, and freedom and obligation. By failing to recognize that this requires an alternate epistemology and by failing to note how this *must* include some idea of nonconsensual obligation, her solution tells only half the story—and it is uncomfortably like the patriarchal half. Indeed, many of her remarks here read as if she seeks only to fine-tune, rather than transcend, liberal paradigms.

Understanding the epistemological focus of my work helps reveal why consent theory is so problematic for feminists and how gendered experiences help produce particular perspectives that explain the representation of reality that we find in liberal theory. One can never reduce anything as complicated as patriarchy to a single cause, as I have indicated (Sinopoli and Hirschmann 1991, 226); but that does not mean gender psychology and standpoint epistemology are not useful in explaining and understanding the duration of patriarchal aspects of political thought. This concern with explanation does not make my argument reductively causal any more than it ignores questions of cause. I addressed the question of causation versus meaning in my reply to Sinopoli, and Pateman is not clear why my remarks do not satisfy her. However, Pateman's charge that I "draw back from causal argument" (p. 180) to bring in the issue of interpretation is severely misleading: How can I "draw back" from a position I never took in the first place? Indeed, I was critiquing precisely the sort of reductive and dichotomous treatment of cause that Pateman seems to endorse.

Feminism is certainly interested in challenging such dualistic approaches to cause and meaning (Harding 1986). Perhaps the problem is due to a failure to distinguish between reasons and causes. *Reasons* might better capture the multifacetedness of a feminist vision of explanation; for it allows for the synergistic, dynamic, and interactive qualities of human relationships without either abandoning the question Why does this happen? or enshrining it in some social-scientific charade. Or if the language of causation must be used, perhaps it should be considered a *contributing*, rather than an *efficient*, cause; if the *final* cause of a political theory lies in particular historical events, such as the exclusion crisis or civil war, the social relations of childrearing create the psychological and cultural environment that influences how theorists choose to respond to these political phenomena. To ignore such cultural aspects seems as misguided as to reduce childrearing relations to a simple, singular cause of everything we find in liberal theory.

Feminists do not have to endorse the exact same approaches in order to be effective; nor can we assume that any individual author has the final word on a given topic in feminist or political theory. I think it is important for nonfeminists in political science to understand this about feminism. So, it is particularly gratifying that two feminist political theorists are able to engage in such a debate within these pages. Certainly, Pateman and I can agree that the differences between us are less important than our common enterprise of exploring how the experiences of women can help us reformulate the way we conduct political theoretical inquiry.

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Notes

Hirschmann thanks Julie Mostov and Ian Shapiro for their comments and suggestions.

1. Rather oddly, Hirschmann gives an abstractly individualist account of promising. She writes, "The purpose of promising as an institution is to ensure that people will keep their word" (1989, 1239). But the practice of promising *is* institutionalized word keeping (can anyone except an abstract individualist ask about its purpose?). The attempt to ensure that people keep faith is made through a variety of means.
2. Sinopoli appears to present a similar objection (Sinopoli and Hirschmann 1991, 223); but he refers to preliberal theorists and so completely misses the point that general arguments about political life that rely on abstract individualism are specifically modern doctrines.
3. Hirschmann neglects these aspects of consent, which are related to the "obligation" that women are held to have to allow men sexual access to their bodies. It can also be noted that some women now become pregnant through artificial insemination to avoid relations with men, because of fertility problems, or to become "surrogate mothers." Another set of questions about consent and power are raised by these issues.

4. In an argument calling for an end to dualisms, her account of freedom depends on a very blunt dichotomy between negative and positive freedom. Hirschmann dismisses Rousseau from consideration on the grounds that since he has a "positive" view of liberty, he is not a liberal (1989, 1233), but it is not only liberals who are concerned with political obligation.

5. Hobbes was reared in a household full of women; but Hobbes is the only classic contract theorist (and one of the few political theorists) to start from the premise that women by nature were as free as, and equal to, men. This seems to cast doubt on Hirschmann's claims about mother-only childrearing.

6. Pateman (1979) insists on a distinction between consent theory and her own theory primarily because in the former we have no influence over the content of what we may or may not consent to, while in the latter such influence is paramount. But surely, this is only a distinction between "liberal democratic" consent and a more radical, egalitarian consent. Consent can be a matter of meaningful choice (e.g., my husband's and my negotiating a schedule for sharing child care), as well as empty and meaningless (an illegal alien's accepting below-subsistence wages). And while the former is clearly superior to the latter (which is exemplified by social contract theory), it nevertheless operates from similar assumptions about individuals. While I certainly support the goal of enlarging women's choices, the feminist literature I draw on suggests that social relationships will always contain some elements of nonconsensual obligations. As long as these are denied, the abstraction of individuals from social relationships persists. See Hirschmann 1992, chap. 2 for a fuller discussion of my views on Pateman's theory of obligation; see Mapel 1990 for a similar (though not feminist) critique of Pateman.

7. Similarly, the "caring" of noblesse oblige to which Pateman refers in closing hardly fits the account of caring provided by Gilligan (1982; see also Gilligan, Lyons, and Hanmer 1990), which requires mutuality and interaction among subjects, rather than the imposition of the privileged person's perspective of how care is to be provided for others.

8. Indeed, one could argue that in fact this is precisely what Locke does, particularly in *Questions Concerning the Law of Nature*, where he distinguishes between "reason" and "right reason." Locke might hold that God purposely makes some men irrationally "love the darkness" precisely so that all men will inevitably move to the social contract; this would then support Locke's implicit claim that his social contract is the only one that fits the divine order. But this even more strongly raises the question of why he felt compelled to hide this behind such a disingenuous account of consent. See Locke 1990, esp. quest. 1; cf. Hirschmann 1992, chap. 1.

9. While the Hegelian master/slave dialectic from which I develop the theme of recognition (Hirschmann 1989, 1236–37) indicates that the master (men) is dependent on at least a minimal human consciousness on the part of the slave (women) for recognition, this could in fact be carried further. For men *could* receive recognition from animals and objects, just as Hegel's slave receives it from nature. Women have certainly been considered animals, objects, and closer to nature by political theorists—thus negating their humanity altogether, not merely diminishing it.

10. This is a particularly curious claim, since Pateman herself uses Freudian theory as an interpretive tool for reading the sexual contract into liberal theory. Furthermore, I agree with Okin (1990, 664) that Pateman misreads Chodorow (1978), whose work would be more useful than Freud's to Pateman's argument.

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NOTES FROM THE MANAGING EDITOR

New Size and Familiar Contents for the APSR

You may have noticed that your March, 1992, *APSR* will no longer align comfortably on the shelf with its predecessors. Despite the disadvantages of discontinuity, the reasons for the change are compelling. The changes were initially proposed by the APSA staff and supported by the Publications Committee of the Association. The new 8 1/2" by 11" size saves substantially on paper costs, as trimming and wastage are no longer necessary. The savings will be used in part to contain general costs for the Association and in part to support an expansion of the *Review* by about 10%. We need the additional size to keep our article acceptance rate in the one-of-ten range and to accommodate the needs of the *Book Review*. (In the current issue, the additional space is being used for backlogged book reviews.) Moreover, the larger page size will make it easier to print readable figures, tables, and equations. Our shift to a new press should also enhance our technical capability. The larger size has required a few design changes, but for the most part we have tried to stay close to the excellent format developed by Pat Patterson and earlier managing editors.

Of course, the essence of the *APSR* is not its physical appearance, but its articles, essays, and reviews. In this regard the reader will find continuity rather than change. It is part of the mission of the *APSR* to be a major vehicle for outstanding scholarly research in political science in all of our subfields. I hope that perhaps we can also help those subfields to communicate with each other.

The determination of quality is made primarily through a peer review process. Almost all of the submitted manuscripts (currently pouring across our desks at a rate of about 50 a month) are assigned referees familiar with the manuscript's subfield. In each panel of reviewers we try to have at least one technical expert and at least one reviewer who can address the importance of the research from a broader perspective.

The selection of appropriate reviewers is obviously the key to selecting the best manuscripts. It is also essential to what Pat Patterson has called "the seminar by mail"—providing rejected manuscripts with high quality critiques and advice. We review manuscripts that not only focus on different subjects in the world of politics but draw on quite different paradigms of theory and method. It is difficult for any one scholar to have mastery of more than a fraction of our large and diverse discipline. I have tried to respond to the challenge of such diversity by checking our intuition about reviewers for each manuscript with at least one member of our new (and hard-working) Editorial Board.

The most important task of the Managing Editor of the *APSR* is to maintain the scholarly excellence of its contents. I also hope to recapture the remarkably rapid and efficient processing of manuscripts that Pat Patterson developed in his six years as Managing Editor. Even with the aid of his procedures manual, a hard-working staff and a willing Editorial Board, this is proving a formidable task. But whatever the stresses of adaptation, I trust that the *APSR* shall demonstrate that an increase in size does not imply a decrease in quality.

G. Bingham Powell, Jr.

FORTHCOMING IN JUNE, 1992

The following articles, research notes, and controversies have been tentatively scheduled for publication in the June, 1992 issue:

Jonathan Bendor and Thomas H. Hammond. "Rethinking Allison's Models."

Jeffrey Berejikian. "Revolutionary Collective Action and the Agent-Structure Problem."

Steven Forde. "Benjamin Franklin's *Autobiography* and the Education of America."

Tracey E. George and Lee Epstein. "On the Nature of Supreme Court Decision Making."

James L. Gibson. "The Political Consequences of Intolerance: Cultural Conformity and Political Freedom."

Arthur W. Lupia. "Agenda Control Versus the Power of Information."

Elinor Ostrom, James Walker, and Roy Gardner. "Covenants With and Without a Sword: Self-Enforcement Is Possible."

Yoav Peled. "Ethnic Democracy and the Legal Construction of Citizenship: Arab Citizens of the Jewish State."

Benjamin Radcliff. "The Welfare State, Turnout, and the Economy: A Comparative Analysis."

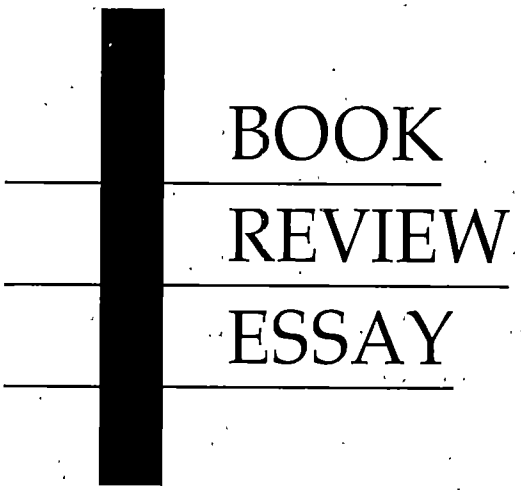
Laura L. Stoker. "Interests and Ethics in Politics."

Viktor Vanberg and Roger D. Congleton. "Rationality, Morality and Exit."

Robert Bates and Amy Farmer Curry. "Community Versus Market: A Note on Corporate Villages." A research note.

Reginald S. Sheehan, William Mischler and Donald R. Songer. "Ideology, Status and the Differential Success of Direct Parties before the Supreme Court." A research note.

Robert S. Erikson, Michael B. MacKuen, and James S. Stimson; Paul Abramson, and Charles Ostrom. "Macropartisanship." A controversy.


BOOK
REVIEW
ESSAY

COLLECTIVE WISDOM

JOHN C. WAHLKE *University of Arizona*

The Analysis of Political Structure. By David Easton. New York: Routledge, Chapman, and Hall, 1990. 336p. $37.50.

The Rhetoric of Reaction: Perversity, Futility, Jeopardy. By Albert O. Hirschman. Cambridge, MA: Harvard University Press, 1991. 197p. $17.95.

Inquiry and Change: The Troubled Attempt To Understand and Shape Society. By Charles E. Lindblom. New Haven: Yale University Press/New York: Russell Sage Foundation, 1990. 314p. $29.95.

American Citizenship: The Quest for Inclusion. By Judith N. Shklar. Cambridge: Harvard University Press, 1991. 120p. $17.95.

The Faces of Injustice. By Judith N. Shklar. New Haven: Yale University Press, 1990. 144p. $19.95.

Models of My Life. By Herbert A. Simon. New York: Basic Books, 1991. 415p. $26.95.

The authors of these diverse works are eminent in their own fields; but they are also senior professionals in the social science community, with experience in numerous government, university, and organizational capacities (including, in several cases, presidencies and other high offices), as well as in research and teaching. They write from a perspective enjoyed by few of their colleagues.

Recent years have witnessed increasing signs of malaise in and about the social sciences. The fact that all the authors were cognizant of, and concerned about—and some participated in—the disaffections of the 1960s and various earlier and later controversies suggested my question: Besides what each book tells us about its particular subject, what might we also learn—whatever our discipline or field of special interest—by viewing them all together in that light?

BRIEF SUMMARIES

Easton says that *The Analysis of Political Structure* is "a book about general theory" (p. ix); it is not itself a theory. It provides "identification, justification, and substantiation of the problem" (p. 282); that is, it tells us how to think theoretically about political structures. These are not visible as physical things, but are "phenomena [that] ought to be considered only as *properties of things*" (p. 30; emphasis original). Yet they are empirically identifiable phenomena and therefore measurable (in the generic sense). In my own words, structures exist in the *relationship* among things: it is the relationships that are empirically observable as properties of the behavior of people. What makes structure politically important is that it constitutes a *constraint* on people, "a limit on the variety of choices open to an individual or collectively" (p. 52).

Easton makes important distinctions (1) among *formal structures* (created by conscious human action, commonly described in constitutions and formal documents and *crescive* structures (developed over time without conscious intent); (2) between *lower-order* and *higher-order* (global) *structures*—directly observable in behavior and not directly observable (but also not abstract or metaphysical) respectively; and (3)—most crucial in Easton's conception—between *lower-level* and *higher-level* structures. His example of a higher-level structure is the political regime, which, although superficially of the same level as its four main explanatory variables (biological organism, political personality, political culture, and political behavior, each of which is also a structure), is, in his analysis, higher-level. He amplifies these distinctions and such related key concepts as institution, group, party, and so on, by differentiating "structuralism as a theory" from "structural theories."

Part 2 analyzes the work of the post–World War II structuralist Marxist, Nicos Poulantzas. Easton gives him credit mainly for being on the right track, in that "by implication, he addressed political life as a system of some sort" (p. 237) and thereby lent support to Easton's major point, as stated in the prologue: "If we are to understand the shape taken by regimes, we shall be forced to see them in the context of the structure of political systems viewed as wholes" (p. xiv). Following further conceptualization of higher-order structures and other concepts in his scheme, Easton explains the framework of his thinking, declaring himself to be a "pragmatic holist" but a "*theoretical* methodological individualist" (pp. 256–57; emphasis Easton's). In the concluding chapter he restates what his contribution has been and explains why, as he forewarned, he has not produced a theory of political structure:

> Our identification, justification, and substantiation of the problem provide the necessary if not inescapable first step. . . . It is one thing to recognize the possible existence of higher-order structures and their likely influence on lower-order ones and, thereby, on the disposition of political power in a political system and on control over outputs (public policies). It is quite another thing to identify a research technology that will permit us not only to describe this higher-order structure but to trace out the ways in which it facilitates, determines, or limits lower-order ones. (p. 282, 285)

That is to say, theorizing has outstripped research technology. The implications of that (if true) will be discussed, as will a related problem that might also interfere with the further development of theory, namely, the plight of the general theorist in perilous times, described by Easton in the final two pages of his book.

Hirschman's *Rhetoric of Reaction* analyzes the rhetoric used by reactionaries in political controversies in

three different periods spanning several centuries and representing three stages of the development of political rights: (1) civil liberties, (2) political rights, and (3) socioeconomic rights.[1] The authors whose works constitute his data concerning the first period, which culminates in the French Revolution and the Declaration of the Rights of Man and its echoes elsewhere, include Edmund Burke, Joseph de Maistre, Adam Muller, and Alexis de Tocqueville. Those exemplifying the second epoch, characterized by the rise of universal male suffrage and democracy, include George Canning, Robert Lowe, Sir Henry Maine, Fustel deCoulanges, Max Scheler, Gustave leBon, Herbert Spencer, Gaetano Mosca, Vilfredo Pareto, and James Fitzjames Stephen. For the third period, characterized by the rise of the welfare state, he cites Friedrich Hayek, Samuel Huntington, Opponents of the English Poor Laws and advocates of new Poor Laws, Jay W. Forrester, Nathan Glazer, Charles Murray, George Stigler, Martin Feldstein, and Gordon Tullock. By subjective content analysis of a nonrandom sample of polemical participants and commentators who reacted against these developments, he offers "a 'cool' examination of surface phenomena: discourse, arguments, rhetoric, historically and analytically considered" (p. x). Hirschman finds "a few common or typical arguments unfailingly made by each of the three reactive movements" (p. 7), which he describes and illustrates in a chapter each.

The *perversity thesis* "is, at first blush, a daring intellectual maneuver. The structure of the argument is admirably simple, whereas the claim being made is rather extreme. It is not just asserted that a movement or a policy will fall short of its goal or will occasion unexpected costs or negative side effects; rather, so goes the argument, *the attempt to push society in a certain direction will result in its moving all right, but in the opposite direction*" (p. 11; emphasis original). Hirschman believes that this argument "is unlikely to exist 'out there' to anything like the extent that is claimed" (p. 35). Examples of the use of this argument are offered from the writings of Burke, deMaistre, and Adam Muller for the first period, leBon and Herbert Spencer in the second, and Forrester, Blazer, Murray, along with polemecists in the Poor Law controversies for the second, and Stigler, Feldstein, and Tullock for the third.

The *futility thesis* argues "that the attempt at change is abortive, that in one way or another any alleged change is, was, or will be largely surface, facade, cosmetic, hence illusory, as the 'deep' structures of society remain wholly untouched" (p. 43). This argument amounts "to a *denial or downplaying of change* in the face of seemingly enormous epochal movements" (p. 70; emphasis original): "In its scenario, human actions or intentions are frustrated, not because they unleash a series of side effects, but because they pretend to change the unchangeable, because they ignore the basic structures of society" (p. 72). Among users of the futility thesis Hirschman includes de Tocqueville (first period), Mosca, Pareto, and Stephens (second period), and Stigler, Feldstein, and Tullock (third period).

The *jeopardy thesis* is "a more commonsensical and moderate way of arguing against a change which, because of the prevailing state of public opinion, one does not care to attack head-on: . . . it asserts that the proposed change, though perhaps desirable in itself, involves unacceptable costs or consequences of one sort or another" (p. 81). Users of this argument include Canning, Lowe, Maine, deCoulanges, and Scheler (in the second period), and Hayek and Huntington (in the third).

These three themes, Hirschman says, "account for the bulk of the rhetorical assaults I have undertaken to analyze" (p. 136). Logically, they reduce to two basic reasons for opposing a reform proposal: (1) it did not accomplish its purpose (perversity and futility) or (2) the costs incurred outweigh the benefits (jeopardy). Believing that 'Reactionaries' have no monopoly on simplistic, preemptory, and intransigent rhetoric [and that] their 'progressive' counterparts are likely to do just as well in this regard" (p. 149), he then turns the tables and shows that liberals and progressives use mirror images of the reactionary thesis. Instead of the incompatibility of reforms with other ends—for example, liberty versus democracy (jeopardy thesis)—they argue the case for their mutually supporting one another (the synergy illusion).

Similarly, where the reactionaries' jeopardy thesis posits threats posed by a proposed reform, liberals and progressives justify proposed reforms with an "imminent danger thesis," citing the dangers posed by *in*action, for example, threats to democracy from citizens made increasingly desperate by an inequitable welfare system. And where reactionaries argue the futility of reform action taken in the face of laws of social inertia, liberals and progressives discover laws of historical movement and progress (the thesis of "having history on one's side").

Hirschman aims at more than merely mapping "the *'rhetorics of intransigence'* as long practiced by both reactionaries and progressives" (p. 168; emphasis original). He wants to point to their survival in contemporary ideological discourse about domestic government—about which he is not overly optimistic, given its origins in ideological controversy. "Tolerance and acceptance of pluralism," he argues, "resulted eventually from a *standoff* between bitterly hostile opposing groups" (ibid.; emphasis original)—hence his "cool" method of examination, chosen to help "move public discourse beyond extreme, intransigent postures of either kind, with the hope that in the process our debates will become more 'democracy friendly' " (p. 168).

Lindblom's *Inquiry and Change,* rather than being about how people talk, is about how they think in politics, specifically "how people in contemporary industrialized societies . . . go about gathering and analyzing information in grappling with social problems. By people," he emphasizes, "I mean all kinds" (p. vii).

His concern is with problemsolving, but he thinks

terms like *attacking* or *grappling with* are more appropriate, since the process is one of unending inquiry. The motive force of this process is *Volitions* which arise from a person's sense that something in society is wrong or lacking and that something ought to be done about it. From this, "building on a foundation of fact, poorly or well constructed, people (never alone but always in social interaction) form, create, decide, will. . . . On how to live—what wants and interests to pursue—at every point and for every facet of this question, people create rather than find. Call what they create a volition. It might be called a judgment, but 'volition' has the advantage of stressing commitment or will" (p. 21).

Probing volitions is the crucial step, or element, in attacking problems effectively. The term emphasizes that the concern is for "thinking that is engaged in real-world problem solving" (p. 35), not just formal policy thinking. Probing is a continual, complex, and unsystematic process. It includes trying to define problems, thinking about normative and evaluative questions, wondering about facts, and logically organized inquiring and dreamy rambling: "Everybody always probes. However badly, all of us engage in examining volitions, even while asleep and both consciously and unconsciously in waking hours. . . . Inquiry mixes with and advances with action" (p. 30). The structure of their probing "takes the form of a web rather than a hierarchy" (p. 40) of means–ends overarching values. Means–ends analysis therefore becomes "a simplifying cogitative strategy" (p. 41), or heuristic, an echo of Herbert Simon and contemporary proponents of information processing.

Lindblom analyzes an important phenomenon that he calls "impaired inquiry" at the macro level and "defective thinking about social problems" at the micro level. He emphasizes that "we are all imperfect or defective investigators" (p. 61). We have to live with the fact that "biology limits the human capacity to probe. It also programs everyone to indulge in various 'irrationalities' for various reasons" (p. 61). Other agencies of impairment are not wholly beyond change or control, however. The two main categories of defects he examines are those which are socially caused (by, e.g., inadequate competition of ideas or socialization) and those arising from the advantaged positions of various participants in the process of probing, including the positions of parents over children, teachers over pupils, foremen over workers, whites over blacks, as well as elite positions in the communications media, education, business, religion, and government.

What can social science do about this? Lindblom's answer is straightforward and unflattering. He "cannot identify a single social science finding or idea that is undeniably indispensable to any social task or effort" (pp. 136–37). He questions applications of natural science models of science to political and social affairs, whose "mentalistic" phenomena are not amenable to explanation and prediction. But he does admit that social scientists can perform other useful intellectual tasks, including reporting (probably the main contribution social science has made and might make), evaluation (although "some heroic attempts at escape from evaluation continue in social science" [p. 147]), concept analysis and cognitive structuring (necessary for any inquiry into the world), checklists and scenarios, "meaning," synthesis and design, and methodology.

In contrast to a "science-guided society," Lindblom depicts a "self-guiding society," characterized by attacking problems with intelligent probing, freed as much as possible from impairments of inquiry. But probing here (unlike the more narrow probing of science) does not assume that with sufficient analysis every problem has a correct solution or pretend ability to determine the correctness of feelings or volitions. Social science here supplements, rather than replaces, lay probing. And it is much less hostile to power than are some versions of the science and other models—recognizing that "probing, discussion, and persuasion cannot take a society all the way to solutions" (p. 222). It requires, of course, a democratic, pluralistic framework within which to act.

Shklar argues in *American Citizenship* that U.S. citizenship has always been a matter of standing, rather than power, "an affirmation of belonging rather than the exercise of a right" (p. 26): "The tension between an acknowledged ideology of equal political rights and a deep and common desire to exclude large groups of human beings from citizenship has marked every stage in the history of American democracy. And it was this juxtaposition of slavery and constitutional democracy, above all else, that set America apart from other modern states" (pp. 28–29).

She demonstrates this by analyzing development of the right to vote and the right to gainful employment, contrasting citizenship-as-standing with conceptions of citizenship that played lesser, if any, roles in the United States: citizenship as participation alternately in ruling and being ruled (Aristotle), the citizen–soldier (Machiavelli), the loyal, obedient subject (Bodin and Hobbes), the citizen–proprietor (Turgot and others).

The case with respect to voting rights is documented by examining actions and behavior, as well as arguments, of proponents and opponents of extensions of the suffrage. A powerful bit of evidence, for example, is the failure of more than half the beneficiaries to exercise the vote after it was won (substantially more, in the case of youths, who were given the right without even asking for it). The evidence shows that the claim for extension of the rights of citizenship was in each case motivated by concern of the excluded for standing: "Not the exercise, only the right, signified deeply" (p. 27). More important, the fear of exclusion was expressed as a fear that without the vote as a symbol of citizenship, they would be no better than slaves.

After the erosion of property qualifications and enactment of the Fifteenth Amendment, race and gender replaced property as qualifications: "When disfranchised women saw black men achieve a right

that they still lacked, their deep racism quickly asserted itself, and it grew worse as they began to seek the support of Southern women. . . . This unhappy chapter in the women's suffrage movement . . . illuminates the darker side of citizenship as standing" (p. 57).

The right to vote has been extended to virtually all beyond a minimum age. But the desperate need for standing as a citizen now finds expression as a claim to the right of gainful remuneration: "The individual American citizen is in fact a member of two interlocking public orders, one egalitarian, the other entirely unequal. To be a recognized and active citizen at all he must . . . also be independent, which has all along meant that he must be an 'earner,' a fee remunerated worker, one who is rewarded for the actual work he has done, neither more nor less" (pp. 63–64).

Shklar finds that here, too, the efforts of workers from revolutionary days down to the present have been to achieve secure status as full members of the community (citizens), not just economic security or monetary gain. Today the Jacksonian ideology "has attached itself not to the right of the worker who demands respect in addition to an honest day's wage for an honest day's work, but to the unemployed. They want to work so that they can reclaim their citizenship as well as their paycheck. Who wants to be 'nothing'?" (p. 95). She finds the same logic motivating welfare recipients today.

Shklar's book is one for all U.S. citizens, as well as a scholarly interpretation to be profitably read by historians and political theorists; for voting and earning, "as they have emerged out of the stress of inherited inequalities, especially the remnants of black chattel slavery, in a society committed to political equality and to the principle of inclusion," are "the most elementary and essential components" (p. 101) of U.S. citizenship.

In *The Faces of Injustice*, instead of the customary question, What is justice? Shklar asks the deceptively simple question, "When is a disaster a misfortune and when is it an injustice?" (p. 1). Putting it this way requires looking at it from the victim's perspective. In any misfortune, the human tendency is for bystanders to see the injured party as a victim of accident or disaster, while victims instead feel aggrieved by an act of injustice, even in the case of an obvious "act of God." When victims' sense of injustice is aroused, they expect redress through the institutions of "primary" justice (Shklar's "more neutral" term for distributive or procedural justice), which assigns status and entitlement of members of the polity by legal rules; or they may perhaps seek revenge or retribution elsewhere. But those institutions, Shklar says, are blind to the possibility of *"passive injustice"*—meaning not "our habitual indifference to the misery of others, but a far more limited and specifically civic failure to stop private and public acts of injustice" (p. 6). And this makes answering her opening question much more complex and difficult. Involved individuals' drawing of the line between injustice and misfortune is subjective, and how they draw it in particular cases is an empirical question. But how polities draw the line is a political question, not a matter of prior legal definition.

Passive injustices and the sense of injustice aroused by them are a critical problem for all government and societies; for even a few citizens outraged at injustice can be a threat to public order, and larger numbers of them a threat to the polity itself. At the same time, the sense of injustice—"the special kind of anger we feel when we are denied promised benefits and when we do not get what we believe to be our due" (p. 83)—is "our best protection against oppression" (p. 85): "In the absence of realistic opportunities for choice, voice, protest, and denial, [the rules of primary justice] are nothing but incentives to injustice (p. 116). Democratic thinking accepts expressions of injustice as an appropriate reaction to an unwarranted social deprivation" (p. 84). Constitutional governments tolerate the expression of citizens' felt sense of injustice.

In short, what citizens consider to be injustice is an empirical, not a normative, question. How they will express their felt sense of injustice is also an empirical question. What legal procedures ought to be open to them to do that, however, is a political, not a legal, question. Shklar's message hardly stops there. It has strong implications for public policy, public law, and civic action. "This whole book is really about America" (p. 6), she says. Like *American Citizenship*, it is a book for all U.S. citizens and not just academe.

Herbert Simon is a scholar unique in the breadth of his talents and skills, the range of his interests and corresponding range of his activities, and the level of his attainments. His comprehensive, candid, and honest autobiography, *Models of My Life*, will pleasurably reward and instruct any reader about the objects of Simon's interest, as well as about the man himself.

His professional career began in public administration. He taught at the Illinois Institute of Technology and then at Carnegie–Mellon University (then Carnegie Institute of Technology) until his recent retirement. He was a moving force in establishing the latter's Graduate School of Industrial Administration and in bringing its business school to recognized national leadership. His departmental affiliations, like his work, spanned economics, psychology, and computer science. He was a frequent participant in academic government and politics as a faculty member, department head, and dean. He resisted the temptation to be a university president when offered the opportunity, but regularly took active part in the selection of top administrators, as well as in other university-level decision making.

He had continuing affiliate or consulting relationships with the Rand Corporation and the Cowles Commission for Research in Economics and consulting relationships with the Ford Foundation and others. He was active in the Social Science Research Council. He turned down opportunities to become president of the American Political Science Association but was active in it in many other ways, includ-

ing participation in the Young Turks "rebellion," which led to the replacement of the then-incumbent executive secretary.

He became a fellow of the American Econometric Society and a Nobel Prize winner in economics—although classical economists generally shunned him for his advocacy of the concept of "bounded rationality." He had a major hand in bringing the social sciences into the National Science Foundation and the National Academy of Sciences, of which he is a member. He was a member of the Committee on Sciences and Public Policy (and the expanded Committee on Science, Engineering, and Public Policy) and the President's Science Advisory Committee.

His doctoral dissertation on public administration (published as *Administrative Behavior*) is generally credited with having been one of the earliest and most decisive steps in carrying out the "behavioral revolution" called for by Charles Merriam at Chicago (when Simon was a student there). He went on to play pivotal roles in the fields of organization theory and theories of the firm, artificial intelligence, cognitive science, and information processing. Besides seminal contributions in all these fields, he has produced major works in economics, econometrics, and decisionmaking.

Models of My Life is not a recitation or chronology of these accomplishments but a lucid account of each major enterprise in his career, including its substance, the rationale behind it (in terms intelligible to outsiders), and its politics. At relevant points throughout are statements and explanations of his thoughts, ideas, feelings, and preferences about issues, ideas, and practices of major concern to social scientists.

EMERGENT THEMES

Collectively considered, these six works offer much food for thought about scientists' social and civic role. Sheldon Wolin has long argued that political theory "is primarily a civic and secondarily an academic activity."[2] Donald L. Langenberg has recently called attention to "a damaging disjuncture between the community of scientists and engineers and the society in which the community is embedded," which he attributes to "our failure to do our civic duty."[3] "Our universities," he says, "and our science and engineering community can and must become more centrally and fully engaged with the great societal issues represented by the slogans I have listed and many others."[4] Such engagement is important for all scientists as citizens, but especially important for social scientists, because "societal reality is, in the end, determined by societal perceptions through the political process, regardless of whether those perceptions are grounded in fact. And that, unfortunately, is what gives truth to [the] adage, 'Only in politics are facts negotiable'" (p. 361).

Engagement with great societal issues is strongly evidenced by at least five of our six works: Shklar's analysis of the cancer of race, which continues to gnaw at the vitals of U.S. society, and her account of injustice; Lindblom's examination of social scientists' probing of social problems (evidence of both his own engagement and his concern to promote that of social scientists and lay citizens); Hirschman's application of his analysis of the rhetoric of intransigence to contemporary political discourse; and Simon's account of his career as a demonstration of the varieties of forms and fields in which engagement is possible. Easton alone, of this group, might appear to avoid confronting social issues. Whether one sees this as an evasion of civic duty or a legitimate desire depends on one's views about the utility of Social Science Knowledge.

What is the role of social scientists qua scientists in society? To do research purely for knowledge's sake or to do research devoid of theoretical basis but aimed directly at social problems? Easton does believe that "a deeper and richer understanding of political life" is desperately needed to cope with society's complex global problems and that general theorists and pure scientists contribute to that. But he also believes that long-range scholarship—today especially—requires insulation from "the unfortunate siege mentality forced upon [it], where the problems for inquiry will be "the problems that theory dictates as requiring attention, not necessarily [those dictated] by the pressing and urgent issues of the day" (p. 287).

By itself, that answer begs the question. The usual academic argument for it is that abstract ideas and even whole theories that at first lie fallow or are rejected outright might turn out in the long run to be founts of endless practical wisdom. It is therefore wrong to ask pure scientists to engage only in medium-range or short-range projects whose relevance to contemporary issues is apparent but whose long-run benefits are no more certain than those of pure theorists.

With respect to the issue of pure versus applied science, the two viewpoints are not mutually exclusive in the overall scheme of things, of course. Easton implies a functional division of labor between pure theorists and others: "I have assumed . . . that there is still room for at least some few scholars who continue the pursuit of basic knowledge" (p. 287). Nowhere does he express objections to research more directly relevant than the theorist's to pressing social problems. His argument includes the necessity for bringing general theory down to earth. But the implication is that lower-level conceptualization of the problems and issues and operationalization of theory relevant to them is primarily the task of scientists other than the general theorists.

Both Lindblom and Simon argue against such a division of labor. Lindblom can find little difference between the investigatory activities performed by professionals and lay probers: "Professional inquiry consists for the most part of lay inquiry done better" (p. 161). Both professional and lay testing "finally rest on judgment rather than some conclusive technique" (p. 169). Perhaps more important, professional social

scientists, both pure and applied, are prey to inquiry-impairing defects peculiar to their own disciplines, which Lindblom discusses at some length.

Moreover, Lindblom points out, "Academic, discipline-oriented, professional inquiry not focussed on policy" and "policy studies that culminate in recommendation" are not the only two choices open, as some practitioners in the "subprofession of 'policy analysis' might imagine" (p. 271). A superior alternative is for advisers to "choose specific analytical contributions to fit the needs and competencies of the functionary. On most of the client's decisions, such analysts would remain silent. On a small number they would selectively combine their competence in inquiry with that of their clients. On only a small number of the client's problems would analysts proceed with the do-it-all or reach-a-recommendation model that some policy analysis textbooks advise" (p. 272).

Simon's position, like Lindblom's, argues for the merging—not the separation—of the efforts of pure and applied scientists. During faculty discussions of the curriculum at Carnegie-Mellon he proposed at one point "that the university establish a core curriculum for the faculty, requiring every CMU faculty member to pass the comprehensive examinations on the common core of the program within the next four years. . . . Then [says Simon] came the punch line: a faculty that is not liberally educated can't provide liberal education. American college faculties, including our own, aren't liberally educated" (p. 267). Despite the rigor and "scientific" discipline of his own work, Simon is no believer in "the myth of the 'exact sciences' or 'hard sciences'. . . . The true line is not between 'hard' natural science and 'soft' social sciences, but between precise science limited to highly abstract and simple phenomena in the laboratory and inexact science and technology dealing with complex problems in the real world" (p. 304).

Similarly, with respect to the distinction between pure and applied science, Simon writes, "The distinction between the scientific and the professional is largely a distinction between analysis and synthesis. Professionals not only analyze (understand) situations, they act on them after finding appropriate strategies (synthesis)" (p. 258). Like Lindblom, Simon would bring applied and pure science practitioners together, not divide them functionally. Like Lindblom, Simon believes it is the more abstract, pure disciplinarians and academicians who most need persuading, as shown by his views on the unhealthy trend he saw developing in professional schools like engineering, business, and medicine, where "[pure] science was replacing professional skills in the curriculum. . . . Engineering schools were becoming schools of applied physics and mathematics, and medical schools were becoming schools of biochemistry and molecular biology. Professional skills were disappearing from the curricula and professionals possessing those skills were disappearing from the faculties" (pp. 257–58).

Collectively, our six works make an effective argument against the dangers of disciplinary parochialism, or balkanization, by avoiding it. They draw ideas and evidence from history, philosophy, and empirical studies in psychology and other fields—not just from political science. They are more "interdisciplinary" than disciplinary. Simon, however, besides spanning and applying his own four main specialties—and others as well—also speaks explicitly and eloquently about the problem: "Academicians typically live out their whole careers within the culture of a discipline, rarely shaking off the parochialism this isolated existence engenders" (p. 173). Again, disciplines, like nations are a necessary evil that enable human beings of bounded rationality to simplify their goals and reduce their choices to calculable limits" (p. 367). About area studies (which are often thought to be interdisciplinary), he observed, "Too often, they seemed to aim at training disciplinary specialization *within* area specialization: Experts of the Russian economy, the Chinese government, the Indonesian family" (p. 173).

It is easy to agree that a world in which "parochialism is everywhere . . . badly needs international and interdisciplinary travelers to carry new knowledge from one enclave to another" (p. 367), but it is not easy to know how to combat it. During the development of the Center for Advanced Study in the Behavioral Sciences, Simon advised the Ford Foundation that "the problem is less one of bringing unlike social scientists together than one of bringing unlike social sciences together in one man" (p. 170). But he "can offer one piece of advice to others who wish to try an itinerant experience: It is fatal to be regarded as a good economist by psychologists, and good psychologist by political scientists" (p. 367).

The lessons to be learned from reading these works (all splendid examples of "probing"), then, are more ethical instruction than empirical fact or theory. They help instruct us how to be good scholars and scholarly citizens not by maxim or catechism but by revealing the complexity of that goal and living up to it. They all agree that failure to extend our scholarly interests and conceptions outside our narrow disciplinary boundaries is bad scholarly performance. They make clear scholars' ethical obligation to be engaged citizens, not merely disengaged inhabitants, of the wider communities of disengaged inhabitants of which we are members. They also suggest that we have much more to learn.

Notes

The credit for beginning this train of thought goes to Helen Ingram, who, as Book Review Editor, recognized almost at once upon receipt of them what the books had in common.

1. Hirschman's starting assumption was that in each of the three stages, and not merely the last of them, the initial thrust to extend the rights of citizenship was followed by "ideological counterthrusts of extraordinary force" (p. 3). T. H. Marshall, whose scheme Hirschman followed, hypothesized sturdy resistance to the initial movements for reform but eventual triumph for all three; Ralf Dahrendorf claimed the battle was never won in the third phase.

2. Sheldon Wolin, *The Presence of the Past* (Baltimore: Johns Hopkins University Press, 1989). 1. See also his *Politics and Vision* (Boston: Little, Brown, 1960) Harvard president Derek Bok has expressed doubts that universities and scholars are doing enough to develop and bring into play, in their own work and teaching, "a strong sense of civic responsibility, ethical awareness, and concern for the interests of others" (quoted by Donald L. Langenberg, 362) *Universities and the Future of America* (Durham: Duke University Press, 1990, 7).

3. "Science, Slogans, and Civic Duty," *Science Magazine*, 19 April 1991, pp. 361–63. Langenberg is chairman of the board of directors of the American Association for the Advancement of Science.

4. The applicability of the argument to social scientists is further indicated by the number of issues on Langenberg's list of 25 "slogans" (shorthand symbols symbolizing the issues) that are of obvious interest to them, including scientific freedom and integrity, "Set scientific priorities," "Save the environment," the information age, science literacy, educational reform, "Politically correct," multiculturalism, "No new taxes," and others.

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POLITICAL THEORY

Nazi Economics: Ideology, Theory, and Policy. By Avraham Barkai. New Haven: Yale University Press, 1990. 291p. $27.50.

As research and debate on German National Socialism have expanded during the past 30 years, opinion has sometimes divided between the so-called *intentionalists* and *structuralists*. The latter find little evidence of coherent ideology or grand design in the policies and government of the Third Reich, which they contend was severely conditioned by contingent circumstances, leading to one improvisation after another, though each in varying degrees constrained by what had gone before. The evidence they cite is at first glance extensive, consisting of the apparent contradictions in Nazi doctrines and Hitler's own statements, the absence of any single clear, unified ideological blueprint, the "organized chaos" of multiple conflicting and overlapping government and party boards and administrative units, and the apparent improvisation of so much of Hitler's timing and strategic gambles. In the most extreme formulations of this school, Hitler himself was a "weak dictator," not fully in charge of the German government and not necessarily responsible for some of its most extreme decisions.

The intentionalists reject such historiographic revisionism. Though they generally agree that National Socialism had no such formally developed and unified ideology as liberalism or Marxism–Leninism, they have concluded that Hitler had a drastic, clearly enough defined *weltanschauung* and, though always willing to compromise on details and short-term policies, held to his broader political and strategic goals with a grim ruthlessness equaled only by that of his grand rival and foe, Josef Stalin. He did not administer personally the Nazi state (for bureaucracy and microadministration bored him); but he guided it firmly in the direction that he sought, ultimately at the cost of risking total annihilation.

Avraham Barkai, a research fellow of the Institute for German History at Tel Aviv University, is a firm intentionalist. His study, which first appeared in a shorter version in German in 1977, does not deal with the broader dimensions of National Socialist ideology or administration, concentrating instead on the specific issues of economic doctrines and policy. In this field, too, the structuralists have defined the Nazi approach as an incoherent and opportunistic hodgepodge, lacking clear theory or system. Barkai provides the first systematic treatment of Nazi economic doctrines in the years prior to Hitler's takeover and sets it clearly in the tradition of the nineteenth-century "German economics" of statism and state socialism. He then goes on to examine in detail the policies and economic administration implemented by the regime during its first three years in power.

He convincingly concludes that though "Nazi ideology was not a unified and consistent conceptual system," the party "was unquestionably an ideological movement, even though its ideas were a jumble collected from here, there, and everywhere without any worthwhile original contributions" (p. 243). The product, by 1933, was a pragmatic collage that emphasized extreme state control and regulation, together with massive deficit spending and credit creation, aimed at ending unemployment and gearing the economy for war. Most general lines of economic policy had already been defined in statements and position papers before 1933. Though central planning was rejected, the Hitler state created a new framework of government control that reorganized and dominated the administrative units of industry and other major sectors, controlling prices and channeling profit, investment and raw materials. The market was not abolished, but it was firmly controlled. Unemployment was eliminated within four years, while production increased rapidly and inflation was effectively checked. The goal was autarchy and self-sufficiency—and rapid reconversion for military expansion. In this regard, Barkai shows that major investment in rearmament was not postponed until unemployment had been ended but had already begun before the end of 1934. Industrialists enjoyed internal autonomy in the production process, as well as strong financial incentives to produce, but had no voice in general policy, priorities or investment, and the use of profits was carefully regulated. Though this system was not the result of any clearly defined new theory, it had a rationale and a purpose and, in fact, worked remarkably well. The goal was not the highest efficiency or the maximal productivity but stability and the achievement of state priorities—goals that were fully attained.

Was the economic system of National Socialism capitalist? Barkai says *yes*, in that the ownership of most means of production remained theoretically in private hands; but it was not a capitalism dominated at the policy-making level by capitalists or one in which they enjoyed much freedom.

For the new, English-language edition of this book, Barkai has added a lucid 18-page chapter on the wartime economy from 1937 to 1945 to provide a fuller perspective. Most of the monographic research on Nazi economic administration still remains to be done, but Barkai's work has been the first to delineate accurately the economic doctrines of National Socialism, place them in the German ideological context, and relate them to the initial policies and administrative structure of the regime. It remains a major contribution to the history and analysis of National Socialism.

University of Wisconsin, Madison STANLEY G. PAYNE

A Treatise on Social Justice, vol. 1, Theories of Justice. By Brian Barry. Berkeley: University of California Press, 1989. 428p. $55.00.

This first volume of Brian Barry's projected three-volume treatise discusses the two dominant approaches to social justice in the Western tradition. The first runs from the sophists through Hobbes to theorists like David Gauthier. It understands justice as a contractual agreement among self-interested individuals seeking mutual gain, as contrasted to a nonagreement world or state of nature where egoists engage in untrammeled competition. The second position flows from the stoics through

Kant to theorists like John Rawls and argues that justice emerges from an impartial point of view that does not make explicit reference to one's own advantage.

This volume compares justice as mutal advantage with justice as impartiality through an intensely rigorous survey and critique of the last 30 years' discourse on social justice. It focuses upon game-theoretic discussions of "fair division," utilitarian theories, and the work of David Hume and John Rawls. Barry addresses theories of social justice that assess basic institutions and the distribution of benefits and burdens over which individuals conflict such as rights, power, and scarce resources." The volume ends with Barry's own formulation of "the circumstances of impartiality." Volume 2 will elaborate and apply justice as impartiality, while Volume 3 will apply Barry's approach to wealth and income distribution.

Part 1 of the present volume analyzes "the so-called problem of fair division" as the basis of justice. This analysis revolves around a lengthy discussion of R. B. Braithwaite's formulation of how two musicians with differing utility curves can divide playing time "fairly." Barry points out that all mutual consent theories are driven by the assumed motive that individuals will seek to escape a nonagreement point that makes them worse off. Consent theories also claim that only self-interested agreement will give justice any stability or plausible chance of being accepted, especially by those with great resources.

Barry convincingly argues that all such theories generate outcomes that replicate or aggravate morally contingent inequality among parties. He demonstrates how these theorists, especially Gauthier, try to mitigate this unequal outcome by eliminating threats or imposing "irrational" strategies on individuals negotiating the hypothetical contract. But these arguments lack consistency with their basic premise.

The treatise also finds interpersonal utility an insufficient foundation for a theory of justice in either utilitarianism or "fair division" terms. This approach, says Barry, can be distorted by strategic misrepresentation and is built upon unstable claims. He concludes, "We must surely regard as suspect any solution that could be put into effect accurately only by Almighty God, unto whom all hearts are open, all desires are known, and from whom no secrets are kept" (p. 100). Instead, he proposes a "resourcist" solution, which defines issues in terms of the distribution of the resource itself, not the utility it generates. He believes that this approach will be more practical, plausible, and potentially egalitarian.

Part 2 shifts to Hume and Rawls and argues that they attempt an unstable compromise that seeks to unite mutual interest and impartiality. In both theories, the logic of mutual interest combined with the requirement to accommodate an unequal baseline undermines the claims derived from impartiality. Generally, Barry suggests that "circumstances of justice" require that original positions or veils of ignorance be devices to manipulate information and uncertainty to make justice "rational" for egoists. Barry believes that John Harsanyi has correctly shown that this logic drives individuals to average utility, not a Rawlsian difference principle. He believes Rawls must constantly "thicken" his original position in later works to account for this until the limitations on information recapitulate the conclusions Rawls wants.

Barry is committed to Rawls's egalitarianism, but believes Rawls's power derives from rejecting the moral relevance of contingent inequalities. But Barry claims that Hume and Rawls demonstrate that one cannot blend impartiality and mutual interest into a unified theory. The mutual benefit side constantly undercuts the demands of impartiality. This becomes evident in Rawls's difficulties in extending the theory to future generations and international relations.

Finally, Barry articulates his own position in part 3. He builds on what he calls a "constructivist" account of justice, where a theorist creates a "choice situation" specifying the power, knowledge, rules of reason, and other aspects bounding the contractors. The principles that emerge from deliberation under the conditions of the choice situation count as just.

Barry's choice situation is not hermetically sealed but interacts with other human intuitions about justice and requires an active acceptance of reasons by others. In his boldest and most fundamental move, he rejects self-interest as the only stable motive for justice. Human psychology and history suggest that humans possess a "moral motive" to justify their actions in terms that do not reduce to power or their own advantage. His conditions of impartiality postulate individuals who know their identity but seek principles "reasonably acceptable" to all. This account presumes what he calls "human decency" and moves justice from a game to a debate, with actors working to convince others with reasons that all can accept. Impartiality takes on an active political and democratic cast with a requirement that contractors accept the reasons. Justice also takes on less exactitude and deductive power than Rawls or utilitarians would prefer. It opens the possibility of a range of "just" decisions made within a broad framework determined by impartial deliberation.

This is a rich and dense book written with aplomb and technical care. As a treatise, it not only presents Barry's theory but also explicates and critiques substantial accounts within the field in a manner that invites further debate. It makes accessible an often arcane but influential set of reflections of the last 30 years and consolidates the insights of game theory, utility, and original position ideals of justice. Several technical appendixes are addressed to specialists; but generally, the book unites passion, technical competence, and rigor.

In one particular aspect it serves as a model of how intellectual debate should occur. Barry's careful, respectful, and relentless study and critique of Rawls's theory leads to the refinement and reformulation of Barry's own theory. His analysis of Rawls teaches how theoretical engagement can be done right. Political theorists interested in justice will find it illuminating and helpful as it places the debates of philosophers, economists, and game theorists within the tradition of political theory. By readmitting complex human motives and supple notions of reason, Barry articulates an approach to social justice for politics, not just for philosophy and economics.

University of Washington J. PATRICK DOBEL

Religion in Public and Private Life. By Clarke E. Cochran. New York: Routledge, 1990. 226p. $49.50 cloth, $15.95 paper.

This volume explores two themes: the nature of public and private life and the role that religion should play, particularly in the public sphere. The argument is essen-

tially a prescriptive one, set out mainly within the context of the U.S. experience. Thus, within that culture, Cochran posits a distinction between a public and a private world, each "possessing distinctive features" yet, at the same time, "not radically separate. . . . These spheres intersect, forming regions of overlap and border territories" (p. 3). The import of religion is that it is one of the major social experiences inhabiting both spheres, especially that crucial area of overlap. On the one hand, it has a private face—the private and personal, most emphasized by Protestant understandings. On the other, it has a public face—the corporate challenge to participate in wider communal endeavors.

This, in turn, brings religion into the political domain, which is a major part of the public sphere. A tension is set up between the two, each—religion and politics—pushing against and challenging the other. Religion's role, therefore, is to connect the public and the private—to build up from private individual experience into the public world of politics and power to which it is fundamentally drawn. In that world, religion "contests . . . relativism by advancing in public debate principles claiming sacred roots" (p. 159). By speaking of justice and peace in a sphere largely governed by self-interest, it draws "politics beyond the level where it would otherwise settle" (p. 163).

In making these arguments, Cochran usefully contests—and rejects—the idea that God is to be restricted to the sphere of religion, private morality, and personal piety. He also, in advocating a *tensional model* between religion and politics, shows that religion can and should be taken seriously in politics—that politics needs the moral challenge of religious belief. It is a stance that rejects the idea of a "civil religion" (in which religion provides a moral foundation for the culture and the state), as well as theocracy. Equally, the relationship cannot be appropriately conceived, he suggests, through notions of secularization or the institutional separation of church and state. In short, he skillfully sets out a particular vision of the way in which religion should play a significant political part.

Two observations, however, are in order. First, perhaps too much of the book was spent in teasing out the public–private distinction to the detriment of working out in detail the tensional model of religion and politics. Some of this there is—on abortion, for example. But, I'd like to have seen more illustration—not just instances where conventional ideas about morality might seem most relevant, like sexual and family issues, but also in other areas like economic and foreign policy debates. This would help emphasize that valid point Cochran makes—that politics can and does teach religion the "realities" of life. It would also help clarify more precisely what Cochran means when he suggests that religion is there to help politics move beyond power and (self-)interest to discover a sense of adventure and a sense of worth.

Second, the argument is largely written against the backdrop of the U.S. experience; yet the issues of public versus private and religion versus politics are apparent in other settings. An exploration of some of these experiences and debates would have been appropriate. One limited but useful step, for example, might have been the consideration of the situation in England, which contributed considerably to the U.S. cultural and institutional framework. Not only, of course, is there an established Church of England representing a very different normative resolution of religion's public claims within liberal democracy, but there is also a substantial prescriptive literature (e.g., Ecclestone's *Church of England and Politics*, 1980; Preston's *Church and Society in the Late Twentieth Century*, 1983; and Temple's classic *Christianity and Social Order*, 1976). Such a broader canvass would have put the argument in greater relief. Perhaps this merely calls for a follow-up volume! If it does, it is a testament to a stimulating effort at clarification and advocacy.

University of Vermont GEORGE MOYSER

History, Power, Ideology: Central Issues in Marxism and Anthropology. By Donald L. Donham. Cambridge: Cambridge University Press, 1990. 242p. $49.50.

This study alternates between general theoretical discussions and ethnographic detail in a particularly fruitful fashion. Donham defines neoclassical economics as minimally characterized by a claim that preferences can be unambiguously ranked and a focus on individual maximizing activity within the context of a given preference structure that remains stable in the short term. Thus, Donham claims that neoclassical theory has wide applicability in time and space at the same time that he argues that it is quite incapable of either significant historical explanatory power or of addressing issues of power and ideology. These critical areas require an analysis of both historical contingency and structural causality. The book illustrates this theme through an analysis of Maale ethnographic data, particularly data on productive labor, including that of cooperative work groups, leavened by dialogue with anthropological and economic theory.

Donham makes a number of brilliant contributions to Marxist theory. These can best be appreciated by contrasting Donham's positions with those of scholars whose work has had the greatest formative influence on his argument. Donham's starting point is Gerald A. Cohen's argument that the superstructure provides essential stability for the base (*Karl Marx's Theory of History*, 1978). Donham pushes this position a little further, suggesting that the superstructure is in fact the sine qua non of *productive inequalities* (p. 195; his brilliant translation of Marx's *Produktionsverhältnisse*, usually glossed as "social relations of production"). Donham follows Cohen in arguing that Marxist explanations of this link are fundamentally functionalist ones. In doing so he uses Raymond Williams's notion of *epochal analysis*, (*Marxism and Literature*, 1977) but tries to push this idea, too, a little further. Rather than circumscribing the term to the analysis of dominant, residual, and emergent cultural processes (constituting the essential ties to history of a specific epoch), Donham argues that a similar conceptualization of patterned space, with respect to power, is also required, on the lines of Immanuel Wallerstein's *Modern World System* (1974) or George W. Skinner's The *City in Late Imperial China* (1977). More generally, he argues that the concept of *articulation* relies too much on functional ties (his key example is Claude Meillassoux's *Maidens, Meal, and Money*, 1981) and so presupposes an exaggerated concurrency in time and place. His solution is to combine insights from Edward P. Thompson, who argued that epochal analysis bypassed the chaos of history, (*"The Poverty of Theory" and Other Essays*, 1978),

with Cohen's functional interpretation applied to epochal analysis. In brief, Donham suggests that epochal analysis reveals how the die of history is loaded—how productive inequalities are arrayed in a given point in time—but cannot fully explain origins of, or transformations in, epochal structures. To do so would require, in addition, a broader historical approach and an appreciation of how historical changes would lead to changes at the level of individual maximizing behavior.

On the ethnographic side, the first contribution of the book is to provide a particularly persuasive analysis of the interconnections between Maale household dynamics, Maale cooperative work arrangements, and traditional justifications for productive inequalities favoring male elders—an ideology based on a fetishism of fertility (rather than of market commodities). The second contribution is to link this analysis both to structures of domination of national and international scope and to historical processes not adequately circumscribed by such units of analysis. Donham's method of choice is to begin with a puzzle: contra Aleksandr Vasilevich Chayanov (*The Theory of Peasant Economy*, 1966), households do not seem to increase their productive work as the ratio of household consumers to household workers increases. Further, contra Marshall D. Sahlins (*Stone Age Economics*, 1972), these discrepancies cannot be explained by redistribution of agricultural products. The solution to the puzzle is that senior households provide labor (of young men in their household), not agricultural products, to households with high needs relative to productive capacity and do this to maintain their social and economic standing as guarantors of fertility in the face of the socialist principles of the 1974 revolution and the antitraditionalist principles of newly converted protestants. Donham is particularly convincing when he suggests that cooperative work arrangements potentially combine to link the population in a variety of ways, the 1975 system being a recent response quite different from that pertaining to the period from the late nineteenth century to the eve of the revolution. Donham's analysis suggests, moreover, that the dominant configuration, defining the basic productive inequalities in Maale society, would more generally have varied significantly over both time and space. Rather than providing a contrast between modern capitalist and traditional noncapitalist economies, Donham suggests that there are no qualitative socioeconomic differences justifying qualitatively different theoretical approaches. He suggests that the other is, indeed, us if properly understood (p. 15).

This book is highly recommended not only for all Africanists but for all readers interested in political economy in its widest sense.

University of Arizona THOMAS K. PARK

The Unvarnished Doctrine: Locke, Liberalism, and the American Revolution. By Steven M. Dworetz. Durham: Duke University Press, 1990. 247p. $37.50.

A Union of Interests: Political and Economic Thought in Revolutionary America. By Cathy D. Matson and Peter S. Onuf. Lawrence: University Press of Kansas, 1990: 237p. $25.00.

It was not that long ago that the Hartz thesis still commanded the loyalties of U.S. scholars who were convinced that the United States was imprisoned by a liberal consensus where Lockean ideas held undisputed sway. However, as Stephen M. Dworetz observes in *The Unvarnished Doctrine,* scholars have abandoned Hartz with astonishing speed and, in place of Lockean liberalism, have substituted an equally omnivorous civic republicanism, which now seems to include everyone except Locke (p. 5). Thus, it is refreshing to find two books arguing that the republican revision is now in need of revising—and from a liberal perspective, no less!

Dworetz's book is important for both historiographic and political reasons. On a historiographic plane, Dworetz conducts "a critical examination of the republican revision and a reassessment of the role of Lockean–liberal ideas in the American Revolution" (p. 7). Dworetz reexamines revolutionary pamplets, election sermons, and newspaper essays to find that a reading of the texts "testifies consistently and often explicitly" to the importance of Locke's liberalism and to his revolutionary stature in the minds of American colonists (p. 8). Thus, Dworetz's historiographic objective is to restore Lockean ideas to a position of importance in the American Revolution and to suggest, at a minimum, that Lockean liberalism coexisted with (or, perhaps, was a part of) the republican ideology. Indeed, Dworetz concludes that American revolutionary ideology is best understood as "liberal republicanism."

However, Dworetz considers the rehabilitation of Locke important for political reasons that extend behond scholarly polemics. Dworetz points out that in the United States "the dominant understanding of the founding doctrine . . . contains prescriptive implications for public policy and constitutes the essential source of historical legitimacy for any general political program" (p. 4). In this context, Dworetz correctly calls attention to the seamy underside of civic republicanism: "Militarism, imposed social and religious conformity, and even American slavery are known to have been justified in the language of civic republicanism" (p. 5). Consequently, Dworetz fears that by expunging Locke from the founding ideology "the reinterpreters exclude from the ideology of the American revolution, or radically demote therein, the one tradition of political thought that *inherently* authorizes religious toleration, constitutional politics . . . and popular revolution against actual or intended tyranny" (p. 4). On these points, Dworetz's work hints that the emergence of "republican" historiography parallel with the rise of the Repulican party is more than linguistic synchronicity. Behind the scholarship of Bernard Bailyn and J. G. A. Pocock lurks the politics of George Bush, William Bennet, and Jesse Helms. Hence, Dworetz seeks "to persuade the democratic critics of liberalism to think twice before jumping onto the bandwagon of American republicanism" (p. 193, n. 2).

On a much broader front, Cathy D. Matson and Peter S. Onuf argue in *A Union of Interests* that while American revolutionaries and early state builders "naturally found republican ideas attractive," the republican political discourse disguised "significant 'liberal', or progressive tendencies in American political economic thought" (p. 11). Echoing Dworetz's theme, Matson and Onuf also challenge the republican synthesis by pointing out that "in historiographical retrospect, the contradictory tendencies in American revolutionary thought and practice appear striking" (p. 11). The authors utilize an impressive array of primary sources to illustrate how U.S.

political economists "drew on classical republican, liberal, and jurisprudential traditions, among others, to conceive and construct solutions to the problems they saw themselves confronting in the aftermath of the Revolution" (p. 2).

The authors' main thesis is that a clash between classical republicans and liberal nationalists was precipitated by a "crisis of the union . . . defined in economic terms" (p. 2). Matson and Onuf argue that during the critical period a variety of "commercial interests" became increasingly dissatisified with the Confederation and began to criticize it from a nationalist perspective. According to the authors, the nationalist movement was initially based in international shipping and trading interests, a coalition that included U.S. shippers, commercial farmers (exporters), and public creditors (pp. 45, 70–71, 147). These groups were soon joined by a "manufacturing interest" consisting of merchants and wealthy mechanics, who then enlisted the support of urban artisans and small farmers engaged in household manufactures (p. 46). Finally, parties interested in the development of the Western lands joined the coalition, including Southern planters and Western farmers seeking a government that could maintain free navigation on the Mississippi River.

At the same time, the authors contend, the critical period exposed a latent tension between virtue and interest in republican ideology. As they note, the republican view of political or moral economy "was founded upon the premise that political right and personal virtue flowed from the ownership of hereditary and transmissible land" (p. 12). However, in the United States, "the idealized republican yeoman too often proved to be among the most rapacious and greedy patriots, not the most virtuous" (p. 36). The agrarian cornerstones of republican ideology were more interested in farm exports and the price of public securities than in virtuous citizenship.

According to Matson and Onuf, what emerged from the confrontation between traditional republicanism and a rising liberalism was the discourse of U.S. *federalism*, in which the ideological disputes between liberal nationalists and classical republicans were finally narrowed to divergences over the *scope and scale* of a federal republic. The authors demonstrate how a distinctive political economy of federalism was forged by a gradual melding of these interests in pamphlets and newspaper essays. By the time *The Federalist Papers* were published, most of the federalist ideas were already in wide circulation and pointing toward "a union of interests" structured by a vision of national economic development. Matson and Onuf thus rightly conclude that the genius of the Federalists was to link ratification of the Constitution to the rapid development of a national economy "so as to appeal to the many optimistic and enterprising Americans who hoped to reap the rewards of independence" (p. 2).

Southeastern Massachusetts University CLYDE W. BARROW

Topics in Social Choice: Sophisticated Voting, Efficacy and Proportional Representation. By Dan Felsenthal. New York: Praeger, 1990. 208p. $47.95.

Dan Felsenthal uses the tools of social choice theory to evaluate three topics overlooked in most of the literature. Part 1 explores the effects of assuming that voters use sophisticated strategies in simultaneous voting arrangements; part 2 evaluates voter efficacy in small electorates and under different voting rules; and part 3 applies a social choice framework to proportional representation schemes. While it considers some important extensions to the existing literature, this book serves best as a supplement to, rather than a replacement for, core texts.

Part 1 incorporates theoretical and experimental analyses of sequential sophisticated voting strategies and provides the most complete response to any of the three issues raised. Using a two-by-two design, Felsenthal compares plurality and approval voting schemes under sophisticated and sincere strategies to determine to what extent either voting mechanism is vulnerable to the undesirable properties associated with sophisticated behavior as defined in the models of Robin Farquarson (*Theory of Voting*, 1969) and Niemi and Frank ("Sophisticated Voting under the Plurality Procedure," in *Political Equilibrium*, ed. Peter Ordeshook and Kenneth Shepsle, 1982). He finds that approval voting does somewhat better in terms of Condorcet efficiency but that both procedures violate a series of socially desirable properties when sophisticated strategies are used. In response to these findings, Felsenthal develops a new behaviorally consistent model of sophisticated voting based on tacit cooperation among supporters of a Condorcet winner. Laboratory experimental tests comparing the tacit cooperation model of sophistication with Farquarson's, Niemi and Frank's, and sincere voting models find support for the tacit cooperation model. This is an important contribution in two respects. First, one of the major criticisms of models assuming sophisticated strategies is that the voter must presume that other voters are always pursuing a sophisticated strategy when one is available—an assumption that is difficult to meet in natural settings. By assuming a sophisticated strategy in which voters should vote sincerely under certain conditions and sophisticatedly under others, Felsenthal's model allows voters to assume a middle ground in which sophistication is used when it would benefit voter's interests. Second, many previous empirical studies find relatively low levels of sophistication, a result that is troubling, given the assumptions of social choice. By redefining sophisticated strategies in this way, Felsenthal incorporates a broader set of behaviors under the banner of sophistication and shows that voters behave sophisticatedly more often than has previously been demonstrated in empirical tests.

The question of how effectively voting mechanisms translate an individual's preference into social choices is at the heart of social choice analysis, and Part 2 explores this question under two rather unusual voting arrangements and under conditions where results are sensitive to the size of the voting population. Plurality and cumulative voting systems are compared and found to produce similar results, suggesting that the added difficulty associated with a cumulative scheme is unjustified. Felsenthal evaluates a combined approval/disapproval voting system to find that it provides greater voter efficacy than the approval mechanism alone, particularly when the number of candidates is large.

One of the most interesting chapters contains an evaluation of how sensitive plurality outcomes are to different quorum rules. While the results presented in this section are individually interesting, their overall

impact is limited by the fact that their tie to the larger literature is left largely to the reader to devise. This tie-in is especially important, considering that the voting rules examined are relatively unusual. The reader would have benefited had the efficacy of these voting arrangements been placed in the context of other studies of voter efficacy and the implications of the results been drawn out more carefully.

While many democratic electoral systems rely on proportional representation, social choice has paid relatively little attention to this form. In an effort to correct for this oversight, Felsenthal develops a normative standard for the distribution of seats in proportional representation schemes using the social choice tehcniques originally developed to deal with single-representative systems. Again, the emphasis is on the link between voter preferences and social outcomes; and three different methods of seat allocation are developed and compared. These methods are tested empirically against plurality and approval voting schemes using the 1985 Israeli elections and are found to produce seat distributions falling between those produced by plurality and approval voting rules. Since plurality systems tend to overdistribute seats to leading parties and approval schemes tend to underdistribute to these parties, these other methods deserve additional consideration as possibly improving the link between voter preference and representative outcome.

The major strength of this volume is that it combines abstract theoretical results with a wide range of empirical tests including computer simulations, laboratory experiments, and field experiments. The variety of presentations provide some material that should appeal to a broad audience in social choice. Two factors, however, limit the book's usefulness. First, there is little to tie the three parts of the book together other than the fact that each deals with a topic somewhat overlooked in most social choice analysis. The author does not provide a synthesis to suggest what the impact of this book's contribution might be for reevaluating the relevance of social choice approaches. The result is that the three parts do not add up to a single coherent volume. This problem is exacerbated by the fact that several of the individual chapters are based on collaborative work and the author has failed to edit sufficiently to remove style differences and inconsistencies among the treatments. Second, the book is based on work that almost without exception has appeared in print in other venues. This would not be troubling if the synthesis in this volume resulted in a product exceeding the sum of its parts. However, often the task of linking the results together is left to the reader; hence, packaging these papers in a single volume does not have the impact it should have.

Indiana University ROBERTA Q. HERZBERG

Nietzsche's New Seas: Explorations in Philosophy, Aesthetics, and Politics. Edited by Michael Allen Gillespie and Tracy B. Strong. Chicago: University of Chicago Press, 1988. 240p. $24.95 cloth, 13.95 paper.

Nietzsche and Political Thought. By Mark Warren. Cambridge: Massachusetts Institute of Technology Press, 1988. 311p. $30.00 cloth, $12.50 paper.

"To improve one's style—means to improve one's thoughts and nothing else!" Nietzsche wrote in *The Wanderer and His Shadow*, priding himself on being both thinker and literary stylist. His goal, so far as it may be summarily defined, was to investigate what the life of a modern "philosopher–artist" entailed and to convince others that living this life was, at the least, highly desirable. Nietzsche's aphorism concludes, "If you do not straightaway agree with this it will be impossible to convince you of it." Here we discover Nietzsche's aristocratic bent, often reviled but also intrinsic to his thought. Nietzsche wrote for a restricted audience because he believed there were few with ears to hear his words. His readers already would have to be philosopher–artists in spirit to understand and appreciate his literary–philosophic creations. *Nietzsche's New Seas* attempts to address the nexus between Nietzsche's philosophy and his art in a manner consistent with Nietzsche's own understanding of the importance of style. It is a stimulating collection of essays. However, certain contributors neglect the import of the first part of Nietzsche's aphorism. Great style is always only the child of great thought. The two may become inseparable in practice, but the prerogatives of parentage should be maintained. Very different in approach and intent, *Nietzsche and Political Thought* incisively identifies, and then purges, those aspects of Nietzsche that are socially exclusionary. While not unaware of the place of style, the book is dismissive of its centrality and unconcerned with its function in the systematic quest for a Nietzsche that accords with its author's politics. Here, the importance of the second half of the aphorism is neglected. For one may legitimately separate Nietzsche's philosophy from his politics; but it is Nietzsche's philosophy, not his politics, that is selective of its audience and intrinsic to his art.

While the nine essays of *Nietzsche's New Seas* are diverse in their approaches, a central concern is to be gleaned from all. What is new both in Nietzsche's philosophical navigations and in these attempts to navigate around Nietzsche's philosophy is a sensitivity to form. The form of interpretation practiced in this collection is different from all forerunners, the editors hold, because "in this new incarnation, Nietzsche no longer appears primarily as the prophet and purveyor of nihilism but as the thinker who marks a kind of ending to, or at least a rift in, the continuity of the West" (p. 1). In what might appear as a kind of rift in the editors' opinions, however, we learn a few pages later that the new approach represented by these essays is nonetheless predicated on the "centrality of nihilism" to Nietzsche's thought (p. 5). The confusion is resolved by viewing Nietzsche's radical break in and break-up of Western culture as the product of his pronouncements on nihilism. Unlike previous philosophers, Nietzsche did not ignore or reject nihilism. Instead, he explored its historical depth and celebrated its "active" form with music and poetry. The celebration of the poetic and musical Nietzsche in these essays, therefore, is also a celebration of that "active nihilism" Nietzsche identified with the death of God and the subsequent demise of God's shadows, metaphysical truths. Once philosophy no longer considers itself metaphysical and foundational, it becomes—and perhaps must become—aesthetic.

To my mind, the highlights of this volume are essays by Karsten Harries, Sarah Kofman, and Tracy Strong. Strong persuasively argues that Nietzsche's concern is with aesthetic self-transformation, not political domination. Nietzsche's fear that he had reached the limits of

what could be achieved through aesthetics, Strong speculates, pushed him into the realm of explicitly political reflections on domination and, perhaps, eventually pushed him beyond the realm of communicative thought. Kofman's essay also attempts to redeem Nietzsche, taking on his misogynist reputation. Nietzsche's ambivalence to women is well demonstrated by Kofman, as is the philosophical importance and literary richness of his notion of the feminine. Kofman asks, in conclusion, if Nietzsche's polemics "are not symptomatic of a deep love of women, all of whom had abandoned him when they might have served him as a lightning rod . . . or even an umbrella?" (p. 199). As insightful as Kofman's essay proves (and his ironic humor notwithstanding), one must question whether trading Nietzsche's distasteful but revealing chauvinist vitriol for an insidious partriarchal gratitude constitutes a worthwhile transaction. In "The Philosopher at Sea" Harries discerningly explores Nietzsche aphoristic literary style, finding its fractures conducive to Nietzsche's philosophic purpose. By way of his artful lure Nietzsche kidnaps his readers for a philosophic voyage in uncharted and choppy waters.

Michael Gillespie's essay, on the other hand, attempts to demonstrate more method to the master's madness, disputing the belief that Nietzsche's philosophy is fragmentary. The claim is that much of Nietzsche's later work (explicitly, the *Twilight of the Idols*) is systematically structured according to the form of the musical sonata, with literary and philosophical equivalents of cadences, keys and harmonic changes. Gillespie's thesis is highly original but quite unfounded. The analysis opens with the claim that the preface to *Twilight of the Idols* refers to the time signature of a sonata (3/4 time, or triple meter): "The preface is divided into two sections separated by a line of Latin. There are 135 words above and 180 words below the Latin line, which constitutes an exact proportion of 3/4" (p. 124). The author notes that Nietzsche's musical intent, here revealed through a numerological puzzle, "might seem far-fetched" (p. 148). Indeed, its dubiousness is certified once one notices that Gillespie miscounted his words, there being 181 not 180 below the Latin line. Failing the possibility that Nietzsche also miscounted, Gillespie's "musical politics" proves rather offbeat. What is salvageable from the essay is the claim that *Twilight of the Idols*, like much of Nietzsche's work, has a discernible structure. That this architectonic quality is loosley shared with musical sonatas (and, one might add, with most books, essays, lectures, films and various other forms of musical composition) is unsurprising.

Contrary to *Nietzsche's New Seas*, Warren's *Nietzsche and Political Thought* is premised on the assumption that Nietzsche's style is not a sufficient focus of study—"nor for that matter is it even a good way of entering Nietzsche's thought" (p. xii). The book is a finely crafted argument detailing how Nietzsche, despite his politics of "neoaristocratic conservatism," may be employed for the purposes of constructing a postmodern (read non-metaphysically-grounded) politics of democratic egalitarianism, reciprocity, and social care. The key to squaring this circle is the rejection of Nietzsche's "premodern" socioeconomic assumptions and remaining metaphysical beliefs that caused him to "read domination back into his philosophy of power in ways that violated the internal integrity of his critical, postmodern insights" (p. 3). This distillation of Nietzsche is then combined with the reworking of his thought to the point where Nietzsche's central concern is determined to be the development of human agency, inherently a political category defined as the capacity to make decisions, initiate projects, and enter into reciprocal obligations.

Nietzsche announced that he sought a "Caesar with Christ's soul." Warren seeks a Nietzsche with Marx's soul. This Nietzsche displays an overriding concern for the problems of social injustice, violence, and development. He is, above all, a social critic. Nietzsche the poet of tragic heroism and philosopher of radical individualism, who plumbed the depths of his soul that he might raise himself above and beyond social norms and constraints, is scarcely to be found in these pages. The Nietzsche that emerges from Warren's book, then, is indisputably a retreaded one. In itself, this is an unavoidable result of any imaginative interpretation; and Warren is rightfully unapologetic for his project. But the question remains as to whether the retreading was so extensive a piece of work as to make the substratum unrecognizable, or whether the core is so thin that despite the craftsmanship displayed in its renovation the product is likely to fly apart when put to the test. Warren's effort is laudable for its rigor and scope. Yet I believe he remains unsuccessful in the very worthwhile task he set himself, namely, "to show how one might choose the philosophical Nietzsche while excluding his politics, but to do so without eclecticism—that is, without fragmenting the internal necessities of his thinking" (p. 208).

University of Florida LESLIE PAUL THIELE

Theory in Practice: Tocqueville's New Science of Politics. By Saguiv A. Hadari. Stanford: Stanford University Press, 1989. 182p. $27.50.

Tocqueville continues to attract and to inform, whether as historian and (sympathetic) critic of democracy or prophet of a "new world." His descriptions, insights, and predictions are like nuggets—promises of even more substantial findings below the surface. *Theory in Practice* plumbs the depths in search of the core.

That yet unearthed treasure is Toqueville's methodology. In Tocqueville we have an illustration of social theory not only rigorous, impartial, and therefore scientific but contextually appropriate, interpretive, and practical. As Saguiv Hadari portrays it, Tocqueville is able to achieve in theory what modernity is unable to achieve politically—the best of both worlds. Describing it in one place as "successful eclecticism," Hadari concludes that "Tocqueville's approach qualifies as both 'economic' and 'sociological', and fuses together 'classical' and 'modern' perspectives" (p. 5). In Tocqueville, then, we have a proven alternative, an answer to the multiple, often incommensurable social theories of today.

This is not to say, however, that Tocqueville necessarily rejects—or that he simply transcends—contemporary dichotomies. (Hadari lists, among these, nomological vs. particular, macro vs. micro, formal vs. interpretive, and objective vs. subjective approaches.) Hadari argues something quite different and contentious, namely, that Tocqueville traverses these dichotomies while still (in some sense) recognizing them, without obliterating them.

Hadari works from the position (also contentious) that Tocqueville's *Recollections, The Old Regime and the French*

Revolution, and *Democracy in America* form and reflect an integrated whole. He finds (in part 1 of the *Recollections*) Tocqueville to be "grounded in abstract, nomological models of behavior" (p. 13), including those of a "game-theoretic" and/or rational choice nature. In his part 2, however, where Tocqueville's study of the French Revolution is emphasized, Tocqueville is revealed as the theorist of "self-understandings." Wishing "to penetrate to the heart of this old regime its ideas, its passions, its prejudices, its practices," Tocqueville's recourse to hermeneutics is unavoidable and the "key to the originality and insightfulness of *The Old Regime and the French Revolution*" (p. 65).

Democracy in America (part 3) reveals Tocqueville as normative, that is to say, striving to provide "practical orientation about what is right . . . without relinquishing . . . the rigor of scientific knowledge" (p. 116). Here Hadari is at pains to portray Tocqueville's approach as one where "'objectivity' does not mean value-free, strictly descriptive analysis" and "normative" does not imply "a lapse into merely subjective evaluations" (p. 116).

Theory in Practice presents a close and detailed argument. It is advised that the reader have a working knowledge of Tocqueville's major works and of the methodological issues of contemporary social theory.

Emporia State University THOMAS CLAY ARNOLD

Property Rights and Poverty: Political Argument in Britain, 1605–1834. By Thomas A. Horne. Chapel Hill: University of North Carolina Press, 1990. 296p. $34.95.
The Realm of Rights. By Judith Jarvis Thomson. Cambridge: Harvard University Press, 1990. 383p. $35.00.

Ours is a rights-centered culture, and our political dialogue is largely framed in the language of rights. But what it means to have a right is not always clear. Clarity about the language of rights is important because theories of rights generally impose the duty not to interfere with the rights of others. For some, questions about what constitutes a right and who is a rights carrier are easily answered. Others, however, find a lack of continuity in the way we assign these terms. One route the controversy has taken pits claims for human dignity against claims for the unrestricted use of private property.

In very different ways, Thomas Horne and Judith Thomson prove to be important and lively contributors to the debate about the status of rights. Horne offers a strong and enlightening discussion of the rights and obligations connected with property ownership in early British theory, and Thomson builds on Hohfeld's contributions to solve some vexing puzzles about rights. Each clears away cumbersome impediments to clarity and invites readers to think freshly about the language of rights.

Thomson argues that one class of rights claims stems from a person's humanity and creates a strong collateral harm principle that cannot be lightly breached. We can expect rights to be respected (on her account) because human beings can behave according to moral laws, and, that being the case, others have claims against us to respect their rights. A different class of rights claims stems from social practices and law and is derived from promising and consenting in particular contexts. By this line of reasoning, private property rights claims are conditional on a legitimate legal system and cannot be considered a natural right.

For Thomson, rights are best understood as clusters of rights, privileges, and duties. What emerges is not a system of rights where one cluster invariably trumps another but a discussion about the *Realm of Rights* where different people, given their varying social circumstances, produce different clusters of rights, privileges, and duties. The array of rights that one person carries may not be the precise array of another person; and the rights a particular person holds at one time may not necessarily be the same rights the person will hold at a later time. Nevertheless, for Thomson, this ought not be taken as a denial of the first category of rights (resembling natural rights), which is something that cannot be altered at its core even if its expression may vary in practice.

The strength of Thomson's work is to teach us to think analytically about rights. At the same time, though, this approach only has a limited political impact. The legal and political systems that play such a large role in her second form of rights often reflect configurations of power that serve some individuals while ignoring the claims of others. But rights are thought to be separate from power, not dependent on it—a subject largely neglected by Thomson.

Horne is primarily concerned with Thomson's second category of rights. He shows how classical liberal theories on property contained both an inclusive and exclusive (or common and individual) dimension and how several writers attempted to accommodate both aspects. Beginning with Grotius and Pufendorf, and continuing through Locke, Smith, Bentham, and Paine, Horne demonstrates the diversities and continuities of property rights in liberal theory and argues that the modern welfare state has its roots in early liberalism.

Some of the authors Horne surveys hold that in cases of individual necessity, the poor are given valid claims on the property of others for their own survival. Sometimes, the right of survival by the propertyless is transformed into a duty on the propertied to pay taxes that are used by magistrates to assist the needy. For others, the right of necessity gave way to a weak duty of charity by the propertied to the poor.

Many natural rights theories that emphasize the exclusive use of property simultaneously forge a strong argument for equality—and, according to Horne, for inclusive conceptions of property and even the welfare state. If people are credited with a natural right to rebel when their right to liberty is denied, what strong argument could deny them the right to the property of others when their own survival is at stake? In a similar vein, Horne also shows that early liberal theories that relied on individual labor to justify an exclusive rights claim to private property were converted by some later liberals to arguments that vast inherited holdings were divorced from labor and therefore subject to working-class claims for a greater share of national wealth. The authors most likely to avoid undermining a strong exclusive view of property were writers like Filmer and Burke, who rejected the egalitarian and rationalistic assumptions of liberalism.

Horne also reminds us that early theories of property commonly carried strong admonitions for moderation and warned about excessive ambition and avarice. However, such virtue is self-enforced. Just as charity is a highly individualistic way of addressing poverty, so moderation is a private disposition, not an institutional response to unlimited acquisition.

Horne's work is a remarkably compelling examination

of liberal theory's many different conceptions of private property and of the duties of property holders to the impoverished. While he sometimes pushes his argument about the implication of charity on future liberal thinking too far, Horne shows that a simple reading of liberalism as a theory of acquisition and possession misses much more than it captures. He also demonstrates that many of the elements in the liberal–communitarian debate blur the recurring feature of most liberalism that property rights are encumbered and that property holders owe some (if only a scanty) responsibility to those who are the least well off.

University of Maryland RONALD J. TERCHEK

Situation and Human Existence: Freedom, Subjectivity and Society. By Sonia Kruks. New York: Routledge, 1990. 215p. $15.95 paper.

John Rawls tells us that a fair solution to the problem of justice must be acceptable to "free and rational persons." Michael Sandel responds that such persons do not exist except as members of a particular community. And a diverse group of postmoderns (e.g., Michael Foucault, Jacques Derrida, Pierre Bourdieu, Jacques Lacan, and Francois Lyotard) agree that free and rational persons do not exist anywhere and never will. Sonia Kruks takes up this issue, arguing quite correctly that it will not be resolved by assertion but only by the careful study of how "situated" men and women truly are. To what degree are we prisoners of the situations we find ourselves in—situations that range from being the possessor of a body upon which we depend, to being a member of a family, to belonging to a social class, to being a citizen of a nation? Surely this is a terribly difficult and important question—that is why it is so often dealt with by fiat.

To answer this question, Kruks turns to a diverse group of French existentialists (Gabriel Marcel, Jean-Paul Sartre, Simon de Beauvoir, and Maurice Merleau-Ponty), devoting a chapter to each, with the exception of Sartre, who gets one for the early period of *Being and Nothingness* and another for the later period of *Critique of Dialectical Reason* (which, Kruks argues, in effect rejects Sartre's earlier claims regarding absolute freedom). Hers is in the main a fruitful quest and a pleasant change from the faddish search for the latest and most extreme French thinker of the month. For what Kruks shows is that in spite of the considerable flaws of each, all (with the exception of the early Sartre) struggle with the relationship between situation and freedom, trying to come to terms with what Merleau-Ponty calls the "impure subject"; that is, the authors struggle with a concept of subjectivity that is neither the constituting consciousness of traditional Western philosophy nor the constituted consciousness of postmodernism. Such a concept conceives of the self as situated, embodied, intersubjective, and practical (while not reducible to any of these).

Kruks carefully and critically explicates the position of each author. In fact, she devotes so much attention to this task that the book risks becoming a sort of Cliffs Notes for the very advanced. This is particularly the case with her treatment of Sartre, since so much of what she rightly criticizes about his project has been noted by many others. It is less of a problem with her study of Marcel and Beauvoir, primarily because they have received less critical attention than they deserve, so that a summary of what they have received is more rewarding. So often, I had the feeling that Kruks knows more than she is telling. This is not the worst experience when reading a book—and certainly better than its opposite.

If Kruks's book risks becoming strictly a critical explication, it is saved from this fate by a short but dynamite conclusion, where she suggests that those who assert either the absolute freedom of the subject (early Sartre), or the subject's absolute unfreedom (Foucault) are holding to inherently unstable positions, at least if the holder of such a position also wishes to be a social critic. Foucault, for example, does not so much soften his antihumanism in several later works as simply revert "to the classical conception of the autonomous subject" (p. 188). How much more fruitful it is, concludes Kruks, to accept and struggle with the idea of the impure subject—neither wholly constituted nor entirely free but always moving between the two, depending among other things, on the subject's actual historical situation.

The existentialists' willingness to struggle with the issue of the situated self, rejecting the charms of extreme solutions and simplifications, seems to me to be their great contribution. Where they fail (the early Sartre most of all and Beauvoir probably the least) is in their tendency to "go ontological"—to write about the essence of being as though they were talking about real human beings. Consider the claim that being realizes itself in constituting the conditions under which it might come to apprehend its own subjectivity. It is a claim that I just made up and that (as far as I know) means nothing. Yet precisely because it is a claim about nothing at all, this is easily overlooked. Although Kruks knows this (I am sure that she would not put it this way), she sometimes lets her authors get away with too many statements of this kind. In the end, it is talking about the real lives of real men and women in real situations that keeps us honest, that is, keeps us involved with the reality of impure subjects—the only subjects there are.

University of Maryland, College Park C. FRED ALFORD

Rawls: "A Theory of Justice" and Its Critics. By Chandran Kukathas and Philip Pettit. Stanford: Stanford University Press, 1990. 169p. $22.50 cloth, $8.95 paper.

Kukathas and Pettit claim that justice as fairness has lost its nerve. Although the contractarianism of Rawls's *Theory of Justice* is puzzling (it is not as indispensable as some suppose, since once "the constraints of right are put in play, then most candidate principles for determining the basic structure of society must be judged to be infeasible"), still, "not only does the contractual device help us to identify what we would choose among basic structures—feasible and infeasible—if we were impartial," but it also dramatizes "the sort of thing that we should be looking for when we seek out the best basic structure for a society" (pp. 63–64). And that project at least involved argument—not merely accommodation and conciliation, which is all there is to the stability-minded "political liberalism" of the recent Rawls.

Strangely (given their mission of reconstructing Rawls's degeneration), the authors do not mention the Kantian interpretation of justice as fairness in *The Theory of Justice;* and the entire issue of Kantian constructivism is treated

so gingerly (in the last chapter) that one would never guess from reading this book how or why the Kantian theme has loomed so large in Rawls's work throughout. This omission is remarkable not least because the book goes on to consider various critiques of Rawls's Kantianism at some length and, in a confused, ambivalent way, to lament his supposed abandonment of it.

Against Nozick, the authors rightly stress the importance of the basic structure and proceduralism, though they do not convey Rawls's own view of his approach as "quasi" or "pure-adjusted" proceduralism. Indeed, on the issue of what a just Rawlsian state should look like, they make the common blunder of suggesting that although "Rawls does not put it in these terms, it looks as if economic justice is given institutional expression by the organs of a welfare state" (p. 51). In fact, Rawls sharply denies that his theory is a defense of the welfare state. As the French edition of *The Theory of Justice* shows, the real contenders for just economic institutions are democratic socialism and "property-owning" democracy—not surprising, given Rawls's emphasis on production.

In reply to the communitarian charge that justice as fairness represents abstract, Kantian universalism, they go too far in claiming that the theory is bound to a particular historical context, namely, the U.S. democratic tradition: "In his Dewey Lectures (1980) Rawls explicitly acknowledges that his concern is not to supply a universal standard of justice but to discover those moral principles which might best serve his own society, with all its particular concerns" (p. 107). But crucially, Rawls's claim has only been that his theory *at least* applies to modern constitutional democracies; he simply wants to bracket many of the historical questions. (This agnosticism also figures in *The Theory of Justice* to a degree, though the issue is complex.) Still, he does want to claim that justice as fairness is "appropriately universal." The depth of its ideals is such that it is more than a public conception for the United States. It applies to many modern democratic states and is capable of being extended into a conception of international justice.

This is important because the confusions of these earlier chapters are aggravated in the final chapter, where the authors claim that Rawls's post-*Theory* work shifted to a more Kantian view (because of the more explicit appeal to a Kantian ideal of the person, supposedly at its peak in the Dewey Lectures)—followed, from about 1982 on, by an unfortunate shift to a more Hegelian or pragmatist view. The latter phase establishes the principles of justice "not by challenging and rejecting competing comprehensive conceptions but by trying, as far as is possible, to transcend and *accommodate* them. The point . . . is not to find the truth about the principles of public morality, but to ensure social unity and stability in long-run equilibrium by forging a stable agreement among the various moral, religious and other comprehensive philosophies" (p. 148). For Kukathas and Pettit, the idea that justice as fairness is a practical political view (one expressly framed for the basic structure of society and drawn from the public political culture, rather than from any particular comprehensive moral doctrine) marks the feasibility concern run riot. Rawls's contractarianism is "now re-interpreted . . . as a solution to the problem of finding a suitably feasible conception" (p. 143). The "constraints on rational choice" simply reflect the latent ideals of U.S. society; and the principles chosen in the original position "amount to a conception of justice which is stable because they are principles which would be chosen by that society. These principles are therefore feasible" (p. 143). Whether the principles of justice are "desirable" is now "clearly of subordinate importance" (p. 143). Rawls, they claim, has despaired of "reasoned debate."

Does this mean reasoned debate in one country, an all-American Kant? Given that the rot supposedly set in around 1982 (well after the Dewey Lectures), this would seem like the only conclusion consistent with the book's reading of the earlier Rawls unless the authors also mean to suggest that the relativism of the Dewey Lectures (on their interpretation) is more akin to the practical and political interpretation than to *The Theory of Justice*. They seem to think the opposite; but where explanation is needed, there is only muddle—a cryptic notice that it is "a precondition of debate" that the debaters "each view themselves at some level as having the weight of reason on their side" (p. 150). That Kukathas and Pettit present only a caricature of Rawls's political liberalism will not surprise anyone who has suffered through the extraordinary floundering of the final chapter of this work. Having announced that Rawls went from a Kantian to a Hegelian phase, the authors simply fail coherently to explain why he did so, why the two principles have remained much the same throughout, why Rawls should continue to use the label *Kantian constructivism*, and so on. Hampered by a misleading interpretive scheme and a feeble grasp of Kantian constructivism, they splash around terms like *Kantianism*, *Hegelianism*, and *pragmatism* with dizzying imprecision, substituting italicization for explanation. The very nature of Kantian constructivism is left a mystery. Rawls's post-*Theory* approach is assimilated to everything from Hobbesian peace seeking to Rorty's pragmatism; but the basic contrast with rational intuitionism is not discussed at all!

If Rawls were simply concerned with underwriting U.S. doctrinal conformity (a job that hardly merits a night watchman), his theory would certainly be in very desperate need of emboldening. However, there is a potential radicalism in Rawls's attempt to use the ideal of the free and equal citizen, at home in such areas as freedom of conscience, to sort out problems of distributive justice (see J. Cohen, "Democratic Equality," *Ethics* 99 [1989]). Kukathas and Pettit consistently ignore or distort this side of justice as fairness. Too many crucial issues (Kantian constructivism, antirealism, etc.) were never on their agenda.

University of Chicago BART SCHULTZ

Gender, Identity, and the Production of Meaning. By Tamsin E. Lorraine. Boulder: Westview, 1990. 227p. $38.50 cloth, $14.95 paper.

Feminism without Illusions: A Critique of Individualism. By Elizabeth Fox-Genovese. Chapel Hill: University of North Carolina Press, 1991. 347p. $24.95.

Feminist Interpretations and Political Theory. Edited by Mary Lyndon Shanley and Carole Pateman. University Park, Pennsylvania State University Press, 1991. 288p. $35.00 cloth, $12.95 paper.

Of the three books under review here, the volume edited by Shanley and Pateman is the most accessible for the nonspecialist. It draws together a variety of excellent papers (many previously published in disparate places),

each of which shows how new light can be shed on the major classical and contemporary texts of political theory by asking questions about women, gender, sexuality, and the family and how these intersect with such perennial issues as the nature of citizenship, the public and private realms, equality and justice. The book is arranged chronologically by theorist, beginning with Plato and Aristotle and ending with Rawls, Foucault, Arendt, and Habermas. Sandwiched between are essays on Hobbes, Locke, Rousseau, Hegel, Marx, and J. S. Mill, as well as Mary Wollstonecraft and Simone de Beauvoir. All the essays are clearly written, insightful, and of interest to generalist and specialist alike. One regrettable gap, however, is the absence of a treatment of women in the Christian tradition. An examination of ecclesiastical misogyny and its place in both early Christian and medieval thought would have more fully completed the volume as a feminist companion to the teaching of the entire historical canon.

The inclusion of essays on Wollstonecraft and Beauvoir is rightly defended by the editors on the grounds that they are the authors of important works in the history of political thought and ought to be considered part of the canon (p. 2). The case for Wollstonecraft's inclusion is made well by Moira Gatens, who explores the ambiguities that the liberal paradigm presents for women through a reading of Wollstonecraft's texts. However, Beauvoir is less well served by Elizabeth V. Spelman's treatment. Although Spelman's critique of Beauvoir for failing to acknowledge the significance of differences between women is important and skillfully argued, it does not function well in this collection as a means of integrating Beauvoir into the canon. The editors could, in my view, also have been a little bolder in integrating more women writers into the canon. For example, an essay on Olympe de Gouges's intriguing Declaration of the Rights of Woman or a treatment of Harriet Taylor Mill would not have been out of place here. But such minor criticisms aside, this book usefully puts together a set of papers that are both accessible and of uniformly high quality.

Tamsin Lorraine's *Gender, Identity, and the Production of Meaning* also bears in potentially important ways on the reading of political theory texts, although this is not her primary focus. Lorraine's concern is with the place of gender-differentiation in the formation of the self, particularly of the authorial self. Rather intriguingly, she draws on both Lacanian and object-relations revisions of Freud to develop a psychoanalytic account of a subject, or self, who emerges from social codes and constraints but only through what she calls *self-constituting activity*, or *self-strategies* that also involve intentionality and choice. The masculine self, in its pure form (to which, of course, no empirical individual ever wholly conforms), corresponds to full initiation into Lacan's symbolic order. The pure masculine is "changeless, eternal, harmonic order . . . the real rendered orderly" (p. 184) and masculine self-strategies involve constituting the self by clear separation from, and opposition to, the other. By contrast, "the purely and eternally feminine is chaotic flux" (p. 183); and the feminine strategy for self-constitution is one of connectedness, even fusion, with the other.

Too much of Lorraine's book is taken up with giving an extensive summary of the psychoanalytic literature on which she draws. The book becomes most interesting in its penultimate section. Here, she takes up the question of reading texts through the gendered self-strategies of their authors. In insightful readings of works by Kierkegaard, Nietzsche, and Sartre, she shows how gaps, incoherences, and inconsistencies in philosophic texts can be read as symptoms occasioned by the inability of their authors to renounce the male self-strategies to which, with varying degrees of intensity, they hold.

Lorraine's psychoanalytic accounts of self-constitution undoubtedly help to explicate the works of individual authors, but they cannot form the basis for an adequate account of selves in social and political life. In her conclusion, Lorraine tries to build an argument for feminist politics from her account of gendered self-strategies, but she ends up in rather naive exhortation. We must, she asserts, develop a "new self" incorporating both feminine and masculine self-strategies (opposition and connectedness). Moreover, such a new self could also be the basis for a "new world view"—for a "world community," even. But how such new selves are to come into existence in a world of already-gendered power relations is not discussed. Nor are the difficulties endemic in modeling whole societies (let alone a world community) on the dyadic psychoanalytic relation of self and other even acknowledged.

If Lorraine is insufficiently sensitive to the distinctions between selves and social wholes, the same cannot be said of Fox-Genovese. Indeed, the central theme of *Feminism without Illusions* is the tension between individualism and the ideal of community as it plays out today both in feminist theory and more generally in U.S. political and cultural life. In essays that range in their specific topics from concrete political issues (e.g., affirmative action, abortion, and pornography) to treatments of the impact of postmodernism on the history profession today or the debate about the canon and cultural diversity, Fox-Genovese's leitmotif remains the fraught nature of the relation between individualism and community. While some freedoms have indeed been won for all in the name of individualism, individualism's construction of the self as male and white has made it of ambiguous value to women, African Americans and other minorities. The way forward, she argues, must be to abandon a conception of atomistic, innate, individual right for "a view of individual right as derivative from collective life" (p. 244).

Fox-Genovese writes engagingly, with an ease and elegance born of her tremendous erudition about U.S. history and culture. She is at her best in pointing out the contradictions that bedevil U.S. feminism because of its failure consistently to escape from individualism. For example, she rightly points out, while feminists make much of women's "different voice" and their orientation to community and caring, they have defended access to abortion on wholly different (and, indeed, incompatible) grounds—as an issue of individual rights, grounded in a crudely possessive individualist conception of woman as the owner of her body.

Although critiques such as this are thought-provoking, there are also some serious difficulties with the book, especially if one asks what its positive implications might be. The two central concepts, individualism and community, are not explicated carefully enough. At first, individualism means "possessive individualism"—an atomistic individualism, which has grounded liberalism by asserting innate, personal rights as claims against society and the state. But as the book progresses other versions of individualism appear on the scene.

Epistemological positions that stress "difference" are a form of individualism. So, at heart, is postmodernism (surely news to its adherents!). Even conservatism (historically the defender of community against naked egotism) is today marked by individualism. Are all these forms of individualism essentially one and the same? Are they all equally problematic for women? I suspect not; but Fox-Genovese does not tell us.

Even more perplexing is the status of community for Fox-Genovese. Traditional theorists of community (such as Tönnies, whom she mentions repeatedly) stress its ascriptive qualities. Yet ascriptive communities have, she concedes, been highly oppressive of women. But if ascriptive communities cannot function today as mitigators of individualism, neither can voluntary communities; for, she alleges, conceptions of community based on a perceived common identity or commitment continue unacceptably "to ground community in individualism" (p. 40).

What Fox-Genovese finally points to as a possible basis for community is the existence of a U.S. national culture, grounded in the canon. Although she acknowledges that the canon needs some expanding to include previously excluded groups, she believes that it does fundamentally define a national community. But even if we accept this controversial claim, such a notion of cultural community can hardly provide an adequate basis on which to ground her political appeal that we should abandon individualist conceptions of rights for a view of individual rights as derived from collective life. In short, the book raises more questions than it answers—but at least they are serious questions.

Oberlin College SONIA KRUKS

The Meaning of Socialism. By Michael Luntley. LaSalle, IL: Open Court, 1990. 214p. $32.95 cloth, $14.95 paper.

The Concept of Socialist Law. By Christine Sypnowich. Oxford: Clarendon, 1990. 195p. $45.00.

The events since 1989 have thrown well-deserved and long-overdue doubt upon both the viability and moral defensibility of actually existing socialism. But few would argue that even the collapse of the Soviet system and its satellites has mortally damaged the idea of socialism even if the term itself is widely held in disapprobrium. When the Berlin Wall crumbled, East German citizens were found to want both the security of socialism and the affluence of the market, not merely "freedom." Scandinavian or West European–style social democracy—not U.S. or Chilean capitalism—remains the preferred ideal to many formerly socialist lands. The principles of socialism thus retain a considerable claim on the attention of modern political theorists. Michael Luntley's aim is accordingly not to vindicate specific socialist governments but to offer a philosophical justification for interfering with individual choices for the overall benefit of society. Socialism is here, therefore, a higher form of morality and, particularly, a vision of the good life that is independent of individual preferences at any given time. Luntley's chief targets in this cogent, persuasive, and admirably lucid book are those liberals, (of late, most notably, Rawls and Nozick) to whom the good can only be defined in terms of individual preferences (a view that might be termed "market positivism"). Instead, he asserts that "primary goods," such as education, exist and should be made codeterminate with citizenship. These goods, in turn, are defined and protected by communities and non-market-oriented groups, whose aims and activities are usually eroded by an overemphasis on market freedom. But Luntley also contends that socialists have stressed a number of issues (e.g., class, equality, justice, and rights) to the exclusion of a concern with community. He aims to please no particular constituency. Socialists will be troubled by his reluctance to concede public ownership as central to their vision, and puzzled by his view that small businesses might accord more fully with socialist aims. (But they can learn much from this critique—so appropriately the product of the rule of the shopkeeper's daughter from Grantham.) Liberals will probably not accept his assault on entitlement theories and, with their opponents, will doubt whether Luntley's claim of the good life divorced from public opinion and market research can be substantiated. Yet Luntley is absolutely right to postulate the problem of non-market-guided ideals or goods as central to the question of what the public interest is. A clean environment and limitation on population growth, for example (which some would argue now take precedence even over the issue of social justice), are two such elements of the good life that the market cannot engender and that demand political action whether the majority can be persuaded of its relevance, timeliness and cost or not. Luntley's defense of interference is thus, as a restatement of the old juxtaposition of negative to positive liberty, relevant to any debate about the ends of society; and his is correspondingly an extremely useful starting point for discussions about first principles.

For all its sense of embodying a higher form of morality, socialism (especially Marxism) nonetheless has never been entirely sure whether its own universalistic claims did not supersede all previously class-bound morality and whether, in turn, the struggle to usher in the new social order did not justify measures in the name of the largest class that most previous moral theories condemned as repugnant. Luntley denies that assessing the components of the good society means forcing any to be free. But how are minority rights to be protected, and under what criteria of general good ought they to be suppressed? This has been the problem of socialist law: if all systems of law are class-derived—indeed, if law itself is a function of class society doomed to disappear with classlessness—what is the status of a proletarian legal system once the new order is achieved, and what should its relation be to questions of *bourgeois justice*, such as the death penalty? Christine Sypnowich recognizes that most socialists, for reasons linked to antinomianism, class analysis, and conceptions of primitive communalism and higher forms of fraternity, have dismissed bourgeois justice out of hand. With this has been ejected much of the language of noneconomic rights, the notion of a rule of law removed from the context of class dictatorship, the separation of executive from judiciary, and so on. Following the lead of Tom Campbell (*The Left and Rights 1983*) and others, Sypnowich contends that this dismissal of the centrality of law to modern society ignores the fact that the substantive justice sought by socialists cannot be achieved without a framework of legal institutions to arbitrate between public and private claims. Instead of "withering away," law would thus sprout new forms in response to new

socialist theories of coercion, justice, and class power. (Sypnowich simply assumes that the new society has abolished private ownership of the means of production.)

After reviewing the sources and components of the Marxian conception of the supersession of law and Soviet debates over aspects of the theory, Sypnowich examines current debates among natural law theorists, legal positivists, and others about the elements of legal conduct and their relation to morality. She also gives a good summary of the evolution of Soviet legal practice and then concentrates on four troublesome areas: (1) freedom and the rule of law (where the threat posed by capitalism to the rule of law is central), (2) human rights and political reform (arguing for a socialist conception of rights based on the idea of human dignity), (3) rights in relation to altruism, and (4) the obligation to obey socialist law. This is much the best account available of the *possibility* of a socialist legal system vastly different from that usually associated with the term. Like Michael Luntley, Sypnowich anticipates an era of political and legal reform in which much old dogma will be abandoned but new ways of establishing a higher form of social justice still informed by classical socialist theory will be attempted. Sypnowich's book might have benefited from a starting point accepting pluralistic property relations as an integral part of socialism, like Luntley's; for property disputes are at the core of every legal system and would be under her own version of superior socialist legality, and all forms of future socialism are bound to be considerably more diverse in this respect than existing socialism ever was. Nonetheless, both of these authors help us to recognize that the debate about socialism, while it may assume many new forms, is hardly yet laid to rest.

University of London GREGORY CLAEYS

Private Property and the Limits of American Constitutionalism: The Madisonian Framework and Its Legacy. By Jennifer Nedelsky. Chicago: University of Chicago Press, 1991. 343p. $29.95.

Property and the Politics of Entitlement. By John Brigham. Philadelphia: Temple University Press, 1990. 223p. $39.95.

These are two books about the *new property*—a concept of property that goes well beyond the traditional view (as reflected in constitutional provisions) to embrace sources of government largess or entitlements. Both authors support this broadening of the concept of property to include procedural protections to propertylike transfers. Jennifer Nedelsky argues that private property has shaped the structure of the U.S. political system and the continuing tension between popular democratic government and the security of property. She claims that the constitutional focus on property has limited the strengths of the U.S. political system and has provided the source of its major weaknesses. These weaknesses lie in the failure of the system to realize its democratic potential by unequally allocating political power and wealth. This inequality was enshrined within the Constitution in an effort by property holders to limit the ability of the majority to use the power of the state to redistribute. Nedelsky claims, however, that the concern to protect private property rights distorted both legal and economic institutions and laid the framework for the current conflict between the traditional rights of property and equality within the modern welfare state. She believes that there is a need to redefine the scope and purpose of the state to accomplish democratic objectives.

To develop the arguments in her book, Jennifer Nedelsky focuses on the arguments of three critical framers of the Constitution: James Madison, Gouverneur Morris, and James Wilson. These people, she argues, encompassed the spectrum of the views on property and democratic participation at the constitutional convention. This discussion covers chapters 1–5, and chapter 6 develops some of the legacies and conflicts that Nedelsky believes resulted from constitutional protections of private property rights.

John Brigham also focuses on the legacies of constitutional guarantees of private property although he is less concerned about the origins of these practices and is more concerned about the debate over redistribution. He calls for an extension of the concept of property to include entitlements, which he defines as transfers that individuals receive, and form expectations over, as rightfully and legally theirs.

To develop his case for viewing statutory entitlements as property, Brigham examines various case studies of land policy, especially zoning issues and the protection of the California coast and tax breaks for environmental uses of the land. These are enlightened actions, he believes, that reflect a more activist view of property. He also points to social security benefits as examples of entitlements that should have propertylike status.

Both Nedelsky and Brigham seek to democratize the notion of property, and it might be that the inclusion of transfers as "property" would lead to redistribution of wealth; but it is not obvious that a more equal distribution would result. The data I am aware of suggest that most transfers go to the middle class and the wealthy, not the poor. Do Nedelsky and Brigham want to enshrine as property the transfer payments received by the nation's peanut, sugarcane, and sugarbeet growers; corporate holders of federally subsidized water; ranchers who pay below-market grazing fees; or middle-class parents whose children attend state universities and pay tuition that covers only a fraction of the costs of their education? These transfers surely fit the criteria outlined by Brigham as propertylike entitlements. I doubt the poor would benefit from attaching more formal property rights to these payments; nor do I believe that these are untypical of the direction of transfer flows. It is also unclear whether constitutional protections of property are really the basis for the unequal distribution of wealth in the society. The little information that we have about wealth distribution in the Soviet Union and Eastern Europe suggests that substantial inequality has existed in those societies with major transfers to party officials and senior bureaucrats even though private property by and large did not exist. Wealth distributions are even more skewed in Third World countries where the protections of property rights vary enormously.

These points illustrate what is so unsatisfying about both books. Neither employs standard social science techniques, in which verifiable hypotheses are presented and evaluated systematically against available evidence. Although Nedelsky examines the positions of three major participants in constitutional debates regarding property protections, the link to current institu-

tions (or "distortions" of them) is not credibly made. Nowhere do Brigham or Nedelsky blend their work with the existing (and large) literature in economics and political science on the path and sources of wealth inequality. This would have been useful in developing their arguments regarding the link between property definitions and wealth distribution.

Even more disturbing is their failure to examine the implications for the level of wealth in the society of a redefinition of property rights. Within economics and political science there exists both a theoretical and empirical literature regarding the definition of property rights and incentives for economic activity, with which these authors show no acquaintance, much less appreciation. Nevertheless, research indicates that the incentives to engage in socially beneficial investments and other economic activities with delayed returns depend crucially upon the security of property rights. Sustained economic growth is rare both historically and currently. The failure of economic development to spread to most of the world's population is at least in part due to insecure or unstable conditions and the incentives such an environment provides for short-term and wasteful economic behavior. The principal source of this insecurity lies in weak property institutions and the arbitrary and frequent confiscation of property by the state at the behest of powerful groups. The broad redefinition of property rights to include entitlements outlined in the two books would remove restrictions on the power of the government to reassign rights in response to the emergence of new and influential interest groups in the United States. Since there is a dynamic process of interest group formation and influence, one can anticipate that over time new and different entitlements or demands for "property" would arise.

Following Brigham and Nedelsky, a democratic government should respond. This is not, however, the environment that will encourage long-term investment or other activities that bring advances in economic welfare. A redefinition of property rights might lead to a reallocation of wealth, though current transfers do not make this obvious. On the other hand, however, it seems quite likely that the new definition of property they call for would lead to a smaller level of wealth to reassigned. Lower levels of economic activity are unlikely to benefit the poor—presumably the people the modern welfare state is supposed to help.

University of Arizona GARY D. LIBECAP

Finding the Mean: Theory and Practice in Aristotelian Political Philosophy. By Stephen G. Salkever. Princeton: Princeton University Press, 1990. 287p. $35.00.

Aristotle on the Human Good. By Richard Kraut. Princeton: Princeton University Press, 1989, 379p. $37.50.

A major development within contemporary practical philosophy, parallel to the Kantian revival, has been the resurgence of interest in Aristotle. (While on balance disapproving, Judith Shklar has called him "the man of the hour.") The recovery of his thought has proceeded unevenly. Interest in his ethical works has been enormous (witness the lush profusion of competing interpretations) and has contributed to the revival of virtue ethics over against rule morality.

By contrast, the reappropriation of Aristotle's politics is less advanced. It is not hard to see why. His treatment of women and slaves seems at odds with modern egalitarianism; the priority he accords to politics seems hostile to both liberal privacy and liberal individualism; his understanding of political agency seems to rest on discredited metaphysical and biological premises.

The uneven development of contemporary Aristotelianism is affected, as well, by the academic division of labor. Departments of classics and philosophy have focused on the ethical works, both as centerpieces of the densely contested arena of classical scholarship and as counterpoints to Kantian and utilitarian moral theory. The *Politics* and *Rhetoric* have been largely consigned to political science departments, where until fairly recently they have received far less attention.

The books under review, each excellent and instructive in its own way, reflect this state of affairs. Richard Kraut is able to plunge in medias res, unapologetically offering his interpretation of Aristotle's conception of happiness and defending it against well-developed rival views. By contrast, Stephen Salkever is compelled to spend a great deal of energy on threshold questions, especially the question of the relevance and acceptability of Aristotle for contemporary political inquiry.

Kraut is quietly bold. Against prevailing inclusivist interpretations, he argues that Aristotle identifies happiness with a single rational activity—philosophical contemplation. The concluding sections of the *Nicomachean Ethics*, contrasting and rank-ordering politics and philosophy, do not contradict but, rather, complete, the discussions of human happiness and function with which the work begins. Kraut's Aristotle is quasi-Platonic (even Socratic), concerned with the conflict between philosophy and politics and determined to vindicate the claims of theoretical, over practical, reason. His Aristotle defends philosophy as requiring less "equipment" than does politics. Devoting one's life to contemplation diminishes life's vulnerability to fortune; the goodness of philosophy is not as fragile as fortune's.

This defense of theoretical reason does not, on Kraut's reading, undermine moral virtue. Philosophical activity is compatible with—indeed, requires—the practice of the moral virtues as understood within the practical realm. (This is the case in part because Aristotle subtly "intellectualizes" these virtues.) Moreover, Kraut argues at considerable length that Aristotle's ethical agent is not an egoist. Philosophers can, on occasion, rationally devote themselves to the well-being of family, friends, and fellow citizens at the expense of their own good.

Kraut's book is distinguished by its impeccable scholarship, careful argumentation, and respectful engagement with rival interpretations offered by Ackrill, Cooper, Hardy, Irwin, and Nussbaum (among others). While it will not resolve the debate over Aristotle's conception of *eudaimonia*, it moves that debate forward in ways that cannot be ignored. My one regret is that the index to this otherwise beautifully produced book is not detailed enough to be fully serviceable.

Salkever defends Aristotle's political philosophy as a distinctive middle way between foundationalism and conventionalism and between theories of mechanistic determinism and unrestricted freedom. His Aristotle views practical philosophy as the vehicle not for resolving political disputes but, rather, for clarifying the terms in which they must be conducted. At the heart of sound politics is well-developed practical reason, expressed in

joint deliberation sensitive to context and particularity. This conception of practical reason reflects an understanding of human biological specificity that is not (despite MacIntyre) metaphysical and hierarchical but, rather, empirical and inclusive. In its antiprecisionism, it emphasizes virtues over rules; in its focus on open deliberation, it is at least compatible with a deepened understanding of modern democracy.

While Salkever agrees in general terms with Kraut that Aristotle ultimately gives pride of place to philosophy over politics, he denies that this rank-ordering (or, for that matter, any other theoretical proposition) has direct consequences for political practice. The central practical function of theory is negative—to teach political actors that the mechanical application of general rules is bound to be inapt. Salkever's discussion of this thesis is subtle and provocative. Still, one wonders whether his interpretation does not overemphasize sensitivity to particulars at the expense of more general considerations, a worry heightened by his relative neglect of Aristotle's moderately precisionist discussion of distributive justice.

Salkever's book is not intended—and will not serve—as a comprehensive guide to Aristotle's political philosophy. It is, rather, a highly intelligent effort to situate that philosophy within contemporary debates in practical philosophy and social science and to argue that social inquiry carried out in a broadly Aristotelian spirit can reach insights not otherwise available. Taken as a plea to political scientists and supporters of liberal democracy to take Aristotle seriously, this book deserves respectful consideration.

University of Maryland, College Park WILLIAM A. GALSTON

The Genesis of the State. By Martin Sicker. New York: Praeger, 1990. 161p. $39.95.

The Changing Architecture of Politics: Structure, Agency, and the Future of the State. By Philip G. Cerny. London: Sage, 1990. 267p. $47.50 cloth, $18.95 paper.

Martin Sicker is a political scientist and also a practitioner (having served as a "senior executive in the U.S. government.") But *The Genesis of the State* has about it an air of the abstract and definitional. He poses questions that positively guarantee theoretical overreach, for example: "How did the state originate and what is the source of its authority?" (p. viii). Many of the greatest minds in political thought and historical study have taxed themselves to offer answers (or at least useful suggestions) in this matter. Sicker dispenses with the question of origins in 24 short pages. This is bound to be unsatisfactory, in large part because he has aimed too high and come up with too little.

Untouched by some of the more interesting debates in recent political science scholarship of a feminist and Marxist stripe, Sicker forges forth, proliferating definitions and relating texts to other texts in this expository work. But he seems not to appreciate the great and abiding fears and hopes that animated both state theorizers and state builders historically. "Adequate definitions" get us nowhere if the place from which we begin has no visible means of support. Simply to assert (as Sicker does) that "the state is the corporate structure, coextensive with a political society, which is the locus of supreme political authority, and which can command an effective force monopoly to assure compliance with its decisions" (p. 9) is to insulate through definitional flat what, in fact, needs to be explained and interpreted. One completes this book, with its quick once-overs of the *divine, patriarchal, force, organic,* and *consent* Theories, without any new illumination on how states emerged, how the state form got universalized, or why men and women remain prepared to sacrifice and to die in the name of the state. Any theorizing about the state ought to help us (minimally) to get our bearings with such mordant matters.

Philip Cerny's *Changing Architecture of Politics* is a conceptually "thicker" project—hence more provocative and ultimately helpful. The prose, alas, is terribly clotted. No doubt, devotion to the concept of "structuration" automatically means that words will not flow liltingly from the word processor. Cerny is devoted to the notion there is some point of origination to be located or some core to the problem to be adumbrated. He finds the core of the problem in asking "whether institutions and structures develop and operate according to an independent dynamic, and, if so, how" (p. x). If one does not share his conviction, the book will probably not be particularly compelling reading. Cerny opts for a "structural or institutional theory of politics." Unlike Sicker, he has reviewed most of the relevant literature, although feminist theorizing is once again conspicuous by its absence. He tries to weave his way in between overly idealized and overly materialistic theories of politics and the state. And he is attuned to the puzzlement of how it came about that "an essentially contingent and polycausal generation process can result in a relatively coherent, homogeneous, and structurally durable result" (p. 27). *Durable result* bears an unfortunate association with things like reliable tires, but one takes the point.

Cerny's insistence that the state is *not* "one, monolithic, state apparatus, but several levels and layers of structured games" (p. 47) is an attunement to complexity that Sicker misses. But finally, this book, too, disappoints. It omits the constitutive role of ideas—absolutely essential to any consideration of the creation of the early modern state and the concept, and practice, of sovereignty. Finally, despite Cerny's bow in the direction of "agency and choice," we are left with little insight into how states become the repositories of human hopes and help to structure our very identities. Two books that serve as a welcome antidote to the weaknesses of the volumes here reviewed are Benedict Anderson's *Imagined Community* and Eugene Weber's *Peasants into Frenchmen.*

Vanderbilt University JEAN BETHKE ELSHTAIN

A Dream Deferred: America's Discontent and the Search for a New Democratic Ideal. By Philip Slater. Boston: Beacon Press, 1991. 224p. $19.95.

Democracy and Equality: Theories and Programs for the Modern World. By Ronald M. Glassman. New York: Praeger, 1989. 234p. $42.95.

Both of these thoughtful and readable books are very much concerned about the future of democracy and the need to think about, and act upon, the problems that

face democracy in the United States. Slater, a wide-ranging social critic, asks, How can the United States move more genuinely beyond the era of authoritarianism to a fuller democratic megaculture? His thesis is that the democratic megaculture requires the complete rout of authoritarianism in its many forms—military, political, economic, social, religious, educational, medical, and psychological. Glassman, a sociologist keenly concerned about social policy, asks, "How can we institutionalize the ethic of social justice alongside competitive individualism without destroying the liberal structure of legal-democracy and market economics?" (p. 204). He advances the thesis that the achievement of democracy and equality requires us to build on, but find correctives for, liberal democracy. He argues that political democracy is essential and a market economy has much to commend it, but the sensible pursuit of equality calls for a modified capitalism drawing upon the corrective insights of Aristotle, Rawls, and Keynes. Taking into account valid objections to equality to be found in Tocqueville, Weber, Hayek, and the neoconservatives, Glassman would retain the strengths of capitalism and the market economy while avoiding the weaknesses of capitalism and classical liberalism. His model, which rejects the utopian vision of both socialism and classical liberalism, is a variety of liberal, capitalistic democracy.

Slater's attack on authoritarianism (and its four pillars: institutionalized submissiveness, systematic oppression, secrecy, and deflected hate) is hard-hitting and cogent, even when sometimes exaggerated or overstated. His defense of democracy as efficient, flexible, innovative, and participatory is heartening (if sometimes open to challenge). His argument on behalf of decentralization is only partly convincing, largely because of his failure to recognize fully enough that some centralization of power is vital in a democracy and that some varieties of decentralization can be antidemocratic and often grossly inefficient. His argument against authoritarianism would have been strengthened by a clearer recognition of the case for legitimate democratic authority. And is Slater correct in his view that the United States is out of tune with the worldwide democratic trend and that U.S. citizens complacently believe that they have already made the transition to democracy?

Many feminists will applaud Slater's contention that traditionally female traits like "flexibility, creativity, sensitivity, understanding, emotional honesty, directness, warmth, realism, and the ability to mediate, to communicate, to negotiate, to integrate, to cooperate" are "precisely those most needed to successfully maintain a democratic society" (pp. 40–41). And they may agree that "since women are far less committed to authoritarianism than men are—less beholden to it, more oppressed by it—they are perhaps in the best position to assert the values of democracy, the values of cooperation, integration, negotiation, openness, and equality" (p. 95). All democrats will cheer his affirmation that "no real democratic tradition can take root in a society in which the macho tradition holds sway" (p. 41)—even if some may have reservations about his contention that "sexism is the greatest obstacle to the achievment of full democracy in any society" (p. 85).

Both Slater and Glassman reveal the modern intellectual's emancipation from communist utopian dreams. Both are highly critical of the authoritarianism of communism's huge centralized bureaucracy. Both recognize capitalism's freedom, initiative, and creativity but fault its authoritarianism and its disregard of equality. Slater's contention that "democracy cannot survive inequality" (p. 68) is a major motif of Glassman's book. Slater stresses the importance of achieving democracy in the workplace; Glassman stresses the need to modify capitalism so as to ensure decent jobs and to overcome distressing inequalities of wealth and class. Glassman is, however, more concerned than Slater with maintaining the vitality of the middle class.

Glassman's argument is enriched by a stimulating effort (chaps. 1–3) to find correctives for liberal democracy and capitalism in Aristotle, Rawls, and Keynes. He endorses Aristotle's efforts to build his constitutional democracy on the foundation of a solid middle class. He shares Rawls's efforts to modify the Lockean contract and employ the difference principle to do justice to the disadvantaged. He admires Keynes's efforts to make capitalism stable and society more equal. Slater's emphasis, by way of comparison, is on the need for a more radical participatory democracy. Of the two, Glassman seems more in touch with social policy and realistic possibilities for change than does Slater. In many respects, however, Slater's critique of authoritarianism (and especially of sexism) cuts deeper than Glassman's critique of liberal democracy and capitalism.

Both authors are clearly critical of the status quo and seek to get beyond it. Slater's critique is the more far-ranging of the two—embracing as it does a critique of religion, education, the medical establishment, and (the wrong kind of) individualism. Glassman's critique, although committed to the advance of equality, is more respectful of valid objections to theories of equality (chaps. 4–5) and more eager to meet those valid objections in practical programs of action (chaps. 6–7).

Slater is better at telling us what is wrong with the United States today than in informing us what to do to overcome such major obstacles as the widening gap between rich and poor, the persistence of military attitudes, the tenacity of authoritarians in power, and public apathy. His efforts to prepare the ground for greater democracy include demilitarizing, containing greed, providing a future, promoting sexual equality, freeing our imaginations, opening communication, and freeing ourselves" (pp. 186–88). Although laudable, these rubrics are, alas, not all that helpful to practical reformers. Glassman, who sometimes advocates federal programs and assistance for the poor or needy that might contradict his opposition to centralization and inefficient bureaucracy, poses tough choices facing the practical reformer.

Even though the critiques of Slater and Glassman are sometimes overly ambitious, we are nonetheless seriously challenged by these two provocative authors and their fundamental concern for the future of democracy.

Drew University NEAL RIEMER

Friedrich Nietzsche and the Politics of the Soul: A Study of Heroic Individualism. By Leslie Paul Thiele. Princeton: Princeton University Press, 1990. 235p. $35.00 cloth, $9.95 paper.

According to Leslie Paul Thiele, the kernel of Nietzsche's thought involves a highly personal exploration of what it means to become a sovereign individual. Thiele argues convincingly that the conceptual content

of Nietzsche's philosophy cannot be separated from its autobiographical origins without loss; his ideas are meant to be understood in terms of the existential pathos that produced them—a pathos of perennial striving to become "what one is."

Nietzsche's choice of a political vocabulary to describe his inner strivings is, in Thiele's view, no accident. For Nietzsche, the human soul is at bottom not a unity but a multifarious chaos of conflicting drives characterized by endless struggle. The multiple voices and perspectives of Nietzsche's writings are a reflection of the tensions of his multiple soul. According to Thiele, "Politics . . . conveys most of the meanings inherent in the development of individuality: the struggle, the ambiguity and ambivalence, the will to power, the compromises and coalitions, domination and rule, plurality and rank, the search for organic unity" (pp. 3–4). In those who have the strength for it, a semblance of unity is achieved through struggle, domination, and conquest on a subliminal level. The "higher man" is he who emerges with an aristocratically ordered soul entailing a hierarchically ordered agonal community of instincts.

Thiele is careful to point out that Nietzsche eschews the egalitarian implications of liberal and democratic individualism in favor of a heroic individualism that is inaccessible to the majority. The Nietzschean hero is one who can affirm life as an ongoing process of becoming through struggle and self-conquest. Nietzschean *amor fati* (love of fate), does not imply a passive fatalism but, rather, the total affirmation of one's involvement in a world in which one is fated to do battle with *fortuna*. The term *hero* may seem problematic, but Thiele offers qualifications. He notes, for example, that Nietzsche repudiates "the cult of the hero" as just another form of idolatry. Thiele also suggests that at the highest level (perhaps unattainable) the perfectly ordered soul of the overman would not need to engage in heroic struggle.

The core of the discussion is not structured around Nietzsche's "doctrines" but, rather, around what Thiele takes to be the various incarnations of the Nietzschean ideal: the philosopher, the artist, the saint, and the educator, and the solitary. Thiele has produced a clear, gracefully written book that offers an insightful and, on most levels, extremely satisfying interpretation. He avoids the pitfalls of turning Nietzsche into the last metaphysician, a deconstructionist, a pragamatist, a literary stylist merely, or a Nazi. He does justice to Nietzsche the seeker of knowledge, as well as to Nietzsche the radical skeptic. And his account of how the chaos of the soul orders itself is more convincing than either Walter Kaufmann's or Mark Warren's because of its greater fidelity to Nietzsche's own remarks on the instincts, reason, and life as will to power.

I only wish Thiele had taken more seriously the social implications of the ideal and the view of life that his subject propounds. Unfortunately, he never discusses the many troubling passages about politics and society in Nietzsche's writings except summarily to discount their significance with arguments reminiscent of Kaufmann's. But real politics do appear to matter to Nietzsche—and understandably. His goal is not only his own self-perfection but, more generally, "the enhancement of man." Again and again, Nietzsche professes a commitment to the promotion of a higher human type: "Every enhancement of the type 'man' has so far been the work of an aristocratic society—*and it will be so again and again*—a society that believes in the long ladder of an order of rank . . . and that needs slavery in some sense or other" (*Beyond Good and Evil* 257; my emphasis). Politics matter even though the Nietzschean philosopher is not a political actor, because his highest efflorescence is facilitated by favorable social and political conditions.

Nietzsche's view of tension and struggle as life-enhancing has social and political implications that parallel the psychological ones: "The highest type of free man should be sought where the highest resistance is constantly overcome: five steps from tyranny. . . . This is true psychologically if by 'tyrants' are meant inexorable and fearful instincts that provoke the maximum of authority and discipline against themselves. . . . This is true politically too; one need only go through history" (*Twilight of the Idols*, sec. 38). It is not only because spiritual struggle resembles political struggle that Nietzsche uses political language to describe the soul. More importantly, it is because there are analogies between the well-constituted soul and the well-constituted society, with the soul of the highest type closely resembling the kind of tension-filled aristocratic order Nietzsche both admires and desires.

Criticisms aside, Thiele's has written a book of very high quality that makes an important contribution. His account of the politics of the soul is excellent; and there can be little doubt that the politics of the soul is of far greater interest to Nietzsche than the politics of the world.

Florida International University BRUCE DETWILER

The Forms of Power: From Domination to Transformation. By Thomas E. Wartenberg. Philadelphia: Temple University Press, 1990. 253p. $34.95.

What happens when philosophy meets political science? It is a loaded question, to be sure, but one that characterizes Thomas Wartenberg's very careful and thoughtful examination of the metaquestion of *power*. Wartenberg distinctively uses prominent philosophers to make his points about particular questions in the analytical debates about power. He simultaneously articulates his own approach to power in a way that seeks to circumvent the conceptual traps that have stymied political theorists. In the process, the analytical discourse on power is placed in a broader context while its narrative is enriched.

Wartenberg's analysis is premised on the insight that social power takes a variety of forms, has different uses, and therefore cannot meaningfully be integrated into one all-encompassing theory. He rejects the idea that power is an essentially contested concept like art or democracy, arguing, instead, that competing conceptions of power conceptualize different, if related, phenomena. For instance, he accepts the distinction between *power-to* as ability versus *power-over* as constraint; however, he sees them not so much competing with each other as conceptualizing different things. He focuses on power-over because of its crucial implications for criticizing social inequities.

Wartenberg shows how power-over can be positive, as well as negative, as in the case of paternalism. He shows how power-over involves both consensual and coercive aspects. Power-over is articulated through different types of power—force, coercion, and influence—

and often simultaneously. These are quite distinct from its uses ranging from domination to transformation. The former involves the exercise of power at the expense of the subordinated while the latter promotes their empowerment. Wartenberg adds that the interpersonal dyad in a power-over relationship is structurally situated. Theories of power-over must also be dynamic and account for the temporality of power. Power-over relationships involve anticipations of future actions that might be undertaken by primary and collateral parties.

These analytical investigations create the basis for Wartenberg's own theory of power-over—aptly titled a "field theory of power." It is, of course, a structural, dynamic theory of how power-over can be used for both domination and transformation. While heavily dependent on a select group of theorists ranging from Plato to Sara Ruddick, Wartenberg manages to offer his own distinctive contribution. He stresses that power-over is not reducible to a behavioral trait or individual possession. He uses Hegel, Marx, Nietzsche, Giddens, and Foucault to show how power operates in a situated, structural context rigged from above or below. He does so without subjectifying power, as he suggests Foucault does. Instead, he maintains a place for human agency while underscoring that individuals' "action-environment" as well as their "action-alternatives" (and therefore the individuals themselves) are structurally constituted.

Wartenberg's articulation of transformative power is particularly interesting. Drawing upon Ruddick's idea of maternal thinking, he shows how recent debates in feminist theory do much to illuminate the complexities of power. In particular, power-over can involve a form of empowerment (e.g., the care and nurturance of mothering) even while it may be situated in a context of domination (e.g., patriarchy). Wartenberg is quick to underscore that mothering need not be limited to the family, not all nurturing is reducible to mothering, and that nurturance need not lead to empowerment, especially given that it may end up reproducing the dominant culture (and therefore reflecting the problem Wartenberg calls *superposition*). Rejecting Nietzsche and I. F. Stone's view of Socrates as an abuser of power, Wartenberg suggests that Socrates' elenchic interrogation provides an even better model of transformative power. Wartenberg notes that this model better highlights the importance of trust in a truly empowering relationship. Yet the empowerment may still remain elusive for a variety of reasons, including, most especially, the problem of superposition. We may trust nurturers to empower us, only to find they have enslaved us to the dominant order that constrains them.

Wartenberg has sought to fashion a specific theory of power while still insisting on the need to appreciate power in its plurality. Such an account of power, Wartenberg tells us, can inform criticism of specific social practices by highlighting the power relationships implicit in them. This is all done in a persuasive way, using important philosophical arguments to make insightful theoretical points. Yet Wartenberg's approach risks essentializing and reifying power at the expense of articulating the specific practices that confront actors in their lived experience in particularized settings. While Wartenberg stresses the variety of power relationships, forms, and uses, he still strives for a level of analytical abstraction that the diversity of power practices may not be able to sustain. His thoughtful reworking of the categories of power analysis, however, may very well provide new ammunition for the more localized battles.

Macalester College SANFORD F. SCHRAM

Freedom within Reason. By Susan Wolf. New York: Oxford University Press, 1990. 162p. $24.95.

Susan Wolf's concern is to offer a philosophical understanding of individual responsibility. She does so by examining two prevailing views of the character of individual freedom and agency and then developing one of her own. In doing so, she sheds considerable light on how and when we can attribute freedom and responsibility to individuals in a world in which their actions may be fully determined by psychological and physical causes. Those engaged in the study of politics can profit from her interpretation of these three views of human agency, particularly her argument regarding the conditions that must hold for the attribution of freedom and responsibility. Moreover, she manages to present them in a manner that is comprehensible to those uninitiated in the esoteric language of contemporary analytical philosophy.

Wolf's argument addresses perhaps the most critical and difficult philosophical problem faced by social and political scientists, that of reconciling individuals' freedom to choose with the deep ways in which social and psychological forces shape human behavior. As increasingly recognized by liberal political theorists, the presumption of substantial autonomy in moral and political decisions is brought into serious question by the "situatedness" of choice. Similarly, the presumption that individuals are able to make their own free and independent choices (at least some of the time and with respect to at least some important decisions) is not easily reconciled with models of explanation that view human behavior as fully determined by external causes. There is an obvious paradox in holding citizens and leaders responsible when external, "independent" variables are the basis for explaining what they do.

In Wolf's analysis, there are two prevailing views of the self: the *autonomy view* and the *real self view*. Those who argue from the autonomy view hold that to be free and responsible, individuals must be able to choose and act completely in the absence of external forces. By this view, social and physical influences must be ignored or overcome through an act of the will if choices are to be freely made. Proponents of the real self view, on the other hand, are willing to allow that individuals' beliefs and identity are fully shaped by social influences; yet, since these have been internalized in unique ways, the pattern of values and preferences that any individual has come to hold is properly seen as constitutive of her or his self. This is particularly true if the self is identified with a person's most deeply held values, rather than with the more superficial, transitory preferences or desires that he or she may hold.

Wolf identifies strengths and limitations with each of these views and, in a thoughtful and persuasive analysis, develops a *reason view* of the self that, she believes, is required for the attribution of freedom and responsibility. The critical element missing from both the autonomy and real self interpretations is the centrality of deliberation and reason in choices for which we hold

individuals responsible. Such reasoning does not require autonomous separation from the world, nor is it excluded from a world that is fully determined by physical and psychological causes. Thus, liberal philosophers' search for independence of thought is unnecessary; and empiricists' explanation of political decisions and actions in terms of external variables does not, for that reason, deny freedom and responsibility to citizens or policy makers. Yet Wolf convincingly argues that individuals are able to reason only if they have been provided with the abilities, understanding, and resources for doing so. In particular, "an agent cannot have the kind of freedom and control necessary for responsibility unless, when making choices about values and actions, she can understand the significant features of her situation and of the alternatives among which her choice is to be made" (p. 117). In elaborating this requirement of responsibility, Wolf emphasizes the importance of normative competence—a competence that involves (among other things) reasoning from right values, rather than wrong ones, and true beliefs, rather than false ones.

To attach freedom and responsibility to one's ability to reason with reference to the true and the good, as Wolf does, is obviously a difficult notion for empirical political scientists and even political philosophers to entertain. Value subjectivism, in particular, is an assumption that is accepted almost without question among political scientists. Wolf understands the appeal of subjectivism, and she does a masterful job of challenging it. She suggests that there are no more compelling reasons for disallowing objectivity in values than there are for doing so with respect to facts. Conversely, reasoning is as central for determining good values as it is for forming what we accept as true facts and explanations.

These are not particularly novel ideas in contemporary philosophy; indeed, a valuable dimension of Wolf's book is her clear summary of arguments that in their original form, are virtually impenetrable to those who are not trained in academic philosophy. But they are ideas that empirical political scientists and political theorists need to encounter. More than this, there are compelling reasons for believing that something like Wolf's reason view of the self is essential for attributing freedom and responsibility. Even if this is not the case, she demonstrates that the autonomy and real self views that underlie most political inquiry offer insufficient bases for attributing responsibility to individuals for their actions. This calls into question normative and explanatory arguments to the effect that citizens and political leaders can be held responsible because their decisions have been made within the context of procedural rights and competitive elections or because they result from a process of agenda-setting and bargaining. If Wolf is right (and her argument is persuasive) our political principles and our explanations must demonstrate that to be held responsible for their decisions, citizens and leaders are provided with the normative competence they need to reason about the political problems and choices they confront even as they live in a fully determined world. Present political inquiry has a long way to go to make this claim.

Northern Arizona University LARRY M. PRESTON

AMERICAN POLITICS

Judicial Compulsions: How Public Law Distorts Public Policy. By Jeremy Rabkin. New York: Basic Books, 1989. 322p. $22.95.

The Legacy of Judicial Policy-Making—*Gautreaux* v. *Chicago Housing Authority*: The Decision and Its Impact. By Elizabeth Warren. Lanham: University Press of America, 1988. 97p. $14.75.

Judging the Constitution: Critical Essays on Judicial Lawmaking. Edited by Michael W. McCann and Gerald L. Houseman. Boston: Scott, Foresman, 1989. 453p. $12.76 paper.

Though using varied approaches and contexts, each of these volumes focuses on recurring questions concerning the nature, role, and limitations of judicial policy making.

Jeremy Rabkin's *Judicial Compulsions* provides an interesting, though ideologically toned, argument against what he views as the consequences and implications of improper judicial intervention into administrative lawmaking. Emanating from the "legal legacy of the fevered 1960s" and "spurred" by the civil rights, antiwar, environmental, and consumer movements, Rabkin states that this development "encouraged a new approach to judicial controls on regulation, allowing beneficiaries of regulation ("so-called public interest groups") to sue agencies to demand more extensive or more rigorous controls (p. 4).

Despite elections that have placed in office more moderate and conservative presidents and members of Congress, Rabkin contends that "judicial controls developed in the heady days of the sixties" remain, penetrating judicial thinking and stirring judicial compulsions to intervene in administrative policy making. This is what encourages contending interests "to view even the details of policy as legally entailed and precommitted" (p. xiii).

Rabkin's major concern is that "such legalism is a distraction from the genuine policy issues that ought to dominate public debate about regulatory performance" (p. xi). Accordingly, the author sees the dire need to limit judicial power of courts and return policy making to officials who are politically accountable rather than rely on judges being used by private advocacy groups to achieve their objectives" (p. xii).

To support his argument, Rabkin develops rather detailed case studies of major policy concerns in three regulatory agencies: OCR, OSHA, and the FDA (chaps. 5–7). In the OCR chapter, for example, Rabkin details the protracted *Adams* litigation commenced by the NAACP Legal Defense Fund and its effects on the functioning and operations of the OCR. The suit was designed to compel enforcement of civil rights laws so as to overcome continued racial segregation in public schools. Rabkin argues that such litigation and "misguided judicial interventions" (p. 260) oftentimes frustrate and distort policy implementation, agency functioning, and our constitutional governing system.

This volume is certain to engender acclaim and criticism. It is likely to win praise from those who think the court's "misguided judicial interventions" do indeed distort and impair the policy process and proper institutional role and functioning. Likewise, Rabkin is likely to find strong support for the view that "traditional constitutional doctrines" overcome by the Warren Court in such decisions as *Brown* v. *Board of Education* (1954) and *Baker* v. *Carr* (1962), have led to "long term federal regulatory venture[s]" and "similar activist ventures in administrative law" (p. 129–30).

Such activist ventures impair "responsible government" and give public interest groups more policy leverage, leading to the "current system," which "has evolved from . . . a disoriented legal culture" reflecting "not simply the obfuscations of visionary law professors or the confusions of impulsive judges, but the underlying ambivalence in modern American liberalism." "To put it most simply," says Rabkin, "administrative law has sought to accommodate the desire for increasingly large government programs capable of rearranging society to serve consciously formulated plans, while at the same time seeking to accommodate deep-rooted suspicions of 'big government' or of centralized administrative power" (p. 243).

On the other hand, Rabkin's volume is also likely to engender some concerns. One limitation of the study, for example, as Rabkin himself seems to recognize (though with qualification) is that "the critical difficulty in all these cases would be to demonstrate that better policies would indeed have occurred *but for the intervention of the courts*" (p. 311, n. 20; emphasis original).

It is, of course, true, as Rabkin suggests, that "courts simply do not have enough power or enough expertise to supply the ultimate policy benefits that interest advocates seek" (p. 259). But neither do the president, Congress, or administrative agencies have enough power or enough expertise *on their own* to supply claimants such ultimate policy benefits.

A more overarching concern, however, is that Rabkin's study portrays a concept of constitutional governance that would somehow permit the Court to return to "traditional constitutional perspectives" (p. 33) and "the grounding assumptions of the traditional constitutional scheme" (p. 6). It would allow the Court to avoid "legalistic compulsions" (p. xiii) and limit judicial interventions to "more serious complaints about the substance of policy" (p. 268). Clearly, such normative criteria (about which reasonable persons might and do differ) offer minimal guidance or standards by which to determine, for example, when it is or is not appropriate for the Court to intervene or otherwise defer to politically accountable officials (see chap. 8).

The difficulty of reaching agreement on such matters (which are themselves the stuff of politics) would seem to suggest a continuing alertness to one of the most *fundamental* yet sometimes overlooked (or ignored) features of U.S. politics. This feature, so artfully developed by Jack Peltason in his pathbreaking *Federal Courts in the Political Process* (1955), suggests that it is not a matter of *their choice* but of *their function* (i.e., judicial review and statutory interpretation) that places courts and judges in the policy-making process.

Thus, whether viewed as liberal or conservative, interpretivist or noninterpretivist, or activist or self-restraining, the actions (or *inactions*) of courts and judges

hold policy consequences that advantage certain interests and disadvantage others. This is the central factor that under particular circumstances leads interests to resort to litigation as political strategy.

Elizabeth Warren's *Legacy of Judicial Policy Making* also looks at the role and impact of the judiciary in the making of public policy. Warren's is essentially a lively case study of the development and impact of Chicago housing litigation that eventuated in the Supreme Court decision of *Hills* v. *Gautreaux* (1976).

In describing the context out of which the case developed, Warren captures well the situation in the mid-1960s that led to the "inevitable" collision between two "inexorable" forces: the determination of the growing Civil Rights movement to overcome racial injustice versus the "well-settled practices of the City government and the Chicago Housing Authority (CHA), backed by the hands-off policy of the Federal housing agency" (p. 11).

Particularly intriguing is the author's description of the four leading characters in this litigation drama. We start with Mayor Richard Daley, who wanted to—and in the main did—keep control and order in his city, which in effect meant maintaining racially segregated housing. Then there was federal judge, Richard Austin, who "privately concluded that he had been 'set up' by Mayor Daley and the Cook county machine as a 'last-minute gubernatorial candidate'," in a Republican year (1956) "and (as a result) his opposition to the machine and the Mayor was undiminished for the remainder of his life" (p. 5). More directly, as Warren put it, "Judge Austin and the Mayor became mortal enemies over the Gautreaux case" (p. 5).

Another leading figure in the litigation was Charles Swibel, chairman of the CHA for 19 years, who ran it "by fiat." Swibel, of course, "was loyal to Mayor Daley and saw eye to eye with the Mayor regarding public housing's place in City policy." Finally, there was Alexander Polikoff, an attorney who, with other attorneys from the American Civil Liberties Union, prepared and filed a class action suit in Judge Austin's federal district court on behalf of Dorothy Gautreaux. These were the lead players from which "the litigation received its stamp" (p. 4).

In chapters 3–5 Warren offers a vivid portrayal of the interaction of individuals, interests, and institutions (including the federal district court and court of appeals) in the drawn-out litigation that eventually led to the Supreme Court decision *Hills* v. *Gautreaux*, originally denoted as *Gautreaux* v. *Chicago Housing Authority*. Here the Court held that it was constitutionally permissible to fashion a metropolis-wide remedy, in that both Housing and Urban Development and the CHA had perpetuated illegal racial discrimination in the Chicago housing market and had necessary authority to make decisions on a metropolis-wide basis (which was the "relevant housing market").

Overall, Warren finds *Gautreaux* the "most successful of the various efforts to deconcentrate subsidized housing" (p. 91). In certain respects, however, Warren indicates some concern about the role, limitation, and impact of courts in policy making. That concern is both reflected and tempered by Warren's observation that today "there is reason to believe that judges are turning to experts for needed background information when they confront cases which deal with broad social issues. . . . A recognition of their own limitations and a desire to be fair makes such expert consultation an appropriate part of the judicial process" (p. 94).

In sum, Warren's is an informative, readable, and lively account of a continuing and important political–social problem and should certainly prove useful to the "students of public policy" (p. ix) for whom the book was principally written.

While Rabkin and Warren use relatively discrete contexts in which to consider fundamental questions concerning judicial policy making, McCann and Houseman's *Judging the Constitution* considers these and related questions from a rather broad range of theoretical and empirical perspectives. Here is a well-planned collection of excellent "original essays offer[ing] a variety of critical views concerning the character and implications of constitutional lawmaking by the Supreme Court in American public life" (p. 1). In a tightly written introductory chapter, the editors skillfully outline an overall context and organization that forges a unity and coherence seldom found in an edited collection of essays.

The essays assume, like the realists and the followers of the Critical Legal Studies movement, or "crits," "that rules and principles—whether rooted in the written text, the framers' intent, standing precedents, or ethical arguments—neither determine constitutional lawmaking in any mechanical sense nor impart to it any universal claims of ethical objectivity" (p. 6). But the essays are by no means doctrinaire. Indeed, unlike realists and some "crits," the authors, in the main, assume that "normal constitutional lawmaking practices remain substantively 'bounded' or constrained in important ways" (p. 6).

Additionally, while most authors view courts and law as potentially important to affect social change, they nonetheless recognize "the important limits to law as a force within society." This is illuminated brilliantly by Deborah Rhode's chapter, "Equal Protection: Gender and Justice." Rhode states that "despite changes in gender roles, the legacy of gender discrimination remains pronounced [indicating that] all too often equality in formal mandates has not translated into equality in actual status" (p. 281). And to Rhode, the persistence of such inequalities "suggests a general point about legal doctrine," namely, that "[while] law has been of critical importance in eliminating formal barriers to equal opportunity, it has been far less successful in challenging the informal structures, implicit assumptions, and social priorities that make such opportunities impossible to realize" (p. 281).

The volume is divided into five major parts, each part beginning with a brief overview of the respective contributions. Part 1, for example, "Contemporary Constitutional Theory," offers contributions on problems of legitimacy by Lief Carter, John Gilliom, and Judith Baier and "The Constitution and Social Change" by Stuart Scheingold and Rogers M. Smith.

Parts 2–5 focus on major areas of modern constitutional discourse and debate. Here, William Haltom and David Gray Adler offer essays dealing with how judicial review of the constitutional "separation and sharing of powers" scheme has affected subsequent institutional development. Part 3, "Property Rights and Economic Regulation," includes contributions by John Brigham and Margaret Jane Radin.

The chapters by Michael McCann, Deborah Rhode, and Rhonda Copelon in Part 4 focus on how judicial

construction of Fourteenth Amendment equal protection has served to advance and limit progress by long-marginalized groups such as blacks and women. The volume concludes with contributions by Donald Downs, Roger Simpson, John Shockley, and Ronald Kahn offering critical assessments on important developments relating to the First Amendment (e.g., protections for speech, press autonomy, and religious practice and personal belief).

What makes this volume exciting and altogether worthwhile, is that though the various authors clearly hold particular and similar concepts about courts and law, they nonetheless offer an uncommonly sophisticated level of *critical* analyses of important contemporary issues. These analyses allow the reader to get a real feel for the nature, role, and limits of courts and law in U.S. politics. As such, the volume should appeal to a range of audiences, including students in graduate and advanced undergraduate courses.

Stanford University LUCIUS J. BARKER

LBJ and The Polls. By Bruce E. Altschuler. Gainesville: University of Florida Press, 1991. 137p. $19.95.

The Joint Press Conference: The History, Impact, and Prospects of American Presidential Debates. By David J. Lanoue and Peter R. Schrott. Westport, CT: Greenwood, 1991. 173p. $39.95.

These two books have little in common except an interest in the U.S. presidency. Lyndon Johnson, the subject of Bruce E. Altschuler's case study, declined to debate Barry Goldwater in 1964 and so warrants no more than a sentence or two in Lanoue and Schrott's volume.

Altschuler's thesis is that the Johnson White House commissioned, interpreted, and exploited polls in increasingly dishonest ways that reinforced, rather than dispelled, presidential isolation. Chiefly concerned with confirmation of their boss's popularity and political instincts, LBJ's minions are said to have engaged in shallow analysis of poll data while ratings were favorable and outright distortion afterward. A more objective reading of the polls, even if unwanted by LBJ himself, might have alerted the president to the limits of permissive consensus and tempered his policies accordingly.

Relying on primary materials obtained at the Johnson Library, Altschuler makes a strong case when recounting White House promotion of a preposterous poll taken in Vietnam, misrepresentation of results from a single-county survey in New Hampshire, and dubious definition of poll responses to exaggerate domestic support for the war in Vietnam. His indictment will ring true to many readers at a time when LBJ is held in especially low repute.

Some charges may not stick, however, since Altschuler contradicts several of his own arguments. A case in point is his criticism of Johnson's aides for not teasing more complex relationships out of the polling data. This complaint loses much of its force when Altschuler reveals that the data in question were often too limited for much exploration. Many, if not most, of the polls done for LBJ sampled opinion in one state, rather than nationwide; and few, if any, offered more than approval ratings, rough demographic groupings, and basic cross-tabulations. Much of this information, moreover, came to the White House free of charge as responses to items tacked onto polls otherwise unconcerned with presidential politics.

Other inconsistencies confound a rather simplistic account of how objective poll analysis could have restrained Johnson's Vietnam policy. Altschuler characterizes early public opinion on what to do about Vietnam as uninformed, ambivalent, but deferential to "any course of action that would work" (pp. 44–45). This was the permissive consensus Johnson enjoyed in 1965 when he embarked on a middle course between withdrawing from South Vietnam and all-out war against North Vietnam. (Altschuler faults White House analysts for not alerting Johnson to the limits of public support for such a policy but only two pages later recounts precisely such a warning by aide Hayes Redmon.) After escalation and subsequent loss of public support, Altschuler contends, LBJ should have heeded the polls and "reconsidered" his policy (p. 59). However, he later concedes that a sharply divided electorate allowed Johnson little latitude for radical change (p. 105).

On the one hand, Altschuler's account of Johnson's flawed strategy for contesting early primaries in 1968 is probably the most detailed available. His account of LBJ's relationships with national pollsters is also informative. On the other hand, several sketchy discussions of polling results cry out for supplemental tables or figures, especially since most of these data are not readily available to other scholars. More tables (or, better yet, a detailed appendix of questions and frequencies) would have enhanced the book's value as a source for others interested in the same issues.

Readers misled by Lanoue and Schrott's title into expecting attacks on the standard format for presidential debates will find, instead, a comprehensive inventory of theoretical frameworks, research methodologies, and empirical findings arising out of studies of most joint press conferences since 1960. David Lanoue and Peter Schrott skillfully weave most of these strands together into a schema theory and media interpretation framework for measuring the content and effects of future presidential debates. Chapter 2 sets the stage for all this empiricism by reviewing the highlights of every presidential and vice-presidential debate since 1960, and by recalling the conventional wisdom dominating journalistic interpretations of each event.

Widely believed since the first Kennedy–Nixon meeting in 1960, the most fundamental bit of conventional wisdom is that debates are important to election outcomes. Clearly, the acceptance of this notion can be discerned in the seriousness with which each team negotiates the technical details and then prepares its champion for the biggest television draw of the fall campaign. Another conventional wisdom of considerable vintage is that media scorekeepers wield enormous power over likely voters when they judge who won and lost the debate. The generally predictable pitches of campaign "spin doctors" immediately following each debate attests to the credibility of this thesis. A third piece of conventional wisdom dating back to 1960 is the conviction that debate watchers are more influenced by a candidate's personality, manner, and physical appearance than by the logic or substance of his arguments.

Lanoue and Schrott address these and similar notions in chapters 3–4. Chapter 3, a valuable discussion of the

uses of content analysis in measuring type and substance of debate messages, establishes that candidate pronouncements are generally more substantive in debate presentations than in set speeches or media spots. (This may not be true of prenomination debates where more candidates have less response time.) After discussing consistency theory, functional theory, and other theories of attitude change, chapter 4 thoroughly reviews the literature of debate effects. Once they have apprised readers of differing operational definitions, Lanoue and Schrott conclude that presidential debates do importantly (if indirectly) affect perceptions of candidates, that this influence is filtered through media verdicts and peer discussions, and that people who did not watch a debate may alter their opinions of candidates in response to general reports of strong or weak candidate performance, but that the voting decisions of even favorably impressed viewers are not so easily changed.

In short, Lanoue and Schrott have written an essential reference work for any political scientist seriously interested in the subject. My only criticism is that their necessarily brief discussion of prenomination debates omits any consideration of how such multi-candidate and often acrimonious events might extend the negative effects of divisive primaries well beyond the states where debates are staged. This seems especially likely when long shots gang up on front-runners in hopes of winning more media coverage or when survivors of the winnowing process rip and tear at one another's accomplishments, judgment, and integrity. As Lanoue and Schrott point out, the format, numbers, strategies, messages, and audiences of prenomination debates are different from general election forums; effects may differ, as well.

Denison University EMMETT H. BUELL, JR.

The Cautious Welcome: The Legalization Programs of the Immigration Reform and Control Act. By Susan Gonzalez Baker. Lanham, MD: RAND Corporation and Urban Institute Press, 1990. 196p. $36.75 cloth, $19.00 paper.

Reflecting an ongoing tension between inclusionary and exclusionary approaches in U.S. immigration policy, the Immigration Reform and Control Act (IRCA), enacted in 1986, had two major dimensions: (1) benefit, or legalization and (2) enforcement. The key legalization provision, concerning general legalization, created a temporary program permitting status adjustment for undocumented immigrants to become citizens if they could demonstrate continuous residence in the United States since 1 January 1982. A second major provision permitted status adjustment of certain undocumented farm workers through (1) Special and (2) Replenishment Agricultural Worker Legalization programs.

The present study is part of a broader one of the implementation of IRCA, the Program for Research on Immigration Policy, undertaken by the RAND Corporation and the Urban Institute. It describes the design, implementation, and outcomes of these several legalization provisions of IRCA. It does not address the enforcement dimension, i.e., employer sanctions and increased border patrol. Drawing on interviews with those involved with program implementation and on firsthand observation, the report's overall assessment is that IRCA's legalization programs worked reasonably well (although the program has not been completed in that a large number of cases remain to be adjudicated).

The book's discussion of IRCA's legislative history underscores that granting citizenship to those who had entered the U.S. illegally was, and remains, controversial. That is why legalization was balanced by provisions for stronger enforcement. Also, the fact that there were several legalization programs, rather than just the general program, was part of a delicate balance to appease various interests, including agricultural interests concerned about continued labor supply.

Most of the book is given to a detailed recounting of the implementation challenges posed by the legislation and responses to those challenges. For instance, because of the need for effective outreach and publicity, the Immigration and Naturalization Service (INS) worked with "qualified designated entities" (essentially, local community organizations, church groups, and the like) to make undocumented immigrants aware of the legalization program. Yet these groups had traditionally been suspicious of the INS. There was also the irony of requiring documentation of continuous residence from undocumented workers. Baker's description of the approaches tried—and the levels of success achieved—in grappling with these and an array of other administrative, legal, and political difficulties makes for interesting reading.

While informative, because written as a report with an applied focus, political scientists are likely to find the work descriptive but not particularly analytical. For instance, the report indicates that in some locales and regions there were more or fewer applicants than had been estimated. The explanations offered for these, and other patterns and outcomes, however, may appear impressionistic or somewhat speculative. Similarly, those interested in the politics of policy formation will have to look elsewhere for a careful and extended discussion of IRCA's legislative history. Nor does the book seek to place its findings within any broad, or even middle-range, theoretical context; indeed, very few scholarly sources are cited, and there is no index. In short, the book's strength is in the *what*, not the *why*.

Baker's conclusion—that "change in the U.S. immigration system can be affected as much by reforming the benefits side as by modifying immigration law enforcement"—is presented in an all-too-brief closing section. It may well be that this conclusion is justified—that IRCA's legalization programs were somewhat successful; but the evidence marshaled in behalf of that argument may not convince everyone, and the standards for judgment that underlie it are not clear.

Thus, this report provides important evidence to assist in evaluating a major dimension of IRCA, and observers can learn much from it. However, the interpretation offered is neither conceptually well developed enough nor empirically well supported enough to be definitive. But this is due as much to the tremendous difficulty in evaluating a program as administratively complex and politically controversial as IRCA as to the assessment of IRCA presented in this volume.

University of Colorado, Boulder RODNEY E. HERO

Deeds Done in Words: Presidential Rhetoric and the Genres of Governance. By Karlyn Kohrs Campbell and Kathleen Hall Jamieson. Chicago: University of Chicago Press, 1990. 275p. $27.50.

Playing the Game: The Presidential Rhetoric of Ronald Reagan. By Mary E. Stuckey. New York: Praeger, 1990. 127p. $37.95.

Politically Speaking: The Pragmatic Analysis of Political Language. By John Wilson. Cambridge: Basil Blackwell, 1990. 203p. $45.95 cloth, $16.95 paper.

All three books are about rhetorical strategies, persuasion tactics, and communication skills of public figures. *Deeds Done in Words* and *Playing the Game* focus on the U.S. presidency and *Politically Speaking* draws some material from the U.S. presidency but more from the British parliamentary system.

All three works claim that rhetoric and communication have a vital role to play in understanding the "going public" part of political science.

Stuckey's book is the most narrowly focused of the three. It is a straight rhetorical analysis of Ronald Reagan's presidency. Stuckey's thesis is "[that] Reagan's use of language in his public speech was instrumental to the creation and maintenance of the "Teflon presidency," and that this use of language created a situation whereby the Teflon was bound to crack, as it did in 1986" (p. 1). Stuckey contends that "Reagan's rhetorical success was built around foreign policy events" and that this is why the Iran–Contra affair "provided the most conspicuous failure" of his administration (ibid).

Wilson's makes the most conservative claims of the three works. He subjects samples of political language to an analysis of linguistic pragmatics, a discipline "concerned with the way in which meanings are constructed" within a particular context (p. 3). One of the ultimate goals of pragmatic exercises in general is to gain "insight into communicative intentions" (ibid). In other words, pragmatists want to be able to say what the real motive and meaning of speech is, beyond what has been said. But their proof must follow established linguistic standards.

Wilson's goal, in contrast, is more modest. He wants to demonstrate that "pragmatic concepts are in operation in real talk, as opposed to contextually sterile examples frequently employed by linguists and philosophers" (p. 181). He argues that the analysis has implications, in turn, for the theoretical discussion of linguistic pragmatics. Since we learn from Wilson that linguistic pragmatics is in the throes of a controversy involved in defining the boundaries of the discipline, we should not be surprised that he devotes an entire book to one step in the defining process.

Campbell and Jamieson transcend the narrow rhetorical analysis of a single president's rhetoric and the conservative textual methodology of linguistics. Very simply, their goal is to demonstrate how speeches perform "vital functions for the preservation of the presidency as an institution" (p. 50).

Mary Stuckey presents good reasons for her contention that a scholar cannot understand Ronald Reagan's Teflon coating apart from a careful study of his rhetoric. But she may have pushed her thesis too far. Despite Stuckey's content analysis of Reagan speeches showing that he spoke more about foreign policy than about domestic policy in each passing year of his presidency, I remain unconvinced that the Teflon presidency was primarily a product of Reagan's articulation of foreign policy per se. Stuckey claims that the period 1983–85 was the Teflon period. Reagan's concentration on foreign policy made his image seem "durable and unassailable" because he could do with foreign policy what he did best rhetorically, that is, "communicate broad ideological parameters of an event or issue with little regard for the details or substance" (p. 87). But James Reston began to outline the Teflon presidency as early as 1981, when he wrote: "For the present it is generally agreed here that Mr. Reagan is personally dominating the news and the politics of this nation. He gets credit for everything that goes right, and . . . Haig or . . . Stockman get the blame for everything that goes wrong" ("A Sweet Fellow," *New York Times*, 8 March 1981). The point is that Stuckey may not have needed to prove the link between Teflon and foreign policy in order to establish her most important point, namely, that rhetorical analysis is an integral part of any serious study of Ronald Reagan's presidency.

I confess I had to work to follow John Wilson's intricately woven arguments. But then, I am no expert on pragmatic linguistics, and Wilson clearly has aimed this book at colleagues in his area of expertise. Nevertheless, there are tidbits in Wilson's work that scholars outside linguistics ought to find tasty indeed. Wilson argues that how politicians manipulate the various forms of *I*-pronouns and *we*-pronouns to present a particular perspective may tell us something about how the politician views issues and other people in and out of government in relationship to himself. Richard Nixon, for example, used *I* and *we* in a one-to-one ratio when he responded to questions about Vietnam. But when he responded to Watergate questions, he used *I* and *we* in a five-to-one ratio (p. 55). Also, Wilson says the choice to use *it* instead of *he, she,* or *they* potentially loads the subject at hand ideologically. The use of *you* and *one* may be cleverly designed distancing strategies. Wilson also explores why Nixon referred to himself in the third person as "*the president*," instead of *I*. The bottom line is that politicians use particular pronouns on purpose and that we ought to explore pronouns when we analyse rhetoric.

As a practical matter, the next step for Wilson—at the risk of stirring up even more controversy in his field—is actually to attempt some rhetorical analysis, using what he has learned. In other words, does linguistic pragmatics, when actually applied to real rhetoric, give us some more insight into the "going public" part of political science?

Deeds Done in Words is a book that was crying to be written in the field of rhetorical criticism. Campbell and Jamieson place presidential speeches into categories, or genres, and then distill the essence of those speeches to come up with certain principles that can then be applied to criticism of other speeches in the same genre. Genre criticism has been around for a long time, but Campbell and Jamieson are masters of the discipline. Although the authors answer their critics up front, the best test of whether or not genre criticism can be applied to presidential rhetoric is, Does it work? Do these genres tell us something about presidential rhetoric we did not know before? In this case, the answer is *yes*.

The genres Campbell and Jamieson have developed should keep students busy for a long time just applying the principles outlined to specific examples of presidential rhetoric. But the authors have done more. They have

taken another leap forward in establishing the essential relationship between rhetoric and the presidency itself. They argue successfully that the genres they have identified actually help define what the presidency is and how that office interacts with the other branches of government and the American people. The rhetoric described by Campbell and Jamieson helps sustain the presidency as an institution. The speeches are real acts, "deeds done in words."

Although Campbell and Jamieson are very careful to provide good reasons why they have limited their book to certain genres of presidential messages—inaugurals, state-of-the-union and veto messages, war, impeachment, pardons, and farewell addresses—and even though they invite us to "explore this link in other forms of presidential discourse" (p. 4), I at least, await their next book. Defining other genres will be more difficult but very rewarding.

As the authors readily admit, their analysis ignores some rhetorically impressive and some historically important presidential messages, like Lincoln's Emancipation Proclamation and Gettysburg Address, Kennedy's "Ich Bin Ein Berliner," and even Reagan's remembrance of Normandy. And as complicated as it might seem to put into genre framework, we somehow need to account for policy messages, speeches announcing fundamental change in direction, institutional addresses to the people, the elusive press conference, and perhaps even the president as campaigner.

ABC News CAROLYN SMITH

Structures in Multiorganizational Systems. By Donald Chisholm. Berkeley: University of California Press, 1989. 196p. $37.50.

Organization Theory: From Chester Barnard to the Present and Beyond. Edited by Oliver E. Williamson. New York: Oxford University Press, 1990. 214p. $29.95.

"Organization matters" has become a catch phrase throughout the social sciences. Does it signal the development of a science of organization? In a collection of essays edited by Oliver Williamson celebrating the contributions of Chester Barnard, Williamson is cautiously optimistic; and with Donald Chisholm's work on informal organizations, caution need no longer limit optimism. Both books provide exciting glimpses of the progress made thus far in developing a general understanding of organizations.

Many authors, including Williamson, have emphasized the importance of hierarchy for achieving coordination. Chisholm, on the other hand, asks whether coordination is possible without hierarchy. His answer is *yes*. Chisholm's affirmative answer emerges from an examination of the San Francisco Bay Area public transportation system. This system is often characterized as chaotic because it is comprised of six primary operators and two supporting organizations with no overarching, unified authority coordinating the various transit providers. Each organization is unavoidably incomplete; in organization theory terms, each is an open system. The actions taken by one transit operator affect at least one, if not all, other operators.

In such a situation coordination may be necessary. But the standard solution—an overarching hierarchy—need not be. It all depends upon the level of interdependency among organizations. Bringing together within one organization a number of once-independent organizations may force *too much* interdependence and tax the cognitive limits of boundedly rational officials. Also, it is incorrect to assume that the lack of a single all-encompassing organization means that no coordination among interdependent organizations takes place. Coordination often does occur but informally—it does not appear on any organizational chart. Officials within different public organizations often develop out-of-channels contacts and agreements with one another. These informal organizations are not used for happy hour socials. Rather, they provide the means by which individuals can achieve organizational goals. Chisholm provides a number of examples.

The advantages of informal organizations as tools of coordination are many, as Chisholm convincingly argues. They are flexible and adaptive; they arise in relation to emerging problems, not in relation to demands of organizational designers; and they may minimize the complexities of interdependencies among organizations. In other words, they are a means of matching structures with environments in a discriminating manner.

Discrimination in matching structures with environments is a major theme emerging from Williamson's edited work. Williamson's contribution "Chester Barnard and the Incipient Science of Organization" provides a concise introduction to his own work, as well as a means of tying his approach to such stalwarts of organization theory as Simon and Barnard. Williamson shares Barnard's conviction that the black box of the organization must be opened. Organizations are consciously created as a means of allowing individuals to coordinate their actions toward a common goal, but when should coordination be attempted through hierarchies? And why hierarchies, rather than other coordinative structures? Briefly, Williamson's answers rest upon two fundamental concepts. First, Williamson borrows Simon's concept *bounded rationality* (there are limits to the cognitive capacities of individuals) and to it adds *opportunism* (individuals attempt to gain advantages for themselves). Hierarchies are a means of economizing on bounded rationality while safeguarding transactions from opportunistic behavior. Whether hierarchies should be used and what form they should take depend upon the characteristics of the transaction. Organizations, or hierarchies, should be matched with transactions in a discriminating manner.

In "The Politics of Structural Choice" Terry Moe asks how Williamson's approach can be extended from private sector organizations to public bureaucracies. The key, according to Moe, is to adopt Williamson's concepts but not the *questions* for which they have been put to use (which have been primarily economic).

Instead, by recognizing political features of the decisionmaking environment, such as public authority, political uncertainty, and political compromise, political (not economic) explanations for the emergence, shape, and performance of political (not economic) structures can be developed. This would involve interest groups, legislators, executives, and bureaucrats bargaining and haggling over structural solutions to common problems (p. 129). Moe thinks that political scientists should no longer relegate themselves to studying the politics of bureaucracy, while leaving questions of organizational

structure to other disciplines. A science of organization that would address interesting political questions requires that the political and the structural be joined.

The thought-provoking work of Chisholm, Williamson, and Moe warrants optimism concerning the development of a science of organization. However, a number of fundamental issues must be addressed before a full-blown science emerges. The schism between the *rational systems* and *natural systems* approaches to organizations must be addressed. Rational systems theorists, such as Williamson, view organizations as being purposefully designed to achieve particular goals. Natural systems theorists focus on the non-rational character of organizations and their creators (e.g., Levitt and March's "Chester I. Barnard and the Intelligence of Learning"). As W. Richard Scott ("Symbols and Organizations") points out, Barnard was both a natural systems and a rational systems theorist. How to bring both strands together in a single science of organization is not clear. Most often, scholars from each approach talk past each other (as demonstrated in these essays). In addition, as Mary Douglas argues ("Converging on Autonomy") culture should be incorporated into a science of organization in order to explain the origin and change of individual preferences instead of assuming them fixed.

While a full-blown science of organization has yet to emerge, both books are highly readable and thoughtful examples of exciting steps taken in that direction by social scientists struggling to understand why organizations matter.

University of Arizona EDELLA SCHLAGER

The Imperial Middle: Why Americans Can't Think Straight about Class. By Benjamin DeMott. New York: William Morrow, 1990. 260p. $18.95.

Making Sense of Politics. By Arthur Sanders. Ames: Iowa State University Press, 1990. 217p. $24.95.

It is commonly observed that U.S. citizens do not think much or deeply about politics. The cacophony of man-in-the-street interviews, radio "talkback" programs, and morselized "factoids" from public opinion polls do not generally provide a very hopeful view of most citizens' comprehension of the political world. Arthur Sanders's in-depth investigation of citizens' opinions about the political world, *Making Sense of Politics*, tends, on the whole, to confirm this assessment. Benjamin DeMott's book, *The Imperial Middle*, provides one explanation of why. Although DeMott's thesis explicitly concerns the erasure of class as a device to explain political outcomes, the mechanisms he describes (particularly the media) can be usefully extended to explain the trivialization of politics more generally.

Sanders's book continues the tradition of Smith, Bruner and White, Robert Lane, and more recent scholars like Jennifer Hochschild and Doris Graber by offering an analysis of citizens' thinking about politics based on intensive interviews with a small (n = 26) randomly selected cross-section of residents of Utica, New York. This method can yield useful insights about individuals' understanding of politics that might easily be overlooked in the conventional public opinion survey. The drawback (Sanders notes almost too frequently) is that the small sample size and variability across interviews makes it impossible to generalize with any confidence about the patterns of relationships uncovered among the subjects. Given these limitations, Sanders manages to do a creditable job of systematically describing the common themes that emerged from his interviews.

The major difficulty with Sanders's study is that while his intention is to probe beneath the surface responses obtained in the large-scale survey setting, his perspective is very much limited by the analytic concepts popularized by this tradition. Sanders's findings concerning the limited understanding of ideological terms and lack of perceptions of differences between the parties echo those from large-scale survey research. His focus on the differences between stylistic and policy-based understanding of the terms of political discourse, however, provides additional insight into the question of why such self-designations tend to predict some political attitudes and behavior even though common understanding of terms like *liberal* and *conservative* is so limited.

The material developed on the centrality of political trust in individuals' judgments about politics is substantially more valuable. Sanders develops a simple typology of patterns of trust based on beliefs about the political system and attitudes toward individuals. Within this typology the attitudes of the *skeptics* (who believe that the system is corrupt but that individuals are basically honest) are worth some attention. Three themes emerge from the investigation of these attitudes. The dominant one has to do with the pernicious influences of money in the campaign process. The second has to do with the perception of the increasing isolation of the politician from the experiences and needs of the contitutents at home, and the third with distaste for compromise and log-rolling. The intertwining of these themes, together with subsequent chapters on the role of personal experience and common sense in shaping citizens' views of politics, does much to explain the popularity of simplistic solutions like the current movement for term limitations.

Sanders's investigation of the themes of freedom, opportunity, and equality provides a window on the thought processes of the "average" citizen that tends to validate the concerns expressed in DeMott's book. However, it is surprising to find a chapter on the values of freedom and equality in politics that makes no reference to Milton Rokeach (*The Nature of Human Values* 1973). While a few of Sanders's subjects (4 of 26) tended to value equality of outcome, and a few *libertarians* (five) tended to value freedom, the dominant frame of reference expressed in the interviews concerned equality of opportunity. The dominance of this notion of a fair chance at the starting gate is the central concern of *The Imperial Middle*.

The citizens that Sanders interviewed are clearly unaware of the concept of class as an explanation for political outcomes. They have all accepted the notion that they are part of the "imperial middle"—not a class but a mass of strivers. If they succeed, they deserve to enjoy the fruits of their success; if they or others fail, it is most likely their own fault. Even those of Sanders's subjects most concerned with equality are disturbed by the unfairness of people being given things they did not work for. According to DeMott, the well-buttressed myth of classlessness immobilizes society and makes it

impossible to deal with clearly evident crises in policy-making: "America's faith in classlessness simultaneously whitewashes unearned advantages" (p. 229). The myth is reinforced by the media (particularly popular entertainment) and by the schools.

DeMott observes that the term *class* has been dismissed from modern language to be replaced by the hegemony of "the imperial middle," to which rich and poor alike seek access. In modern culture the concept *class* has been redefined and trivialized as a rip-off run by prestige peddlers—an advertising gimmick—that wise citizens are capable of recognizing as a sham. Status is temporary and various—no longer fixed but defined by movement and flux. The masking language of class relies heavily on the euphemism *life-style*—giving the illusion of individual choice and reinforcing the ideal of mobility and individual responsibility.

The myth of classlessness is reinforced by the "omni syndrome"—which is reinforced in turn by a multitude of examples of "downward access." If the well-off and advantaged can enjoy the entertainments and life-styles of the "Joe Six-packs" of the world, then, obviously, Joe can step up (if he cares to). But in modern entertainment, Joe is too wise to want to. He can see through the illusions and glitter. It is more fun to watch "Lifestyles of the Rich and Famous" on television than to have to take care of all that fancy stuff.

DeMott's explanation of the role of the school in reinforcing this myth is particularly enlightening. In school everybody starts off with a fair chance. Failure in school is seen to be the result of a lack of intelligence, laziness, or poor judgment. As Sanders's subjects illustrate, the personal experience of the egalitarian school atmosphere reinforces the societal myth that personal error is responsible for lack of success. Both school and mass media work together to replace structural explanations for social and economic outcomes with psychogenic ones that degrade the individual product of the working-class environment by dismissing aspects of experience and culture that provide a different framework from which to understand moral choices in the context of limited opportunities. (DeMott's reanalysis of the motivations of Mary Beth Whitehead [Baby M's genetic mother] is particularly enlightening here.)

DeMott argues convincingly that media constraints reinforce the taste for simplistic thinking rather than educating the public about the complexities of policy decisions and social truth. He proposes the "double truth" of social responsibility coupled with individual responsibility as the necessary context in which to view policy solutions. It then becomes the intellectual responsibility of those who have a part in making policy decisions to reject both sentimentalism and judgmentalism in favor of the complexity that gives each factor its appropriate weight. Properly done, this might prevent the continual vacillations of policy committments driven by underlying assumptions that assign blame for failures wholly to society during some periods and wholly to the individual during other periods. DeMott argues for complexity and balance. It remains to be seen whether such can be achieved in a culture dominated by sound bites and quick fixes.

University of Houston KATHLEEN KNIGHT

The Descent of Icarus: Science and the Transformation of Contemporary Democracy. By Yaron Ezrahi. Cambridge: Harvard University Press, 1990. 354p. $35.00.

The Bittersweet Century: Speculations on Modern Science and American Democracy. By Paul N. Goldstene. Novato, CA: Chandler & Sharp, 1989. 220p. $22.95.

For all their pervasive impact on human life in the twentieth century, science and technology remain alien and forbidding territory to most political scientists excluded from the customary domains of scholarly investigation. It is refreshing, therefore, to see two new books that take on the challenge of integrating science into the analysis of U.S. democracy. Though markedly different in style and substance, these books together send a welcome message that science and technology carry too much explanatory force to be ignored by contemporary political theory.

Yaron Ezrahi's capacious study looks across some four centuries of cultural history to find connections between science and technology and the forms of liberal–democratic politics. The practice of submitting scientific claims to public witnessing during the Enlightenment and the corresponding decline in the authority of private and esoteric knowledge made it possible, in Ezrahi's view, to reconcile democratic participation with centralized political action. Modern science, with its emphasis on visual, experimental demonstrations of truth, transformed the relationship between the governors and the governed. A *celebratory* visual culture, in which the monarch was endowed with superior claims to knowledge—a status symbolically affirmed by the king's all-seeing position in the Stuart court theater—gave place to what Ezrahi terms an *attestive* visual culture based on a presumed equality of perception between the performing agent and the spectator public. Science and technology thus irrevocably altered the space for public action, requiring political actors to be held accountable against norms of objectivity, impersonality, and representativeness. In a scientifically conditioned culture that dissolved hierarchical differences and credited all viewers with equal capacity to discern reality, an instrumental paradigm for politics naturally flourished, for the success or failure of political actions could now be assessed with reference to common understandings of facts and the truth.

To demonstrate these parallels in the evolution of scientific and political culture, Ezrahi musters a kaleidoscopic array of evidence from the history and sociology of science, art and architectural history, sociology, and political theory. His intellectual range is extraordinary; and if the reader feels captive at times on a compulsory tour of the major monuments of Western social thought, the vista is seldom uninteresting, the guide is as literate as he is learned, and the argument is by turns compelling and provocative.

The author's talents are splendidly in evidence in a sequence of chapters that link the experimental strategies of scientists like Robert Boyle, Joseph Priestley, and Michael Faraday with the attestive orientations expressed in the works of contemporary painters, poets, and political theorists. Ezrahi shows how the culture of attestation won particularly influential adherents among U.S. political thinkers, from the federalists' espousal of a "science of politics" to the U.S. pragmatists' adoption of the "republic of science" as a model for democratic political behavior. The United States' love affair with

action over thought and experience over inference was reflected, Ezrahi argues, in a social science tradition that rejected Durkheimian notions of the duality between individual and society in favor of a view of public order arising from the mutual discipline of multiple individual actions.

Coming to the end of the twentieth century, however, Ezrahi sees a disintegration of the United States' commitment to the attestive culture of Enlightenment science and its political implications. Developments within science are partly to blame. Thus, Ezrahi believes that the discovery of irreducible uncertainty in physics has fundamentally weakened the power of science to legitimate political action. If the attestive eye can no longer establish the reality of physical events, still less is it capable of verifying the consequences of social or political action. Science has become "privatized" in the sense of ceasing to be a potent resource for public political discourse. In place of the earlier attestive culture, there has arisen a new *reflexive* visual orientation, wherein the act of seeing (whether with the human eye or the mechanical eye of the camera) shapes that which is seen and the observer becomes a constitutive element of the observed "reality." Ezrahi attributes to the decline in the public authority of science a variety of shifts in late twentieth-century U.S. politics: a loss of faith in instrumental public action; a preoccupation with short-term, aesthetically gratifying political objectives; a rise in eclectic pluralism; and heightened criticism of expert authority.

A serious flaw in this otherwise admirable study is Ezrahi's tendency to treat science and technology as wholly independent variables, rather than as forces that both influence, and are influenced by, politics. The latter view is much more in keeping with current work in the history and sociology of science, and the failure to represent it is a curious blind spot in a book that generally makes such sophisticated use of these sources. The attempt to pull the analysis into the present is also fraught with problems; for here, as the author recognizes, "the absence of an historical perspective limits our ability to interpret recent experience and distinguish significant from marginal developments" (p. 239). Recent experience, in fact, leads one to question the more optimistic, as well as the more pessimistic, strains in Ezrahi's concluding chapter. In the aftermath of a "Nintendo war" in the Gulf, sustained by high levels of public enthusiasm, one wonders whether the attestive visual culture and the manipulative instrumentalism it supports are not alive and well, at least in the sphere of high-technology military action. Perhaps more significant is Ezrahi's omission of environmentalism as a possible counter to the privatization of science; this newly powerful political ideology is not only deeply (if ambivalently) rooted in science but also appears to be stretching out the timespan for legitimate instrumental action in Western democracies. One misses in this context any discussion of the possible globalization of the attestive culture through such transforming visual images as the fragile and lonely planet Earth suspended in space.

Loose ends are more conspicuously evident in Paul Goldstene's book, which starts with some of the same premises as Ezrahi's but comes to startlingly different conclusions about the impact of science and technology on politics. The "bittersweet century" of Goldstene's title serves as the leitmotif for a series of short, reflective essays decrying the United States' deviation from a truly egalitarian democratic ethos. Liberalism, Goldstene contends, has foisted on the United States a system of elite rule founded on the superior claims to knowledge of the few and supported by the corporate organization of technology. In positing an alliance between liberalism and technology, Goldstene acknowledges that the applications of science can be appropriated by politics but evades the larger implications of this argument by drawing an artificial boundary between science and technology. Like Ezrahi, Goldstene associates science with the core assumption that all individuals are equally situated in their capacity to know. Unlike Ezrahi, however, he believes that science is on the point of exerting a redemptive force on U.S. politics by liberating it from the tyranny of elite rule. Goldstene draws messages of optimism from modern biology (which has undermined the basis for racist and hereditarian thought) and from relativity and quantum physics (which have subverted positivist and privileged claims to knowledge). "The logic of science," he concludes, "results in democracy" (p. 153)—a sweet promise that offsets the for him bitterly elitist impulses of the twentieth-century United States.

A lack of specificity and a partial reading of the record of science make these arguments highly problematic. Advances in biology, for example, may indeed have eroded the basis for nineteenth-century racist views and the naive eugenics of the early twentieth century. But genetics, molecular biology, and sociobiology are reinjecting a note of determinism into explanations of human behavior that is all the more persuasive because its scientific foundations are regarded by many as unimpeachable. In ascribing a democratizing effect to modern physics, Goldstene overlooks a point of which Ezrahi is acutely conscious—that theoretical progress in this field has assailed the value of science itself as a way of knowing reality. Nor is there much support in current historical or sociological studies for the boundary Goldstene seeks to construct between science (which he idealizes as a creative striving for knowledge) and technology (its bureaucratic and corporately structured counterpart). Ezrahi comes closer to prevailing opinion when he treats science and technology as equally important resources in supporting the instrumental paradigm of liberal–democratic politics. There is, finally, little in Goldstene's account to explain how the democratically invigorating force of science will win out over the existing "technobureaucratic" organization of U.S. politics. Without such explanation, and with only weak connections to recent theoretical studies of science and technology, the book remains little more than a wistful, fin-de-siècle footnote beside Ezrahi's more enduring work of scholarship.

Cornell University SHEILA JASANOFF

Laws, Men, and Machines: Modern American Government and the Appeal of Newtonian Mechanics. By Michael Foley. London: Routledge, 1990. 284p. $47.50.

Michael Foley has thoroughly demonstrated the extent to which U.S. scholars, journalists, and political leaders, as well as the public, utilize the language of Newtonian mechanics to analyze and evaluate political institutions. Specifically, Foley points to the centrality of

the concept of *balance* in discussions of U.S. political institutions, particularly the separation of powers. Foley demonstrates that the manner in which the separation of powers is routinely treated "has given the separation of powers every appearance of being a real physical system composed of discrete units interconnected with one another to produce an enclosed pattern of direct physical contact, in which the constituent parts change in response to the forces exerted upon them by the others" (pp. 72–73). In short, inherent in the discourse citizens use to discuss the separation of powers is the paradigm of Newtonian mechanics.

Interestingly, in making the case for the influence of the Newtonian paradigm on U.S. political discourse, Foley is not arguing the influence of Newtonian ideas on the political thought of the framers of the Constitution. Indeed, he suggests that debates regarding the relative impact of Newtonian ideas on the framers' political thought are "not only inconclusive, but largely unproductive" (p. 50). Foley analyzes contemporary treatments of U.S. political institutions and reaches the conclusion that the idea of balance, understood in Newtonian terms, is inherent in the U.S. political culture. Indeed, he suggests that the "cult of balance" is "the predominant and guiding conceptualization of American government" (pp. 225, 233).

The significance of the Newtonian paradigm was made most apparent, Foley illustrates, during the "imperial presidency" crisis of the 1970s. After decades during which the growth of the presidency was described, explained, and justified in biological or organic terms (i.e., as a vital institution adapting to changing conditions), the imperial presidency crisis brought back to the fore the language of mechanical balance; that is, the crisis was widely interpreted as an imbalance of power, a deviation from some ideal balance of power between Congress and the presidency that would almost automatically (i.e., mechanically) be recaptured through the resurgence of congressional power. In a final chapter, Foley suggests that the idea of a mechanical balance also pervades U.S. discussions of the judiciary, federalism, the federal bureaucracy, the mass media, and other institutions.

Foley is not interested in evaluating the utility of the Newtonian paradigm as an analytical tool or as a standard of political evaluation but merely demonstrating, through remarkably extensive citation of academic and popular sources, the extent to which the paradigm dominates U.S. discourse concerning national political institutions. Herein lies both the strength and the weakness of *Laws, Men, and Machines*. Foley's thesis is unchallengeable, but its significance demands exploration.

It is beyond question that the separation of powers is nearly universally talked about as a kind of mechanical balance that historically tilts to one side or the other before some equilibrium is achieved. But what are the consequences that flow from conceptualizing the separation of powers (and, e.g., federalism) in these terms? What are the implications of utilizing the idea of balance as a standard of political evaluation? Foley does not address these questions.

What are the normative commitments that underlie the mechanical conceptualization of U.S. political institutions? With what view of democracy, for example, is it most consistent? What sorts of political reforms benefit from the mechanistic paradigm? To what extent are the sorts of political reforms suggested by Lloyd Cutler and the Committee on the Constitutional System, who propose to create a more parliamentary style of government hindered by the "cult of balance"? In short, what is the political significance of the cultural attachment to Newtonian mechanics?

It may be unfair to criticize an author for what he has neglected to write. However, it seems clear that the flaw of *Laws, Men and Machines* is not that Michael Foley is wrong (for certainly, he is not) but, rather, that he fails to demonstrate the significance of what he has found.

University of Texas, San Antonio JOHN R. BAUER

Boston against Busing: Race, Class, and Ethnicity in the 1960s and 1970s. By Ronald P. Formisano. Chapel Hill: University of North Carolina Press, 1991. 323p. $34.95 cloth, $12.95 paper.

When violence erupted in Boston in the early 1970s as school buses transported students pursuant to a federal court order to desegregate the public schools, the nation was shocked. How could this citadel of culture, home to over a hundred colleges and universities, the launching point of the American Revolution, and a hot bed of antebellum abolitionist thought, become a bastion of resistance to African–American claims of civil rights and equality under law? The initial understanding, which has taken on the aura of conventional wisdom, is that resistance came from racist Irish–Americans in South Boston and Italian–Americans in East Boston. In *Boston against Busing*, Ronald Formisano offers a richly textured explanation that accents racism, of course, but also class and ethnicity as key variables in understanding what happened in Boston. His account is a substantial contribution to the literature not just on Boston but on school desegregation generally.

While earlier commentators focused on extremist whites, Formisano seeks to understand the attitudes, beliefs, and strategies of the "silent majority"—whites who opposed racial busing but did not resort to tactics of violence and civil disobedience. Formisano sympathetically portrays white middle- and working-class parents who believed in the rule of law and were prepared to send their children to desegregated schools but who feared—reasonably at times—that violent clashes would occur and resented the political and economic powerlessness that left them unable to change the law or send their children to private schools.

Formisano's analysis of the tactics and strategies of the antibusers suggests that they ironically built their campaigns on lessons learned in the African–American protests of the 1950s and 1960s and in Vietnam War protests of the 1960s. In this connection, he highlights the important role of women in the antibusing movement. Although these working-class women shared few values with their more affluent sisters in the Women's movement, the antibusing movement launched a number of women on career paths that took them away from their previous role as homemakers.

Formisano joins a chorus of voices that looks rather bleakly at the overall experience with school desegregation. In Boston between 1973 and 1987, the reality of "white flight"—parents either moving to suburbs or placing their children in private schools—resulted in a decline in the percentage of white students in the Boston public schools from 60% to 26%. Not surprisingly, this

process left behind lower-class white students in school with lower-class black students. When economic conditions permitted, black parents also chose to remove their children from the public schools.

By recognizing the complexity of antibusing sentiment, Formisano sheds light on the role of economic class in school desegregation. In Boston, affluent suburban whites supported the policy objectives of desegregation, knowing full well that their communities would remain rigidly segregated: "The lower classes did the desegregating, the middle classes did the fleeing, . . . while the affluent were exempt from the start" (pp. 232–33).

He is less persuasive in demonstrating the role of ethnicity in the school desegregation crisis. He broadly characterizes certain traits as "Irish," "Italian," and so on. However, just as antibusing sentiment was not homogenous, neither was Boston Irish society. Ardent Irish supporters of desegregation included not only U.S. District Judge W. Arthur Garrity, Jr. but also Senator Edward Kennedy and many civil rights attorneys. In West Roxbury and Hyde Park, the antibusing response was quite different from that of predominantly Irish South Boston, even though Irish–Americans constituted a substantial segment of each community.

An intriguing element that Formisano does not elaborate is the role played by law and the legal system—particularly federal law and federal judges—in the Civil Rights movement. He notes that elected state officials typically refused to take any responsibility for the process. Looking at the larger canvas of the national Civil Rights movement, federal judges who are appointed for life played an enormously instrumental role in desegregating various public facilities. Their independence (by virtue of their lifetime positions) allowed them to act in a way that was not beholden to petty politics or local concerns.

University of Arizona College of Law ROBERT JEROME GLENNON

Extraordinary Measures: The Exercise of Prerogative Powers in the United States. By Daniel P. Franklin. Pittsburgh: University of Pittsburgh Press. 172p. $24.95.

U.S. citizens pride themselves on possessing a government "of laws, not men." Yet our national history is crowded with examples of extraordinary actions taken by the officers of the republic in response to challenging circumstances from Lincoln's conduct of the early Civil War to the internment of Japanese–Americans during World War II to the aggressive exercise of judicial review by federal courts in recent decades. These actions and a host of others are based on the prerogatives of the state and its officials, rather than explicit constitutional grants of power. As such, they pose a problem for the U.S. political system, says Daniel P. Franklin in this brief volume. "How," he asks, "can plenary power be reconciled with democratic principles, and how can citizens be protected against governmental abuse of authority in times of emergency?" (p. 3). Franklin does not provide a complete answer to his own question but, instead, offers a primer on the issue of prerogative powers in the U.S. constitutional system. His book is a valuable, well-written introduction to a subject that has received insufficient attention in the literature on U.S. politics.

Franklin begins by noting that there are essentially two ways of dealing with the issue of prerogative powers in the United States. The first is to see them as extraconstitutional. This position maintains the image of pristine constitutional government ("of laws") while accomodating the need for "extraordinary measures" in times of emergency. The other view is that the Constitution is big enough and flexible enough to encompass unwritten prerogative power, and that, as a nation, we are safer with such authority inside rather than outside the constitutional domain. Franklin clearly advocates the latter position, makes a case for it, and examines what it means for government.

His argument stands on two legs. First, the founders of the U.S. republic saw the traditional royal prerogative recognized in medieval political theory and European practice as invested in the new U.S. regime (moving, as he puts it, "from king to country"). Second, the alternative, extraconstitutional view of prerogative is dangerous because it makes such powers illimitable. Therefore, if we are to understand our system, he believes it incumbent on us to examine the nature and limits of prerogative powers held by the different branches of government under the Constitution. He not only reviews the well-trodden ground of presidential prerogative but explores the comparative wilderness of congressional and judicial prerogatives.

Franklin's discussion of presidential prerogative is valuable in two respects. First, while it only scratches the surface of this subject (which has received extensive attention from political scientists, legal scholars, and historians) it does highlight most of the major issues involved. But, more importantly, it also shows how foreign policy prerogatives, administrative prerogatives, and even rhetorical prerogatives fit together in a larger picture of presidential power.

The book opens new areas for most readers in its discussion of congressional and judicial prerogatives. Franklin shows how a number of congressional powers and practices really stem from prerogatives and must be understood as such—namely, the investigatory power of the legislature, the use of legislative vetoes (continued even in the face of contrary court decisions), and even control over certain internal procedures. For the courts, the major prerogative of interest to Franklin is judicial review. His discussion of it, within the larger context he is considering, offers a useful perspective on an important topic.

The book's brief survey of these issues leaves the reader wanting more. Perhaps the chief flaw of the book is that a number of subjects are either insufficiently covered or dismissed in a perfunctory way. For example, Franklin never really examines the alternative (extraconstitutional) view of prerogative. Nor does he examine congressional control of internal procedures in sufficient detail. Moreover, there is not as much structure in the internal organization of his chapters as I, at least, would have liked.

At the same time, however, the reverse of these flaws constitutes the book's chief virtue. It provides a brief, readable introduction to an important topic; a global perspective on a subject that is usually treated in a much more fragmented fashion; and an extensive (albeit in-

complete) set of citations for further reading and research. It is a book that anyone seriously interested in U.S. politics ought to read.

Miami University (Ohio) RYAN J. BARILLEAUX

The National Party Chairmen and Committees: Factionalism at the Top. By Ralph M. Goldman. Armonk, NY: M. E. Sharpe, 1990. 649p. $49.95.

This is the only book I know of that so thoroughly details the history of U.S. party leadership from the beginning of the republic to 1960. In Goldman's view, this history teaches at least three general lessons: (1) that politics and political partisanship are basically about the management and socialization of conflict; (2) that political factionalism at the top generally has roots in the electoral foundations of the party system; and (3) that federalism is often a solvent of party, making party nationalization a slow process that is accomplished only tenuously in the late twentieth century.

Adopting E. E. Schattschneider's view that politics is the management and socialization of conflict, Goldman focuses virtually all of his attention on the history of intraparty conflict that has plagued the life of the national committees. In this connection, the author points out that the roots of party faction existed from the beginning of the republic in spite of a homogeneous electorate consisting mainly of property-owning white males. Given an electoral foundation for consensus, one would think there would be little room for a fiercely partisan politics at the top. However, even though the Federalists were conceptually antiparty and refused to take grass-roots organization seriously, that did not prevent the emergence of two rival and cohesive leadership cadres centered around Hamilton and Jefferson. True, these early "parties" amounted to little more than personal networks. But the absence of fierce partisanship in the electorate did little to generate consensus among elites. Hence, while Goldman tends to suggest that the grounds for factional division at the top are generally traceable to the characteristics of the party electorate, this is not universally true. The early party coalitions remained fluid and personality-based at least until the Civil War when sectionalism became the basis of organized factions.

But sectionalism was also responsible for breaking apart organized factions, suggesting that party is not always the solvent of federalism that V.O. Key suggested it was 30 years ago. During realigning periods, groups at the top splinter and resettle with the formation of new issue cleavages in the electorate. Drawing our attention to the decentralization of the U.S. party system, the book contains many stories about how party factions in state organizations eventually translated into factions at the national level.

In addition, we learn that the national chairmen of each party were always the creatures of a state party system (usually New York, Pennsylvania, or some other politically dominant state). As such, "the state origin of chairmen was undoubtedly related to the facts that these states were also major sources of presidential candidacies, state party influence at the national conventions, and, in victory, major recipients in the distribution of federal patronage" (p. 554). That the northern industrial states controlled the party leadership for much of history explains why the Republican party was never supplanted by the Progressive insurgency of the early twentieth century, and why Democrats weathered the Populist movement: "The degree of national committee contact with state organizations often depended upon which was the dominant state faction" (p. 567).

Not until the 1920s did the parties begin to consolidate power at the top. Factionalism did not cease, but it did seem to lose some of its sectional character. Simultaneously, at the electoral level, the 1920s marked a temporary reconsolidation of the party system. But for the first time, the national party organizations seemed to have acquired an independent existence, apart from state-level activity. Issues had become national in scope and the universal impact of the Great Depression would only reinforce this nationalization of party politics. Faction within the parties now had a different foundation. Rather than sectional splits, intraparty conflict was now based upon divisions between the executive and legislative branches (chap. 19).

Again and again, a familiar pattern of party organizational surge and decline appears in Goldman's history. Democrats, as the out-party, unify to elect a president. Then, once the goal of winning is no longer there to unify, intraparty strife sets in over issues and spoils. Republicans, now the out-party, struggle to reunify, recapture the presidency, then dissolve in strife and turmoil.

This regular series of peaks and troughs corresponds closely to electoral trends in party support. Whether the causal direction runs from electoral behavior to elite behavior or the other way around is not always clear from Goldman's chronological treatment. Except for the early period of party formation, it appears that elites act in response to local and sectional electoral pressures. The initial impetus for the formation of a Democratic National Committee in 1848 was the need for party leaders representing different regions to consult and contest presidential issues and candidacies (p. 556). But it is still true that factions that stubbornly obstructed party cohesion have had regional electoral roots at least up until the 1920s and 1930s. It is just not clear that the national committees succeeded in containing faction on their own, as exogenous actors in the party system. The rare periods of intraparty cohesion appear to be coincidental to periods of electoral stability, rather than the accidents of inspired leadership or the result of institutional developments in the national committee bureaucracy.

With these kinds of questions in mind about the relationship between electors and elites, I believe that this book is a natural companion volume to histories of U.S. electoral behavior, such as James Sundquist's *Dynamics of the Party System* (1983). What is missing is a treatment of national party developments since the 1960s.

Goldman concludes on a controversial note, arguing that the national committees have grown in size, permanence, and political importance (p. 576). But while the committees may be better staffed than ever, that does not mean they are politically more important. All trends indicate that in many respects they are less important, even as they have become more bureaucratized. While the national committees are more independent of state and local organizations, at the same time, they have lost influence with the emergence of independent party organizations that have developed within the House

and Senate and with increasingly independent candidates that have turned to their own organizations for fund-raising and support.

Finally, the Goldman account does not address the issue of national committee response to divided government, a post-1960 phenomenon. It is just not as simple as in-party versus out-party any longer. Is the Republican National Committee currently an in-party or an out-party? Winning the presidency without winning at least a closely competitive margin in the Congress does not make an in-party. Today, with comfortable margins in the House and Senate, Democratic chairman Ron Brown is able to conduct himself very much as if he is running the in-party. Hence, these recent developments complicate the discussion, making it difficult to pronounce such confident judgments about the relevance of an in-party/out-party understanding of national committee power.

Senate Labor Committee JAMES GIMPEL

Culture and African American Politics. By Charles P. Henry. Bloomington: Indiana University Press, 1990. 140p. $22.50.

Black Americans' Views of Racial Inequality: The Dream Deferred. By Lee Sigelman and Susan Welch. New York: Cambridge University Press, 1991. 214p. $22.95.

These two very different books on black politics have a common theme, namely, the diversity and complexity of black political attitudes. The controversy over the nomination of Clarence Thomas for the Supreme Court is just one demonstration of the importance of this theme.

The Sigelman and Welch book, a secondary analysis of national survey data on black political attitudes, examines a nontraditional topic using traditional methodology. In contrast, the Henry book uses a very nontraditional approach, analyzing folklore, blues, and black churches to identify the cultural roots of black politics.

Sigelman and Welch make a major contribution to our understanding of racial attitudes. Relying heavily on the 1981 and 1986 ABC News–*Washington Post* telephone polls of blacks, the authors find a continuing massive perceptual gulf between blacks' and whites' attitudes toward racial inequality. Blacks and whites differ dramatically in the amount of prejudice and discrimination they see and in their explanations of this racial inequality. Blacks do not believe the playing field has been leveled, while many whites insist that it has been or has even been tilted toward blacks, who are "taking over."

This study of the racial gap, which receives less media attention than the gender gap, fills the scholarly void with a well-written, clear, concise examination of perceptions of the amount of prejudice and discrimination against blacks, explanations of why blacks are worse-off than whites, and preferences for policies to end racial inequality. Synthesizing previous work while both confirming and challenging expectations, it should be widely used by those who want to probe more deeply into these topics.

Elaborating on the two separate but overlapping worldviews, Sigelman and Welch argue that blacks see themselves largely as victims of white-dominated society. Seeing white hostility toward blacks as widespread, 60% of the black respondents say they have been affected by at least one type of discrimination in education, housing, jobs, or wages. Two, three, four or even more times as many blacks as whites perceive white discrimination. Similarly, blacks typically point to structural sources of discrimination, while whites downplay their own responsibility for black problems.

Sigelman and Welch also argue that the attitudinal differences among blacks are as important as the differences between blacks and whites. Most blacks do not forswear all black responsibility for inequality, nor do most whites forswear all white responsibility (p. 107). But blacks and whites differ among themselves in the weight they give to structural and attributional–dispositional factors. The primary variable associated with differences in explanations of racial inequality is the extent of perceived discrimination against blacks. There are also differences in black attitudes toward busing, affirmative action, and aid to blacks and the poor; but blacks are generally more activist than whites. Both blacks and whites, are less enthusiastic about government programs than they were during the 1960s.

The authors are commendably explicit about the limitations of the available data and the measures and methods of analysis, acknowledging the coarse measures of discrimination and their inability to make precise comparisons over time. We need to know much more about variations over time and those associated with regions, local contexts, racial consciousness, party identification, and level of political activity. Sigelman and Welch's book cries out for replication and elaboration.

Charles Henry's book is also an ambitious work, distinguished by the crossing of disciplinary lines. His search for the cultural roots of contemporary black political behavior, actually concentrates, after a chapter on African American folklore and the blues, on the way black churches and black leaders have incorporated and modified the cultural roots.

His main argument is that African American politics has a distinctive style and combination of worldviews. The black style, in contrast to mainstream U.S. politics according to Henry, includes a wider variety of political expression and demand, emphasizes identity and self-respect that cross class lines, depends on rhetoric and charisma, and is rooted in a black church tradition that blends sacred and secular vision.

The argument is disjointed, and the use of material from other disciplines does not do justice to earlier scholarship on African American culture. Nevertheless political science will benefit from this broader exposure. Critiquing earlier studies of black leaders for neglecting the participatory component in their rhetoric, Henry says the church-based militant tradition was the catalyst for the Civil Rights movement that transformed black politics. He argues that black cultural styles mean that important political action occurs outside the voting booth in revolts, protest, and direct confrontations in the tradition of "black bad men" (p. 48).

The root of both Martin Luther King's and Jesse Jackson's success, according to Henry, is the black church tradition's emphasis on faith, collectivism, suffering, and authoritarianism and linking of the Bible's moral lessons to current political events. Drawing parallels between Marcus Garvey's and King's efforts to construct a civic religion, Henry argues that Jackson's

campaigns were a reassertion of Martin Luther King's protest style of leadership stressing self-respect, self-help, and faith in the future. Henry challenges Adolph Reed's criticisms of Jackson's antidemocratic and organic style, saying that Jackson represents "a logical extension of black cultural politics with system transforming potential" (p. 86).

Henry concludes by affirming that black culture has empowered blacks to survive oppression and promote a community of interests and criticizing black neoconservatives for obscuring this vision. According to Henry, black politics is linked both to U.S. politics and to the struggles of oppressed groups worldwide. Questions can be raised about his interpretations of both black political culture and mainstream U.S. political culture but not about the importance of the questions he is addressing.

Both these books contribute to correcting some of the limitations of traditional political science by changing the nature of the discourse. In stimulating more complex and comprehensive theories and models, they demonstrate again the way the issue of race is transforming not only U.S. politics but also the discipline of political science.

Wellesley College DALE ROGERS MARSHALL

The Changing World of the U.S. Senate. Edited by John Hibbing. Berkeley: Institute of Governmental Studies Press, 1990. 403p. $24.95 paper.

Elections, Representation, and Congressional Voting Behavior. By Robert A. Bernstein. Englewood Cliffs: Prentice-Hall, 1989. 117p. $18.60.

Research on the functioning of political institutions has been one of the most important topics of study in U.S. politics. Do institutions represent the views of those governed? How have the internal operations and level of representation changed over the course of the nation's history?

Two recent books provide very different perspectives on this subject. An edited volume on the U.S. Senate compiled by John Hibbing presents a series of high-quality articles by many of the leading scholars in the field (based on an October 1988 conference at the University of Nebraska, Lincoln). These articles deal with several general subjects: the historical development of the Senate, internal operations, and elections and representation.

Part 1 is must reading for all those interested in the historical evolution of the upper chamber. It has chapters dealing with reconstitutive change in the Senate from 1789 to 1841 (Swift); party unity from 1880 to 1920 (Brady, Brody, and Epstein); and party voting from 1877 to 1986 (Hurley and Wilson). Because these three pieces are so well written and clearly conceptualized, they perform a valuable service in the field of congressional studies. The history of Congress in general has been neglected by political scientists, and these chapters go a long way toward filling that gap.

Part 2 emphasizes the internal operation of the Senate. Among its articles are materials on party leadership (Patterson), leadership institutionalization (Canon), multiple referral (Davidson), unanimous consent agreements (Smith and Flathman), and televising of Senate proceedings (Fenno). The Fenno piece is particularly interesting because of its attention to a part of the congressional process (the internal media) that has clearly grown in importance in recent years. Fenno uses the Senate debate over television to examine institutional change and members' arguments about the type of institution that is desired.

Part 3 has a number of very important discussions about elections and representation. It includes articles on policy voting and representation (Wright), California Senate races (Jacobson and Wolfinger), the role of challengers (Squire), House members running for the Senate (Copeland), the dynamics of Senate races (Stewart), split party control (Oppenheimer), and campaign spending (Abramowitz). Wright's article presents a novel use of state-level data to evaluate senatorial responsiveness, while Jacobson and Wolfinger use the 1980 and 1986 California Senate races to demonstrate the importance to incumbent success of strategic framing in the campaign.

The final group includes a series of roundtable essays by Polsby, Ripley, Fiorina, and Matthews. These articles integrate the research findings of earlier chapters and point the direction for future work. The essay by Polsby presents an informative reflection on the changing role of the Senate. Fiorina calls for more comparative research and the need to be more explicit about the theoretical underpinnings of House–Senate differences.

Bernstein's is a very different type of book. It is what I would call a "message" book because of its clear enunciation of a simple theme, which he calls the "myth of constituency control." In this logically argued and concise book, he argues that citizens rarely know or respond to policies supported by their representative, that constituencies rarely reward or punish incumbents on the basis of policy preferences, and that members therefore are not controlled by constituencies.

One strong point of this book is its review of a considerable amount of the research that has been done on the subject of constituency control. Bernstein has done an admirable job of synthesizing a diverse range of material on public opinion and legislative voting decisions. At times, though, the author equates constituency control with issue control. Almost all the evidence gathered relates to constituency control through the communication of policy views.

This ignores recent arguments (e.g., the economic voting thesis) that constituents can engage in democratic control through more broad-based types of evaluations. If voters can exercise significant control using retrospective judgments, citizens may not be as poorly armed as Bernstein suggests. In addition, new models suggest that members can be controlled through the anticipation of citizen preferences. Arnold, for example, demonstrates that anticipated reactions can be as important to members of Congress as more commonly studied forms of after-the-fact control.

But for scholars interested in a concise and readable review of past research on representation, this book is very useful. It is well written and clearly argued. The author deserves credit for his careful attention devoted to testing key hypotheses in the representation area. The volume can be assigned in introductory courses on elections and Congress.

Brown University DARRELL M. WEST

The Reach and Grasp of Policy Analysis: Comparative Views of the Craft. By Richard I. Hofferbert. Tuscaloosa: University of Alabama Press, 1990. 192p. $19.95.

Defining policy evaluation as the use of social science research to measure under what conditions public programs achieve their objectives (p. 4), Hofferbert draws upon his experience as an academic policy researcher and student of policy analysis in developing countries to examine the opportunities and problems confronting this process.

Part 1 reviews the contemporary history of U.S. policy analysis since the halcyon "social engineering" years of Lyndon Johnson's Great Society and examines the activities of university-based policy research using cases with which Hofferbert is directly familiar. Chapters 2 and 3 provide useful examinations of extension research in land grant universities and practical challenges facing university researchers engaged in public-funded program evaluation research. It is possible to derive from these chapters hypotheses that could be tested through surveys of university and other research centers. Part 2 examines the obstacles and opportunities confronting the export of U.S. policy analysis methods and epistemological assumptions to the analysis of developing nations (particularly Turkey and Nepal), using previously published research.

Hofferbert argues that political scientists engaged in policy research engage in either *product* or *process* research. While the former focuses on policy performance and is useful to legislators who desire independent means of evaluating agencies, the latter has greater long-term utility to program administrators. Moreover, process research also stands the best chance of endearing the discipline to policy analysis consumers, since it examines the relationship between the organization of authority as an independent variable and the accomplishment of policy objectives.

Hofferbert asserts that the "green years" of Great Society optimism minimized the long-term relevance of the work of policy analysts cloistered in universities and maximized their political vulnerability when social policies did not meet political expectations quickly or decisively enough. This has resulted in the institutionalization of policy analysis in places out of sight of many political scientists. Within state and local government institutes, agricultural extension centers, and similar places, however, policy research has flourished.

While there is much to warrant such an interpretation, rising overhead to sustain policy research in traditional academic departments, the colocating of some policy analysis fields (e.g., environment and energy) into multidisciplinary research centers, and the fact that the social problems being analyzed were larger than initially envisioned, as noted by observers such as John Schwarz in *America's Hidden Success,* (1988) may also have contributed to the apparent decline in support for social-science-based policy research.

Whether using firsthand experience in the evaluation of elderly services in New York state or applying contemporary policy analysis to evaluating Turkish economic development, the book is at its best when it sticks to its pragmatic theme—that effective process analysis requires a basis of trust between researcher and policy practitioner, as well as frequent consultation (p. 19), adaptation of research methods to field conditions, and resolution of tension between a disciplinary reward structure that often appears to penalize "practical" research.

Unfortunately, Hofferbert does not explore the phenomenology of effective political science participation in policy research. As Hofferbert's cases illustrate, even university-based policy analysis is often conducted in interdisciplinary environments where project research agendas are established by several disciplines and methodologies and approaches are defended before committees unfamiliar with the idiosyncrasies of the researcher's discipline. Yet there is broad appreciation for the political variables responsible for policy success or failure. How are problems defined in such a context? How are research strategies developed to target these political variables in these environments effectively? This omission is ironic, since Hofferbert does explore the independence of political variables in successful policy implementation in developing countries with pluralistic political participation.

Other topics salient to the subject are also omitted, such as the rise of policy think tanks, both liberal and conservative, giving rise to rapid evaluations of existing public programs. The rise of these institutions may, in part, explain why, despite the decline in pure social science research dollars for studying social programs, the amount of policy analysis output (sometimes masquerading as objective scholarship) still arguably increases. One wishes that Hofferbert had generalized his analysis to permit a weighing of factors intrinsic to policy evaluation success, particularly in his discussion of extension research at land grant colleges where problem-oriented research has been conducted by social scientists in natural resource, economic development, and technology transfer policies. Taken as far as his cases permit, Hofferbert provides a good starting point for exploring these issues.

Oak Ridge National Laboratory DAVID LEWIS FELDMAN
University of Tennessee, Knoxville

Race and the Decline of Class in American Politics. By Robert Huckfeldt and Carol Weitzel Kohfeld. Champagne–Urbana: University of Illinois Press, 1989. 205p. $27.50.

The central arguments of this book deserve close study by both political scientists and political strategists. In addition to an account of the rise of race and the decline of class and income distribution as fundamental organizing themes of U.S. electoral politics, the authors develop original approaches to these problems: how to secure the representation and fundamental rights of blacks and other minorities, blunt and alleviate racial polarization in the party system, secure more effective representation of class, and prevent the withering away of the Democratic party.

An unusual book, replete with mathematical models and computer simulations (many readers will have tough going there), it also offers many pragmatic suggestions for restructuring the system. The analysis incorporates historical accounts and well-displayed quantitative data and is driven by a profound concern for the quality of democracy in the United States. It has much to offer on all these fronts. Another strength is the bringing together of political vision, quantitative theory, and

data; flaws of style in an often-well-written book are a result of the ambitiousness and unwieldiness of that effort. In addition, it will anger many people because it cuts into the premises on which their political experiences, positions, identities, and loyalties are based.

The empirical foundation of the book is the thesis that "race has persistently obstructed the development of class politics across more than one hundred years of American political history" (p. 188) and is often "a wedge that disrupts lower-class coalitions, thereby driving out class in our political arrangements" (p. ix), so that "the decline of class as an organizing principle in contemporary American electoral politics is directly related to the concurrent ascent of race" (p. 2)—with the result that race is now the most important line of conflict in U.S. politics, while the politics of class has been virtually eclipsed.

The political eclipse of class is dangerous: "So long as a society is class-structured, democratic politics must also be structured by class if it is to have legitimate meaning and content" (p. 191). The increasing significance of race over class politics was not the substitution of one legitimate dimension of conflict for another: "Unlike class, race is not an appropriate basis for the organization of democratic politics because, in a democracy, both parties must be concerned with the egalitarian treatment of blacks and white. . . . Both the Democratic and Republican parties must be parties of civil rights" (p. 191).

The fact that 9 out of 10 blacks support the Democratic party has led many whites whose racial attitudes make them uncomfortable in a party that gets a significant part of its support from blacks or that emphasizes racial issues to leave the party. Racial polarization of the parties provides such people a reason for leaving the Democratic party on racial grounds. Because defections by whites in many settings are greater in number than conversions to it by blacks, the Democratic party has been severely disadvantaged.

Thus, the racial polarization of the parties leads to a system in which it continues to be rational for the Republican party to make negative racial appeals. If blacks were somewhat more evenly distributed between the parties, following their now-more-diverse class interests, then the parties would be more competitive, both would have less incentive to arouse racial fears and animosities, and both would have to guarantee civil rights. And blacks *would* distribute themselves more evenly between the parties (in the authors' scenario) if party appeals were oriented more toward class, and less toward racial, issues.

The authors argue that the racial polarization of the parties was not, and is not, inevitable. The racial structure of the contemporary party system is a result of a racial strategy that had surfaced earlier but was given enormous impetus by Lyndon Johnson in the 1960s. A class-based Democratic party strategy could in time reverse the structuring forces set in motion by racial issues. Some strategies and structures of party competition give ready expression to racism; others do not. Even in a society in which racism is common, a more strongly class-based politics could appreciably reduce the political role of racism.

The authors' model of party competition is likely to prove very useful. Because the eclipse of class by race varies greatly, depending on party strategies and on the particular mix of blacks and whites in the parties in a particular location, the model is rooted in the characteristics of locations (e.g., precincts, states, the nation). Some combinations of strategies and groups, if allowed to run on without intervention, lead in the model to the virtual demise or enduring minority status of one of the parties; other combinations lead to more or less stable and vigorous party competition. By constructing a locational model, the authors have opened the way to viable, low-cost but theoretically fruitful empirical research on the politics of race and class in many settings.

Many blacks—and whites—will find it very difficult even to consider seriously the suggestion that affirmative appeals to race, as well as negative ones, should be moderated or that more blacks should support the Republican party—even less the notion that the Democratic party and the party system can be rescued by such steps. No doubt, these are hypotheses that can be tested finally only in the doing. In the meantime, the authors of this book have provided us with theory and data that change our understanding of the possibilities and their consequences. Their analysis of race and party system justifies, from an important perspective, the shift in party strategies predicted by Kevin Phillips in *The Politics of Rich and Poor* (1990). While their book can not settle the questions it raises, it raises them in an especially fruitful way that should guide future research and perhaps (as its argument becomes more widely known and understood) political action, as well.

San Francisco State University RUFUS P. BROWNING

Television and the Crisis of Democracy. By Douglas Kellner. Boulder: Westview, 1990. 287p. $48.50 cloth, $14.95 paper.

After two decades of steady media criticism from the Right, *Television and the Crisis of Democracy* is the latest in a series of books from the Left attacking network television in general, and news coverage during the Reagan administration in particular. The author's thesis is that "network television is the shame of the nation today—a complacent, compliant servant of power. Rather than promoting democracy, robust public debate, and an informed public opinion, the networks diminish and weaken U.S. democracy. They also promote the conservative interests that own and control them, thus precipitating a massive crisis of democracy and journalism in the United States today" (p. 173).

While remaining true to its leftist roots, this book offers a comprehensive literature review that dutifully includes mainstream research. An extensive bibliography covers the spectrum of media sociology and political communication. The author offers a hegemony model more comprehensive and multidimensional than standard Marxist and neo-Marxist theoretical analysis and less rigid than instrumentalist approaches. Within this theoretical framework, the author posits that three powerful corporations attempt to control leisure, entertainment, information, and mass communication in the interests of corporate profit and social control.

In a well-written historical section on the rise of network television, Kellner convincingly describes the forces that made U.S. television the most commercialized in the world. While most countries in Europe held steadfastly to a noncommercial model, U.S. citizens

were racing toward an unbridled commercial system controlled largely by monopoly interests. Kellner contends that the overcommercialization of radio helped promote a homogeneous consumer culture and established the hegemony of national networks over local affiliates.

Much of the criticism of the news coverage of the Reagan administration is similar to that found in Mark Hertsgaard's *On Bended Knee* (1988) and Martin Lee and Norman Solomon's *Unreliable Sources* (1990). Kellner argues that television and the media were key factors in protecting the Reagan administration from "complete collapse" after the Iran–Contra affair and in the election of George Bush. The author's criticism of the media's lack of coverage of alleged behind-the-scenes negotiations between Reagan campaign operatives and the Iranian government in 1980 over the timing of the hostages' release seemed to be highly prophetic. In an appendix, Kellner fully details the media's shortcomings in covering the October Surprise, offering details on which the media failed to focus until mid-1991.

In a chapter on alternatives, the author argues for a more pluralistic system of broadcasting. He proposes a substantial strengthening of the public broadcasting system, the resurrection of the National News Council, free airtime for political candidates, and a congressionally mandated study of media concentration. The author uses his own experience producing programming for a public access channel in Austin, Texas as a primer for more effective citizen participation in the media system.

This book goes to great lengths to build up the networks to mythic proportions at precisely the time they are in steep decline. Descriptions of network affiliates as "vassals" under the control of "television overlords" are already dated as today's affiliates wage open rebellion against their weakened masters (p. 73). A publication date of 1980 rather than 1990 would have given this book more credibility. Even a decade earlier, however, the notion of the all-powerful television networks was fraying around the edges.

By tenaciously sticking to his thesis of network hegemony, the author foreclosed a thoughtful examination of the political balance in the postnetwork era. There is no discussion of whether the decline of the networks and the ascendance of cable have benefited the Right or Left or how "narrowcasting," globalization, and the future convergence of media might affect the political balance of power. Other authors have already accounted for the changing electronic media marketplace, and their works are better for it.

Southern Illinois University, Carbondale JOE S. FOOTE

Crimes of Obedience: Toward a Social Psychology of Authority and Responsibility. By Herbert C. Kelman and V. Lee Hamilton. New Haven: Yale University Press, 1989. 382p. $35.00 cloth, $14.95 paper.

On 16 March 1968 Lieutenant William Calley led the second platoon of Charlie Company in an assault on My Lai 4. It was later revealed that as many as four hundred elderly Vietnamese civilians and children had been systematically slaughtered by Calley and his men. Social psychologists Herbert C. Kelman and V. Lee Hamilton view the My Lai massacre as a frightening example of the threat posed by lockstep compliance to authority and explore the social psychology of obedience in this new volume.

Their work is one of several recent books exploring various dimensions of obedience to authority and marks a much-needed shift in emphasis. For almost two decades social scientists have been infatuated with why people (collectively) rebel while ignoring the equally vital question of why people obey. Relatively little systematic work has been devoted to analyzing the well-documented human capacity for cooperation and acquiescence in venal and morally questionable governmental policies and programs. Even less effort has been devoted to understanding those solitary nonviolent resisters who, historically, have at great personal risk defied authority and forced us to admit that "the emperor has no clothes." The questions of obedience and resistance (the authors observe) have taken on even greater currency and import with the involvement of "the best and the brightest" in Watergate and Iran–Contra and with the rampant crime in the suites. It is increasingly apparent that state terror and the abuse of power are not the exclusive preserve of immoral individuals. Instead, such acts are typically bureaucratic crimes that result from the coordinated efforts of otherwise normal people who dutifully fulfill the expectations and dictates of their superiors. In short, to paraphrase Pogo, "We have met the enemy and it is us."

Those seeking emotional power and moral potency may encounter difficulty wading through Kelman and Hamilton's concern with theory building, scale construction and data analysis. At times, the authors' prose resonates with awkward and dense jargon. Their book also suffers from a glaring lack of coherent transitions and organization. They provide the pieces, but the reader must be sufficiently deft to put the puzzle together. Nevertheless, Kelman and Hamilton have made a major, albeit flawed, contribution to the psychological literature on governmental crime and immorality. (They focus on crimes of obedience that they consider to be illegal or immoral acts committed in response to the orders or directives of authority [p. 46].) They have provided intellectual perspectives and insights which promise to guide and inspire future sojurners in this relatively uncharted area.

The initial portion is a familiar rendition of various contemporary cases of governmental excess and crime. Chapter 3 provides an unexpected sketch of traditional and contemporary religious justifications for disobedience to the dictates of authority and the "superior orders" defense in the law of war. These chapters unfortunately remain like the dark side of the moon. They are of import; but the authors' fail to explore their practical significance and terrain fully. Once having disposed of these obligatory matters, Kelman and Hamilton turn to a theoretical discussion of regime legitimacy and draw heavily on Stanley Milgram in enumerating the psychological processes facilitating and inhibiting disobedience. These chapters provide familiar but valuable insights that unfortunately are not satisfactorily integrated into the volume.

The core of the book is the authors' analysis and discussion of a 1971 national survey and a refined version of the survey administered to a Boston sample in 1976. These surveys explored respondents' attitudes concerning whether they would have participated in the My Lai killings, their perception of how others would have behaved, and the justifiability of prosecuting and

penalizing William Calley (who was ultimately paroled) and the others involved in My Lai. The authors distinguish respondents based upon their willingness to exonerate—versus assign responsibility to—Calley, his unit, and his superiors. Kelman and Hamiltion ultimately explain the respondents' differing attitudes based upon three divergent orientations toward national government: rule, role, and value. Those with a *rule* orientation believe they are obligated to follow the dictates of authority. Weak, vulnerable, and fearful, such individuals find security and safety in adherence to their leaders and superiors. *Role*-based citizens view themselves as good citizens who support the U.S. government. They are emboldened by the regime's strength and prestige, which provides them with a sense of pride and accomplishment. These two ideal types would probably overwhelmingly accept the familiar jingoistic credos My Country Right or Wrong and America, Love It or Leave It. Disobedience is centered in the rather small segment of the population characterized by a *value* orientation. The support of this highly educated group (which appears to range between roughly one-fifth and one-third of the population) depends upon the regime's conformity to the norms and values that these moralists believe undergird the political system. The authors, to measure these three orientations, develop scales that likely will be refined and employed in the future by researchers studying compliance and disobedience.

Kelman and Hamilton, of course, analyze attitudes rather than behavior. Nevertheless, their general conclusions mirror those of other researchers who have pointed to the United States' alarming tendency to elevate patriotism over principle. The inclination is to exculpate both those in the trenches (and, to a lesser extent, government leaders) of responsibility. Only the autonomously minded are able to withstand peer pressure, ridicule, and harassment and assert that right and might are not necessarily synonymous. The authors offer a laundry list of institutional reforms and innovations to cultivate democratic dissent. It is unlikely that such proposals will be seriously considered or discussed. We will probably persist in feting disobedients abroad while flagellating them at home. Dissenters will continue to be dismissed as kooks, crackpots, and criminals. The irony is that the passion of resisters historically has provided a vital fuel for powering the engine of our democracy. Meanwhile, educators might ponder the fact that 51% of the respondents in Kelman and Hamilton's 1971 sample replied they would obey orders to shoot all the Vietnamese inhabitants of a village suspected of aiding the enemy. Thirty-three replied that they would refuse to shoot (p. 173).

University of Illinois, Chicago MATTHEW LIPPMAN

Procurement and Public Management: The Fear of Discretion and the Quality of Government Performance. By Steven Kelman. Washington: American Enterprise Institute Press, 1990. 213p. $26.50.

Steven Kelman's new book has two topics. The narrower topic, the procurement of computers by the federal government, will strike most people as a bit soporific. But the broader topic, the role of rules versus discretion in the public sector, is a central issue to anyone interested in public management, public policy, or organization theory.

Kelman's theme is that government procurement is far more rule-bound—and leaves far less to discretion—than the same process in the private sector and that this is a terribly costly arrangement. He shows how common sense is constantly perverted by the requirements of the bidding process.

The problem is not, as one might think, that federal computer purchasers are required to offer contracts to the low bidder. Other factors may be taken into account and often are. The problem is that purchasers may *not* take into account anything that cannot be clearly specified in the bid documents—including vendors' reputation in the marketplace and even their past performance at the same agency. One of the most depressing findings in this book is that those involved in federal procurement actually take pride in their ability not to be "biased" by this vital information.

While the perception, says Kelman, is that government needs more competition in the bidding process, the reality is that private-sector procurements are in general far *less* competitive. Instead, private purchasers seek ongoing relationships, which not only yield valuable information about performance but, more importantly, motivate the vendor to do a good job.

By requiring competition on written criteria, the federal government eliminates any incentive for vendor performance above the contractual minimum, such as finding innovative solutions to problems. Kelman cites the example of a vendor who proposed a cheaper and more effective equipment configuration in a bid, only to be entangled in a scandal because the purchasing agency awarded it the contract instead of reopening the bidding using the new specifications.

All this boils down to a point that is now familiar to readers of the organizational economics literature: it is impossible to write complete contracts under conditions of uncertainty. This book illustrates the point very clearly and with a dismaying wealth of examples. Dedicated bureaucrats struggle to define in advance what will constitute good performance by vendors; but their best efforts are doomed to failure, since no one can anticipate all eventualities.

The alternative, Kelman sensibly suggests, is to give bureaucrats more discretion so that they can exercise judgment. He argues that the risk of corruption can be minimized by modern investigation technology (including, he cheerily notes, wiretaps) and by having a relatively open process requiring consensus among a number of individuals. A conservative reform would be to incorporate judgments of vendors' past performance as an explicit evaluation factor. A more radical reform would be to give multimember panels essentially complete discretion in awarding contracts, requiring only that they state their reasons in writing.

Kelman makes a good case for reform. No one reading this book could possibly think that things are just fine the way they are. As a piece of policy analysis, this is a strong effort. As political science, it is less satisfactory—and at times frustrating. Why, exactly, do we give so little discretion to public officials? Kelman answers this question only briefly, but his approach will be familiar to readers of his earlier work: U.S. citizens have a culture that simultaneously holds high expectations of public officials and views them with acute suspicion. As a result, we enmesh them in restrictions and are intolerant

of the slightest deviation. The fault is not in our bureaucrats but in ourselves.

If this is the whole story, it is relatively easy to be optimistic about reform: we just have to stop being so silly. But is it the whole story? Consider that other great web of constraining rules, civil service, which many public managers would consider a far more important problem than procurement restrictions. A look at history shows that civil service was not born of a cultural whim but of a bitter and drawn-out battle. Most significantly, it was viewed as a way to control not bureaucrats but politicians. The same may be true for procurement: if restrictions are based on a well-founded fear of misbehavior (specifically political entrenchment) by politicians, then reform becomes much trickier.

Moreover, there are winners, as well as losers, under the current system. Kelman alludes briefly, for instance, to some people in Congress who are deeply suspicious of any more discretionary process that might benefit IBM. Who are these people, and what are their interests?

Kelman also tends to use *government* as a synonym for *federal government*. But there is considerable variation among local governments in their personnel systems and, presumably, of procurement also. How can this variation be explained? A better understanding of these sorts of questions would take us far toward understanding why bureaucracy looks the way it does.

University of Arizona HOWARD FRANT

The National Security Constitution: Sharing Power after the Iran–Contra Affair. By Harold Hongju Koh. New Haven: Yale University Press, 1990. 340p. $35.00 cloth, $14.95 paper.

Constitutionalism, Democracy, and Foreign Affairs. By Louis Henkin. New York: Columbia University Press, 1990. 125p. $25.00.

Is our eighteenth-century Constitution fit to govern a modern democracy? More specifically, in foreign affairs, can we allow the president enough flexibility to promote the national interest and still keep his vast powers limited and accountable, as constitutionalism requires? Koh and Henkin are troubled (Koh deeply, Henkin mildly) by evidence that the presidency has been able to evade both constitutional and statutory restraints in conducting the nation's foreign policy.

Henkin's book is strongest at what might almost be called the philosophical level. Constitutionalism, the United States' birthright, implies limited government. There can be no legitimate power that is "not checked, not balanced, not even the president, not even in foreign affairs" (p. 36). But if constitutionalism is our birthright, democracy has been our guest ever since the Declaration of Independence evoked the core values of equality and government by consent. Henkin stresses that a democratic constitution should promote the largest possible participation by the people and the most authentic representation—"open government . . . responding to the people's values" (p. 37). We are, he argues, a "dual democracy" (p. 40). The president commands information, expertise, secrecy, speed, and efficiency; Congress represents the people's "wider, more sober, more deliberate (more cautious) long-term values and judgments" (p. 43).

He admits to some unease about recent performance. In making treaties, for example, presidents should be more candid, the Senate more restrained (p. 68). In general, we should have "fewer decisions by one representative [the president] alone" (p. 43). Nevertheless, we do not need "amendment or radical reconstruction of the Constitution as regards foreign affairs in particular" (p. 40)—just a more assertive Congress and courts less prone to excuse themselves from "political questions."

Henkin has written many books on the Constitution and foreign affairs. He has earned the right to make a magisterial statement. Yet this is a disappointing book. Too often it is complacent where it needs to sound an alarm.

Koh's book is more substantial, fresher and more penetrating. Focusing on the Iran–Contra affair, it presents a comprehensive and devastating critique of the three major investigations: the Tower Commission, the congressional inquiries chaired by Senator Daniel Inouye and Representative Lee Hamilton, and the special prosecution by Lawrence Walsh. None of these efforts, Koh argues, came to grips with the fundamental issues: In an interbranch dispute over foreign policy, who decides? If constitutional and statutory norms are violated, how can the government be held accountable?

The Constitution, he notes, gives almost all the enumerated powers in foreign affairs (declaring war, raising and governing armed forces, appropriating funds, regulating foreign commerce, defining and punishing piracies and offenses against the law of nations) to Congress. Yet presidents "almost always win in foreign affairs" (p. 117). Why? The answer is that presidents conduct relations with foreign powers and have all the structural advantages (unity vs. bicamerality, concentration vs. dispersion of power). In addition, presidents lately have benefited from what Koh calls the "activist logic of this extrovert era" (p. 119).

On one key point, Koh and Henkin agree: Congress has the right, under the Constitution, to set limits on presidential foreign policy making. To establish this point, both rely on Justice Robert Jackson's concurring opinion in the steel seizure case of 1952 and set it against the expansive claims made by Justice George Sutherland in *United States* v. *Curtiss–Wright Export Corporation* (1936). Whereas Sutherland asserted that the president has powers in foreign relations that do not depend on the Constitution, Jackson argued that presidents who act in the absence of a congressional grant of power are operating in a "twilight zone," relying solely on their own independent powers. When a president acts against an explicit or implied congressional determination, as Truman did in seizing the mills (and as Reagan did in directing aid to the Contras in violation of the Boland amendments), "his power is at its lowest ebb," wrote Jackson (quoted on p. 108).

Koh concludes that Jackson's opinion establishes both the dominance of Congress over foreign policy making when it chooses to act and the responsibility of the courts to enforce this dominance when a case clearly presents this issue. The problem is that as a practical matter, Jackson's doctrine too often loses out to Sutherland's. Congress and the courts share responsibility for this weakening of constitutional bounds. Congress tends to acquiesce in presidential initiatives, due to "myopia" (tailoring remedies to the most recent abuses, rather than addressing the structural issues), poor draftsmanship (as in the War Powers Resolution), poor choice of legislative tools (reliance on legislative vetoes

or a mustering of majorities to block presidential initiatives), or lack of political will. Why does Congress legislate this way? Koh draws on the work of Morris Fiorina and David Mayhew, among others, to explain how political incentives work.

What is the remedy? Koh prescribes new "framework" legislation and the appointment of a congressional legal adviser specifically for foreign affairs. We also need, he writes, a "constitutional substitute for the legislative veto" (p. 176), more effective use of the appropriations power, stiffer criminal penalties for violation of national security statutes, and application of the impeachment power against executive officials other than the president.

Having shown that the "National Security Constitution" laid down by the framers was ignored by George Washington and by every determined president since; that Congress has routinely acquiesced in these evasions; and that the courts, too, have been tolerant, Koh concludes that Congress and the courts must now insist on reaffirming it. Essentially, he would restore the rule of law by writing new laws and by implanting another lawyer in the system.

These professors of law agree that constitutional reform is unattainable and not worth serious consideration. Against current practices, Henkin invokes the norm of constitutionalism, while Koh calls for new organic statutes. Yet Koh, in particular, convincingly explains why these approaches have not worked since World War II. What changes their prospects now? These works, written by lawyers and informed by the best work in political science, leave us profoundly uncertain about the question whether we can give the president enough power to conduct effective foreign policy and still keep him accountable.

Smith College DONALD L. ROBINSON

The Poor in Court: The Legal Services Program and Supreme Court Decision Making. By Susan E. Lawrence. Princeton: Princeton University Press, 1990. 207p. $29.95.

It is all too rare to find a scholarly book that increases our understanding of institutional behavior, imparts new and interesting substantive information, and invokes innovative and appropriate methodology. *The Poor in Court* not only accomplishes these formidable goals but is also well written and meticulously researched.

From the opening chapter, we know that a good read is to follow, because Lawrence sets up an interesting research challenge. She seeks to explore two somewhat competing and certainly complex theories of Supreme Court decision making: (1) a large body of literature indicates that certain litigants are highly adept players in the legal game, that is, they have developed strategies and tactics that enable them to access the Court to achieve policy ends; (2) there exists an even larger stack of studies asserting that the primary determinants of judicial outcomes are the values and attitudes of the individual members of the Court. If the latter holds true, can litigants (particularly those of a different ideological stripe from the majority of justices) actually influence the process? Addressing this question (more aptly, *solving this puzzle*) is a difficult conceptual and analytical task, but one worth undertaking. Its solution is of obvious significance to those laboring in the field. More important, it would reveal much about the nature of Supreme Court decision making and the institution's role in the governmental process. Because Lawrence reaches a simple, yet credible solution—to which I shall return—this book expands and reconditions our understanding of the Court.

To begin puzzling together these approaches, Lawrence focuses on the litigation activities of the Legal Services Program (LSP). This was a good choice and, I think, an interesting one as well—*good* because the LSP did not possess the characteristics we often associate with successful litigating enterprises. Much scholarship on litigant behavior and influence has focused on interest groups, which often have clear-cut policy objectives and strategies for achieving those ends. For example, many are highly selective about the kinds of cases they enter. The LSP, though, was quite different. It had no grand litigation campaign in mind. It took clients "because they met the indigency requirements of the Program rather than because their cases fit into a litigation strategy" (p. 9). In a note, Lawrence reacts to criticism that she is biasing her study because others have indicated (as have statistical profiles) that the LSP was an influential player. I think she is correct. If a litigating operation, such as the LSP, can influence Court decisions, then we would have to reevaluate our existing knowledge. In short, the LSP was a good choice because it provided the ultimate test of the potential effect of litigants qua litigants. It also was an interesting choice. There are numerous examinations of the LSP—especially of its history and day-to-day activities—but not of its influence, assessed systematically. By exploring it through this lens, Lawrence imparts important information about the program and its ability to engage the poor in the civil litigation process.

The next task faced by Lawrence was to devise an appropriate research strategy to evaluate whether the LSP influenced Court decisions. Given the focus of her analysis, coupled with the many debates within the subfield of law, courts, and judicial process, this is a nontrivial issue, posing numerous challenges. First, she had to get a handle on the amorphous entity that is the LSP. Choices were called for, and she made reasonable ones. She limited the study to the LSP's litigation activities between the 1966 and 1974 terms (before it was officially replaced by the Legal Services Corporation) and then to those cases it appealed to the U.S. Supreme Court or in which it participated as an *amicus curiae*. Second, she needed to define *influence*. Though many of her predecessors operationalized that concept as gaining access to the Court *or* as convincing the justices to adopt arguments on the merits, she chose to look at both. Finally—and most critically—she had to devise a scheme for assessing influence, one that would satisfy all camps. Herein lies the volume's greatest strength: because she mixes quantitative and doctrinal approaches, she is able to speak authoritatively to the various constituencies and theoretical concerns of the subfield. Consider her *modus operandi* in studying the effect of the LSP on decisions on the merits. She initially presents compelling numerical evidence indicating that it was a far more influential player, across a diverse range of legal areas, than group or behavioral approaches would have predicted. She then presses the discussion to pursue the issue doctrinally, fully recognizing that wins and losses,

for example, are one thing, the ability to shape the development of the law quite another.

This comprehensive, integrated approach pays off. When Lawrence argues that the LSP was extraordinarily influential, we believe her; the case is that strong. When she seeks to translate her findings into a more generalizable, theoretical framework, we also believe her; the argument is that appealing.

And what does Lawrence teach us about the puzzle of Supreme Court decision making? Most important is this: neither behavioral approaches nor those centering on litigant prowess provide a particularly compelling explanation of the LSP's remarkable success before the Court or of decision making. Instead, according to Lawrence, we must consider them in conjunction with "the temper of the times;" that is, the existing political climate can create "a 'window of opportunity' for the joining of new litigant claims, available legal bases, and judicial attention and sympathy" (p. 150). In terms of the focus of her study, such allowed the "poor's participation in Supreme Court decision making to produce significant doctrinal change." This conclusion is substantial, but its importance extends beyond the case at hand. Not only does it provide a reasonable solution to a long-standing research puzzle, revealing how those enmeshed in competing schools of thought can bridge their differences, it also has significant implications for our understanding of the Court and its role within the larger governmental process.

In the end, then, this book makes a major contribution to our understanding of things legal and extralegal. It is my sincere hope that all scholars of the judicial process read it. Indeed, my greatest fear about the book is its title. *The Poor in Court* is, yes, about the mobilization of rights for indigents, but it is about much, much more.

Washington University, St. Louis LEE EPSTEIN

Privacy in America: Is Your Private Life in the Public Eye? By David F. Linowes. Urbana: University of Illinois Press, 1989. 190p. $19.95.

Knowledge As Power: Political and Legal Control of Information. By David Sadofsky. New York: Greenwood, 1990. 133p. $38.95.

Sir Francis Bacon's aphorism that knowledge is power now has even greater relevance to the study of politics. First, there has been an extraordinary expansion in the creation and diffusion of knowledge, information, and data. Second, the widespread diffusion of the new information technologies, especially computers and telecommunications systems, has facilitated a major increase in the scale and sophistication with which humans can gather, manipulate, and transmit data.

For political scientists, it is particularly germane to consider whether this proliferation of information and information technologies has caused significant shifts in the distribution of power. We still do not have a well-developed theory of precisely how knowledge converts into political power. This area of inquiry includes many subtopics: Who controls contemporary information technologies and their products? Are data and information resources of power and influence similar to knowledge? Whose interests are served by the uses of these resources? Under what conditions are actors' rights violated by the uses of knowledge (information, data)?

David Linowes and David Sadofsky address some of these subtopics in their books. Each focuses primarily upon certain aspects of information use in the United States. Linowes is particularly concerned with issues of information privacy and considers whether the new information technologies have altered the relations between individual citizens and the large public and private sector actors who have access to information about those citizens. Sadofsky examines whether there are systematic differences in the information policies of different governmental institutions and whether the implementation of these policies affects inter-branch relations.

Linowes, who is a professor of political economy and public policy and served as chair of the U.S. Privacy Protection Commission in the late 1970s, mounts considerable evidence that widespread abuses of individual privacy are being perpetrated by institutional actors. In the journalistic tradition of David Burnham's *Rise of the Computer State* (1983), Linowes uses descriptive facts and numerous, detailed incidents to suggest a pattern of problems with the current collection and uses of personal information by government agencies and by such private organizations as insurance companies, banks, and credit-reporting firms.

"Vast amounts of personal information are being amassed without the subject's knowledge, and preserved indefinitely to be retrieved instantly. This information can be used in ways that have disturbing implications for an individual's personal freedom. The legal system," Linowes argues, "has lagged far behind new technological advances" (p. 9). He contends that many organizations gather or acquire information about individuals they should not possess and that in many instances they act upon inaccurate information. His examples suggest that neither individual action nor the system of statutory safeguards is sufficient to protect against widespread infringements on personal privacy.

Sadofsky, a joint public administration Ph.D. and J.D., grounds his analysis in contrasting views of the U.S. republic. He argues that a conception of strong executive governance, derived from the Hamiltonian perspective, has a corollary information policy that promotes the collection of vast amounts of information by the bureaucracy, the centralization of that information within the executive branch, and substantial constraints on public, and even legislative, access to that information. This conflicts with a Madisonian–Jeffersonian conception of participatory governance including concern for the protection of individual rights. According to Sadofsky, among these rights are the citizen's right to information about government decision and action and the protection of privacy, due process, and personal liberty by constraining bureaucratic uses of information. The Congress (delegated the task of informing the public) and, especially, the judiciary can act to limit the excessive information control attempted by the executive. But, Sadofsky argues, Congress has been too reticent in limiting the executive's information control and has failed to form its natural alliance with the judiciary in this policy domain.

Sadofsky's evidentiary base mixes consideration of the founding fathers' writings with both historical and contemporary court cases dealing with the government's right to limit information, the citizens' right to privacy, and the right of both to know. Sadofsky's own bias is clearly in favor of what he terms "robust openness of

expression" and the widespread availability of relevant information during government policy debates. He opposes the information secrecy he attributes to "the hierarchical style of the executive" in its dealings with the other branches of government and the public.

There is less overlap than one might expect in these two short books. Linowes is concerned with the linkage between the individual citizen and large public and private organizations, especially the protection of individual privacy. Sadofsky examines the role of information in national, interbranch politics, especially interorganizational restraints on the free flow of information. Linowes's anecdotal style eschews theoretical grounding, systematic analysis and efforts at generalization. He also offers little political or institutional analysis regarding the structural relations among actors. Sadofsky provides some examples of information use and abuse but relies primarily upon on case law to support his premise about interbranch conflict over information policy. For Linowes, contemporary information issues are crucially tied to the impacts of the new information technologies. In his view, these new technologies have fundamentally transformed how information is gathered, manipulated, and used. One could read Sadofsky's book and not even realize that computers and information technology are now extensively utilized in government.

Linowes's book is perhaps most effective as a warning to the elite public and the policymaking community that the United States continues down the slippery slope of eroding personal privacy in the face of organizations' massive and constantly expanding uses of computerized data bases on individuals. It is less likely to appeal to political scientists interested in rigorous empirical analysis or theory development.

Sadofsky's book will be most appreciated by those interested in a well-argued critique—grounded in constitutional law—of executive constraints on the use of information in the policy process. It might be less satisfying for those who see the U.S. executive branch, judiciary, and national legislature as complex arenas of bureaucratic politics with extensive *intrabranch* differences as to why and how information is collected and used in the policy process, rather than as unitary actors driven by *interbranch* differences. One important consideration that seems underestimated in the discussion is the changing ideology of the judiciary (particularly the Supreme Court) regarding information policy issues. The absence of any explicit treatment of the effects of the new information technologies on information policy and use is also somewhat surprising. Overall, these books add to other research developing the basis for a richer theoretical and empirical explanation of the increasing role of knowledge (information, data) in the political process.

University of California, Irvine JAMES N. DANZIGER

Sex, Gender, and the Politics of ERA: A State and the Nation. By Donald G. Mathews and Jane Sherron De Hart. New York: Oxford University Press, 1990. 283p. $24.95.

Over the past decade social scientists studying the policy history of the Equal Rights Amendment (ERA) have seemingly turned on its head the old saying that victory has a hundred fathers and defeat is an orphan. The failure of this proposed constitutional amendment has been attributed variously to a limited window of opportunity, dynamics of community conflict, issue transformation typical of symbolic politics, inadequate state-based organizational resources and strategies, and shifting legal doctrines that undercut the urgency for policy adoption. Mathews and De Hart here reiterate, extend, and only rarely refute this previous scholarship. However, their conclusion that "primarily, ERA failed because a large number of men did not like being challenged by women, and they did not believe that the things women were interested in were important" (p. 211) barely hints at the theoretical richness of this most comprehensive examination of an unratifying state's (North Carolina's) struggle with ratification.

The authors base their analysis on participant observation, extensive archival research, and more than one hundred interviews, conducted from 1973 to 1987. North Carolina was considered one of the states most likely to approve the amendment before the deadline. And by the end of the 11-year period (1972–82) during which the ERA was before the legislature, the North Carolina ratification coalition had become perhaps the strongest in the nation.

Two themes are developed here. One involves the conventional history of state-level interest group competition and legislative decisionmaking, informed by discussions of the precedent of woman suffrage in North Carolina and the postsuffrage politics of the ERA (1923–72). The meticulous description can be both frustrating and fascinating. At times, the reader is sorely taxed to recall the official role of named individuals rivaling in number those in a Russian novel. To compensate, there are the rewards of scintillating "factoids" (e.g., the use of Sam Ervin's Senate franking privilege by anti-ERA leader Phyllis Schlafly and ratificationist mailings of chicken droppings to legislators opposing the ERA in the final vote of 1982.) This story is a familiar but engagingly narrated one.

The most original and important contribution stems from the authors' avowed purpose "not . . . to explain why ERA failed so much as what the debate surrounding the issue tells us about the women's movement and its place in this country during the 1970s. . . . The intricate and subtle fusion of sex, gender, race, and power . . . underlies resistance to change in 1990" (pp. xi–xii). In explicating the intersubjective meanings of the ERA for supporters, opponents, and legislators, the authors find a common ambivalence concerning the gender revolution of feminism.

Ratificationists emerge as far more committed to cultural and personal change through the ERA than they admitted publicly (or to themselves) in their appeals for women's rights, equality, and change in the name of tradition. For antiratificationists, the ERA was a feminist-led attack on the absolute rightness of traditional gender roles, the basis of personal identity, social legitimacy, and the moral order. Many legislators too saw the ERA as a symbol of feminist and federal intervention and associated it with a loss of power for men and state officials generally. In faithfully and sensitively giving voice to these perceptions, the authors neither give license nor abdicate their responsibility as scholars to point out inaccuracies and logical inconsistencies, provide interpretation, and draw implications.

Mathews and De Hart present a vivid portrait of apocalyptic politics in the United States. Here cosmopol-

itan ratificationists, with their faith in social science, legal–rational authority, and bureaucratic forms of organization, discovered that knowing the conventional rules of pluralist politics and executing them well did not lead to victory. The paradigm of the United States as an underdeveloped nation receives much support here. But this is only one of several embedded texts, all of which are valuable and compelling for students of U.S. politics, public policy, interest groups, and gender.

Marquette University JANET K. BOLES

Getting Justice and Getting Even: Legal Consciousness among Working-class Americans. By Sally Engle Merry. Chicago: University of Chicago Press, 1990. 227p. $50.00 cloth, $19.95 paper.

Sally Engle Merry provides a thoughtful and rich ethnographic portrayal of a sample of white working-class individuals (primarily women) who seek redress for personal wrongs within the lower criminals courts and their associated mediation programs. This is a careful examination of citizens who bring their problems to the courts and of court officials who respond ambivalently, redefining the problems because they do not view them as truly legal issues.

Merry frames the interplay between plaintiffs and court officials through a discussion of three distinct discourses that coexist within the lower courts, each of which involves predominant categories and remedies: legal discourse that focuses on rights and evidence; moral discourse "[about] how people should treat one another, about relationships, and about respect and reputation"; and therapeutic discourse "[about] treatment and cure and about the way behavior is shaped by environment and social pressures" (p. 12). Plaintiffs tend to operate within a legal discourse, but court officials commonly redefine the problems and place them within moral and therapeutic discourses.

The author's insightful discussion of the citizens' sense of entitlement to seek court remedies discloses the tension inherent in this entitlement based in a broad sense of rights: that is, property, autonomy, and parental authority and the notion that court resolution of problems is more civilized and reasonable than violence. She provides instructive and lucid examples of specific types of problems and the distinct set of legal categories and rights associated with each: neighborhood problems (property ownership), marital problems (contract), boyfriend–girlfriend problems (protection against violence); and parent–child problems (state responsibility for teenage children in trouble). It is a complex portrait of individuals who do not express alienation from U.S. society yet evidence awareness of the social and economic inequality that eventuates their positions of relative powerlessness.

In this adept picture of contemporary citizens as clients, Merry suggests that due to the symbolic power in succeeding in producing a gesture from the courts, plaintiffs are empowered in relation to those against whom they bring complaints. Yet they are enmeshed simultaneously in the control of the courts and the concomitant power to define, reinterpret, and dominate. The risks of going to court are significant: stigmatization; having one's problems (especially in the cases of women) reinterpreted as issues of character and emotion rather than legal rights; and failure to find meaningful solutions.

The book is a strong contribution to the literature on mediation programs and dispute resolution, even as the author effectively points out that these individuals conceive of their complaints as "problems" rather than "disputes." Particularly important is her analysis of court processes as those of "cultural domination" in terms of the power of interpretation, categorization, and framing ways of talking and acting.

As is the fate of all subtitles (due to the requisite brevity), this volume's subtitle, *Legal Consciousness among Working-Class Americans*, is potentially misleading. This is not an encompassing look at the working class. It is a study of manifestations of the everyday consciousness of those who use the courts, that is, a relatively small proportion. It looks exclusively at whites (mostly women) in Massachusetts settings in the early 1980s. The author acknowledges the parameters of her analysis and includes a useful appendix summarizing other studies of court users that have demonstrated that "the social characteristics of court users are roughly the same as those I observed in Cambridge and Salem" (p. 183). Nevertheless, the use of the terms *consciousness* and *working class* engenders expectations of a macrostructural analysis of political economy, expectations that go primarily unfulfilled. *Consciousness* refers to the everyday thinking and reasoning of individuals, inferred by the author from informal conversations with them and observations of their "talking and behavior" in and out of court. Likewise, readers who expect an in-depth analysis of juvenile justice will find that it does not play a prominent role in the analysis. These limitations do not negate Merry's significant contribution; this thoughtful and illuminating book merits the wide audience and appreciation it will receive.

Arizona State University M. A. BORTNER

Governing the Commons: The Evolution of Institutions for Collective Action. By Elinor Ostrom. Cambridge: Cambridge University Press, 1990. 279p. $14.95 paper.

This is clearly an important book, but its significance depends upon how one interprets its intended scope. On the one hand, *Governing the Commons* can be seen as a major contribution to natural resource and economic development policy and, more specifically, to the management of common pool resources such as fisheries, groundwater basins, and irrigation systems. Such resources have long been subject to overexploitation and misuse by rational, self-interested actors. The conventional solutions are either management by an external governmental authority (Hardin) or privatization of the resource (Baden). Ostrom criticizes both approaches for their generally unrealistic assumptions (e.g., no communication among users of the resource) and instead explores the conditions under which a third alternative, the development of self-governing institutions by the resource users themselves, can be successful in achieving sustained use over considerable periods of time.

The broader—and, in my opinion, more correct—interpretation is that Ostrom's book is an example of an institutional rational choice analysis of solutions to problems of collective action. Here, again, the emphasis is on the development of self-governing institutions by the

individuals directly involved; but the scope includes not only such relatively small organizations as law firms and cooperatives (p. 25) but also a wide range of situations subject to the underprovision of collective goods: "The central question in this study is how a group of principals [actors] who are in an interdependent situation can organize and govern themselves to obtain continuing joint benefits when all face temptations to free-ride, shirk, or otherwise act opportunistically" (p. 29).

Irrespective of one's interpretation, the book is clearly part of Elinor Ostrom's long-term research program seeking to foster an institutional rational choice approach to public policy. Comparing her analysis here with her earlier conceptual work (Kiser and Ostrom, 1982; Ostrom, 1986) reveals several continuities but also one important difference. The general conceptual structure remains very similar: she assumes that individuals are intentionally rational; that their behavior is a function partly of their own (self-interested) goals and information and partly of the decision situation in which they find themselves; that the decision situation is, in turn, a function of institutional rules and community characteristics (especially the extent of normative consensus); and that there are three levels of action, namely, constitutional, collective-choice, and operational—with outputs from higher levels setting the institutional rules at the subordinate level. Another recurring theme is the focus on institutional rules. In her discussion of the factors contributing to successful common pool arrangements, for example, she discusses community characteristics (discount rates and behavioral norms) in two paragraphs, while the analysis of institutional rules is accorded 12 pages.

While the general structure and focus is familiar, the discussion of institutional rules has been significantly revised. Largely absent is the abstract list of rules drawn from game-theoretic analyses of decision situations: boundary rules, scope rules, aggregation rules, payoff rules, communication rules, and so on. In its place is a much more "functional" analysis: How do the rules allocate access to the resource? How do they provide the infrastructure necessary to utilize the resource? How do they provide for monitoring of individuals' behavior, the imposition of sanctions, and opportunities to alter the rules? (pp. 42–45). The result is a set of eight "design principles" for successful self-governing institutions, including (1) clearly defined boundaries, (2) monitors who are either resource users or accountable to them, (3) graduated sanctions, and (4) mechanisms dominated by the users to resolve disputes and to alter the rules (p. 90). The basic task is to foster contingent self-commitment among the members: "I commit myself to follow the set of rules we have devised in all instances except dire emergencies if the rest of those affected make a similar commitment and act accordingly" (pp. 99–100). This is all very sensible; but people who have followed Ostrom's work would probably like an explanation for why so much of her previous set of rule categories has apparently been jettisoned. If they proved relatively useless, she should tell us.

For all of its strengths, this book still has some limitations. First, almost all the empirical work (except for the analysis of groundwater management in Southern California) involves secondary interpretation of research previously done by sociologists and anthropologists in specific local areas in Canada, Japan, the Phillipines, Sri Lanka, Switzerland, Turkey, and other places. In most cases, the results are drawn largely from the work of a single investigator, with no discussion of the investigator's methods or of possible problems with the validity or reliability of the results. The implicit rule seems to be *trust*—which for someone as methodologically sophisticated as Elinor Ostrom is most surprising. Second, Ostrom has self-consciously selected cases in which self-governing institutions should be relatively easy to study and *relatively* easy to organize (p. 26). Each involves a single country, a relatively small number of people (up to 15,000), and situations in which the individuals both heavily depend upon the common pool resource for their economic livelihood, and can harm each other but not outsiders (i.e., no significant externalities). In addition, her discussion in Chapter 5 strongly suggests that self-governing institutions are much more difficult to sustain in cases involving significant cultural and value differences or in which extralocal officials have some incentive to meddle in local affairs.

This book represents an "interim report" from one of the most sophisticated and important research programs in public policy. In their future work, I hope, Ostrom and her colleagues will give a little more attention to non-rule variables and explore the potential for dealing with collective choice problems in larger, more culturally diverse settings.

University of California, Davis PAUL A. SABATIER

Attack Politics: Strategy and Defense. By Michael Pfau and Henry C. Kenski. New York: Praeger, 1990. 191p. $42.95.

At the heart of Michael Pfau and Henry C. Kenski's project is the search for a viable response to negative political campaign messages. Because regulation of campaign discourse is constitutionally problematic, campaign self-regulation strategically unlikely, and news media exposure of unfair advertising improbable, negative advertising is here to stay. The remaining alternative, according to the authors, is the development of countermessages that "inoculate" supporters against the effects of attack messages.

Pfau and Kenski's suggested response to attack messages rests on the persuasion research begun by social psychologist William J. McGuire in the 1950s and 1960s and subsequently pursued by a number of others (well documented in the authors' bibliography). McGuire and his colleagues were interested in whether individuals could be made more resistant to persuasion, particularly under conditions of forced message exposure. The "brainwashing" of U.S. prisoners of war during the Korean War first drew McGuire's attention to the question.

Persuasion inoculation theory is really quite straightforward. Just as controlled exposure to an attenuated viral strain produces biological resistance to disease, controlled exposure to threatening attack messages encourages the stiffening of psychological resistance to persuasion. Inoculation theory posits that refutational pretreatments, usually offered in the form of a warning of an impending persuasive attack, "triggers the motivation to bolster attitudes, thus fostering resistance to counterpersuasion" (p. 75). Awareness of an impending

attack message (threat) in conjunction with refutational preemption are both necessary for inoculation. Inoculation is particularly interesting as a potential counter to attack messages launched late in the campaign when the lack of time makes specific rebuttal messages impossible.

Pfau and Kenski tested the application of inoculation theory to campaigns in two experimental field investigations, one involving the 1986 U.S. Senate campaign in South Dakota and the other the 1988 presidential general election. In both studies the authors sampled potential voters in the Sioux Falls, South Dakota, metropolitan area (N = 733 for 1986, 314 for 1988). Subjects were randomly assigned to a variety of treatment conditions and a control condition. Using direct mail, subjects in the treatment conditions were exposed to *inoculations* offering either explicit rebuttal of the content of a subsequent attack message (*same pretreatment*) or a more generic defense of the candidate (*different pretreatment*), (p. 87). The 1988 field experiment included treatment groups exposed to either same or different inoculation treatments plus a treatment condition that offered a second reinforcement message prior to the attack message. Yet another group received only a postattack refutational message following the attack message.

Dependent variables consisted of receiver attitude toward the candidate or issue supported in the attack message and the likelihood of voting for the candidate supported in the attack message. ANOVA factorial analysis of variance was the primary statistical device used in the data analysis.

A complex array of findings is presented in the last 60 pages of the book. Generally, the authors demonstrate modest support for the inoculation hypothesis. Results suggest that inoculation pretreatments tend to (1) undermine the credibility of the source of the attack, (2) undermine the specific content of the attack, and (3) reduce the likelihood of successful persuasion regarding voting preferences. The difficulties with inoculation strategies, on the other hand, are (1) that achieving sufficient levels of threat from pretreatments, crucial for inoculation, in a campaign setting proved somewhat difficult, since in political campaigns subjects may tend to discount campaign messages of all sorts, including pretreatments (as the authors themselves conclude, p. 97) and (2) that the greatest success with inoculation is found among those who are already least likely to be susceptible to persuasion: strong party identifiers.

Though the authors are to be commended for imaginatively tackling what is perhaps the central problem of contemporary campaigns, the book is poorly executed. Over a third of the book is devoted to descriptions of various negative campaign messages, particularly since 1980. While offering little new information, these descriptions become tedious. It is not entirely clear why so much space was given merely to recording a widely recognized phenomenon, particularly at the expense of other substantive theoretical questions. The impact of negative campaigns on participation, for instance, is ignored.

The second part of the book is much more technical, with long passages of statistical information likely to lose undergraduates, many graduate students, and professors unfamiliar with factorial analysis of variance. The lack of a nontechnical description of methods exacerbates this problem. In short, while some readers may find themselves skimming the first part, others will have to struggle through the second part. These are unfortunate defects in an otherwise interesting application of theory to practice.

University of Utah STEVEN LIVINGSTON

Technologies without Boundaries. By Ithiel de Sola Pool. Cambridge: Harvard University Press 1990. 283p. $27.50.

When Ithiel de Sola Pool died in 1984, the world lost one of the great liberal optimists and a believer in the liberating power of instrumental rationality. Optimism and instrumental rationality are both much maligned these days, particularly within postmodern political theories. Thus, this book is unlikely to have the impact of his earlier, award-winning *Technologies of Freedom* (1983), to which, as the preface to the current volume points out, this book represents a sequel.

As was his method in *Technologies of Freedom*, here Pool's approach is to assess the impact of new communications technologies on existing and developing democracies. In his opinion, these technologies will soon rival in civilizing power the development of printing five centuries ago. Furthermore, the new "information society" is capable of reversing trends toward excessive centralization, urbanization, and nationalism that printing set in motion much to the detriment of Pool's overall goal, namely, the increase in individual liberty and opportunities for growth.

Throughout the book's wide-ranging discussion of satellites, computers, and digital transmission technologies, Pool iterates his canonical belief that the technologies themselves are ethically neutral with the antiphon that their results are invariably democratic unless tampered with by governments. This is the faith of Newton and his Enlightenment inheritors of modern science, a credo that gave birth to liberal governments as well as capitalist economies. There is a renewal of hope, as well, that technology might even return us to a more natural state (if not exactly the state of nature). Pool exclaims that a trait of the new technologies is a "return to more natural modes of communication . . . to overcome the artificiality in use or burdensome cost of media . . . to regain some measures of naturalness, spontaneity, interactivity, or ease of use" (p. 177).

Nowhere is this belief in the liberating aspects of communications technologies more evident that in Pool's discussion of development. While noting that "nature" respects no boundaries between states in making new satellite communication possible, Pool downplays the threat that such communicative imperialism (such as U.S. television) poses for cultural integrity within developing societies. Development in his view is always toward democracy, and communications technologies will aid in that movement if the "right-wing sentiment of protection of ethnic identity" is seen in its true light as "squarely against the liberal principle of freedom of information" (p. 115).

At the very least, Pool maintains, the technologies themselves are empty vessels, neutral carriers of the messages and substances entrusted to them. Thus, they reflect the nature of the societies they serve, whether authoritarian, oligarchic, or free (p. 195). But they hold great liberating promise for developing countries if those countries adopt the correct model for development. That

is the "infrastructure approach," in which public funds are spent to provide necessary equipment for access to worldwide communications without regard to the substance carried over the air waves. Such an approach is "populist": "It defines development as the dispersion of autonomy and initiative throughout the society, and so it sees communications facilities as tools to be put in people's hands for them to use" (p. 183).

To his considerable credit, Pool's faith in technology and its rationalizing power never descended to the simplistic (and ultimately dehumanizing) dogma of waiting for the technological fix. He was far too insightful an observer of history and politics to mistake that instance of myopia for vision. Yet his "soft determinism" concerning communication technologies' ability to transcend political, as well as physical, boundaries ultimately ignores part of the essential nature of politics and rationality. Both manifest boundaries that it is not within their nature to cross without danger. Politics is about limited communities and shared opinions, not about universal humanity and ultimate truth. Technology itself cannot make human rationality capable of achieving either of these latter generalities. To assume it can is to mistake the nature of politics as somehow less than science as an essential human endeavor.

University of Connecticut RICHARD P. HISKES

Beyond 911: A New Era for Policing. By Malcolm K. Sparrow, Mark H. Moore, and David M. Kennedy. New York: Basic Books, 1990. 269p. $22.95.

Policing As Though People Matter. By Dorothy Guyot. Philadelphia: Temple University Press, 1991. 357p. $39.95.

These two books reflect a growing consensus in the research community that the "reform model" that has shaped U.S. police administration since the late 1920s has hit a dead end. At that time, top administrators faced organized bootleggers, meddling politicians, and a rank-and-file that was racist, corrupt, brutal, and lazy. The reform model stressed bureaucratic centralization, political independence, and internal inspections on the part of police organizations and a cool, detached professionalism (of a curious, by-the-book nature) on the part of individual officers. The daily activity of today's departments is workload- and technology-driven, pressured by 911 calls, dominated by radio dispatchers, and managed from a quantitative performance orientation that drives everyone to "make their stats."

Where has this gotten us? Now the police face soaring violence, drug-infested neighborhoods, crumbling housing projects, and shimmering racial tension. The stats have got worse. The police feel unappreciated, and the public feels ill-served. While this is not all their fault, what police have done to date has not gotten society out of this hole. However, the political strength of the myth of police as crime fighters has such a grip that it is a courageous leader who takes any but the easy course of just crying for "more police" in response. A few Jeremiahs can be heard, however; and ironically, many of them are to be found among the ranks—police chiefs and patrol officers who are aware that what they are doing is not working and needs fixing. Both of these books are organized around their particular police heroes and tell their tales of the new politics of policing in the context of local case studies.

Sparrow, Moore, and Kennedy have lots of heroes, from cities as diverse as London, Houston, Newport News, and Los Angeles. All of them have engaged in reshaping departments, moving them away from the reform model and toward a "community policing" orientation that stresses autonomy and personal involvement on the part of individual officers and decentralization and openness to outside influence by their organizations. From these experiences the authors have distilled a kind of primer for managing change in police organizations. The book details how their heroes confronted some well-known obstacles to police reform, including the macho values of the police subculture, bureaucratic rigidity, public indifference, and the risks of relying on the discretionary authority of street police (which are considerable and still include bias and corruption). It counsels that departments must open up so that the priorities of the communities they serve are reflected in policymaking and so that the public can learn more about the police and how they actually operate. The book describes how the complaints, community relations, and press operations of police departments currently serve just the opposite function. The authors also call for greater cooperation among the police, the public, and other public and private agencies. This is not trite; it is risky. The public often has things to say that the police do not want to hear. Other organizations have differing needs and priorities, and the police traditionally have not been very willing to accommodate them. Instead, they rail that no one else can understand them and that they could really be effective if everyone else would change. This book is based on site visits, interviews, and discussion of these issues by chiefs and policing scholars around a seminar table in Cambridge. We have to take the authors' word that the reforms were good or bad and succeeded or failed, but they make a convincing case. This is a wise and smart book, cast at about the level where both the researcher and the police intellectual will learn a great deal.

Guyot only has one hero, the reforming chief of upstate Troy, New York. He did not get as far as any of the others; it would be fairer to say that he moved his department *into* the reform period during the late 1970s and early 1980s, when Guyot was doing her fieldwork there. He tried to institute a few of the innovations described by Sparrow, Moore, and Kennedy; but they never got off the ground. This book has several strengths. It provides a great deal of detail about things that really matter in making organizational change, including how her chief dealt with a union, the civil service system, his city council, local newspapers, a fearful public, and zero budget increases. The detailed chapter on union power makes one wonder how heroes from the other cities got away with as much as they did. Union power is an important topic on which the Sparrow, Moore, and Kennedy are not particularly strong. General discussions are echoed throughout in narrative material gathered from the backseat of patrol cars. The Troy story is also interwoven with a review of past research on all of these topics and more, including a history of police reform efforts, the pathologies of police performance evaluation, the limits of official statistics, and the causes of crime. The result has a rambling, cut-and-paste flavor; but the literature review adds con-

text and generality to the detailed case material that the other volume did not require. Actually, the books walk both sides of the level-of-detail question. While Guyot occasionally tells us somewhat more than we want to know about Troy, the Sparrow, Moore, and Kennedy volume is long on quotes but a bit short on evidence—it skips lightly over its case material. Their strategy works because it is deftly done, but in lesser hands the primer might not have been as convincing.

Northwestern University WESLEY G. SKOGAN

The American Revolution in the Law: Anglo–American Jurisprudence before John Marshall. By Shannon C. Stimson. Princeton: Princeton University Press, 1990. 228 p. $27.50.

Shannon T. Stimson's *American Revolution in the Law* presents a provocative and fresh historical account of the U.S. legal tradition. Focusing on the objectives of jural actors and seminal legal and political thinkers in England and America during their respective revolutionary upheavals in the seventeenth and eighteenth centuries, Stimson sets out "to challenge the assumption of a basic unity of vision and purpose at the roots of Anglo–American jurisprudence." In turn, she hopes to explain why "the question of the scope and limits of the judicial function in a democratic state is the overarching question of U.S. jurisprudence, whereas in English jurisprudence it remains a question of little significance" (p. 3).

Stimson disagrees with such "new historians" as John Reid and Thomas Grey, who discern a basic continuity between colonial legal assumptions and the common law tenets of Coke, Hampden, and Pym. She marshals evidence from important libel and treason trials and prominent political argument to conclude that the U.S. legal tradition departed substantially from England's in terms of such fundamental questions as the nature of sovereignty and law and the meaning and extent of citizen participation in such legal institutions as the jury. To be sure, English juries in the seventeenth century were compelled by circumstances (mainly the prevalence of uncertainty about the content of common law and concerns about despotic application of the law by judges) to develop a quasi-independent "nascent 'space' for judgment" (p. 4), which included jury nullification and some power to interpret the law. However, as the famous 1649 treason trial of the Leveller John Lilburne revealed, this expansion of the jury's deliberative power amounted neither to a radical change in the law nor to a norm of judicial independence. At the end of the eighteenth century, English juries still "accepted a version of law as essentially government's instrument, whose content they must rely on judges and magistrates generally to determine" (p. 30).

American legal change was more radical, pointing toward judicial review and a relatively independent judiciary. Drawing on Locke and the logic of the Enlightenment (Stimson's reliance on the influence of Locke's "framework of ideas" pits her against such historians as Bernard Bailyn and J. G. A. Pocock), colonial Americans developed a belief in democratic deliberation and a concomitant distrust of sovereignty, including judges appointed by the Crown. Legal actors in the eighteenth century viewed law as vital to the public interest of a democratic polity. As an essential domain of legal participation, the jury became the locus of a "widening [of] the space for reflective judgment about laws once made" (p. 59). Andrew Hamilton's appeals to the jury about "popular notions of government and society" in the famous 1734 Zenger libel trial epitomized this jury role. This expanded role of the jury not only established the conditions out of which the notion of an independent judiciary arose but also initiated the very tensions between unelected judges and the "people" that became an important feature of constitutional law.

Part 2 discusses some major perspectives on the nature of judicial power and review in revolutionary and post-revolutionary America, paying special attention to John Adams, Thomas Jefferson, Alexander Hamilton, James Wilson, and John Marshall. As is known, these men entertained different views as to how far government (especially national government) can be trusted and what relationships between nationalism and localism and between judges and "the people" (as interpreters of the law) were proper. Marshall tried to resolve these tensions by a jurisprudence that entailed "politics by discussion" (p. 139), but even he was unable to resolve the central tensions of judicial review in the U.S. democratic order.

Stimson's book is meticulous, thought-provoking, and largely persuasive. However, she could have demonstrated more clearly the links between the earlier theory and practice of juries and subsequent theories and debates about the power of judicial review. Though these institutions are linked by a common legal culture, the complexity of this culture (which Stimson amply demonstrates) makes the relation between jury practice and eventual judicial review unclear. In a similar vein, the connections between the views of judicial power discussed in Part 2 and the previous historical analysis of juries are not fully articulated or tied together. One also wonders how a consideration of James Madison's views would have affected the analysis and conclusions.

This said, Stimson's book provides an interesting and fresh approach to the emergence of debates over judicial review; anyone interested in constitutional history or theory must read it. Stimson impressively challenges major historians and raises points that are relevant to the virulent contemporary debate over constitutional interpretation. She does not address "how" to interpret the constitution; but she shows that the U.S. legal tradition provides no definitive answer to this question and that the practice of interpretation is inherently political and value-laden. This conclusion offers little comfort to those (like Judge Bork) whose critiques of judicial activism ultimately rest upon a separation of law and politics that is more reminiscent of Blackstone than the U.S. legal tradition Stimson so ably portrays. On the other hand, Stimson, like Sanford Levinson (*Constitutional Faith*, 1988) shows that debates over "who may interpret the constitution are also essential parts of our constitutional heritage. This suggests that critiques about the nondemocratic nature of judicial activism (including those of Edwin Meese and Bork) should not be automatically dismissed. The tensions between democracy and law are indelible parts of our legal tradition.

University of Wisconsin, Madison DONALD A. DOWNS

State Supreme Courts in Nation and State. By G. Alan Tarr and Mary Cornelia Aldis Porter. New Haven: Yale University Press, 1988. 283p. $12.95 paper.

It has long been an accepted canon that state supreme courts can play an important policymaking role in their own state and in the nation at large. Unfortunately, few books have appeared that have sought to explain exactly what that role is. G. Alan Tarr and Mary Cornelia Aldis Porter successfully seek to enhance our understanding of these courts' policymaking role in this book. The central theme of the book, which recurs in all chapters, is that state supreme courts are in a position to carve out significant policymaking roles in the federal system. Moreover, they argue, the nature of that role will vary from state to state.

The authors develop these points in six chapters. The first two deal with relationships between state supreme courts and other actors in the federal system. In both chapters a significant amount of literature is ably digested and summarized. Not a great deal of new ground is covered, however. These first two chapters provide the basis for the meat of the book—case studies of change in three state supreme courts: Alabama, Ohio, and New Jersey.

Each state court is examined in detail in a chapter of some length. The approach used is not quantitative. Instead, the authors construct their case studies by relying on secondary sources, analysis of important cases, and (in the cases of Alabama and Ohio but, for reasons unexplained, not New Jersey) interviews with state supreme court justices.

Certain common issues are examined in each case study: the posture of each court prior to its change; the factors leading to change; and the current posture of each court in terms of such issues as relations with other state actors, relations with the U.S. Supreme Court, and each court's attitude toward innovations in certain areas of the common law. At the same time, details specific to each state are spelled out.

Finally, the authors offer some conclusions, of which two stand out. First, contrary to commonly accepted beliefs, rapid change can take place at the state level. Alabama, for instance, has moved out of the dark ages into the mainstream of U.S. law in a short period of time. What accounts for these changes? Here the case study method (especially one that uses a limited number of cases) may show its shortcomings. The authors point to the national court reform movement as one factor instrumental in fostering change. Other national trends (e.g., the Civil Rights movement) are also noted. Finally, in the case studies certain individual personalities loom very large (e.g., Arthur Vanderbilt in New Jersey), though their influence is downplayed somewhat in the concluding chapter. Indeed, one can come away from a reading of the case studies with the impression there is much that is idiosyncratic in the development of state courts. The examination of a greater number of cases may well uncover more general trends not easily found in a case study approach.

A second conclusion is that of diversity. Each court has an "institutional identity" that sets it apart from the others. Contextual factors, the authors argue, are important in establishing and fostering each court's identity. This raises a question that the authors do not—and perhaps cannot—answer fully. What contextual factors are important, and what is their relative weight? Here, too, the limitations of the case study approach may have appeared.

Thus, the book leaves a reader with many questions. For instance, what accounts for variation among state supreme courts? What is the relative importance of the different factors that foster change in these important policymaking institutions? However, no book answers all questions. In addition to showing the importance of state supreme courts in U.S. politics and enhancing our knowledge of their role, the authors have raised questions that should be of interest to other students of court policymaking and U.S. state politics. The book deserves a wide readership.

University of Wisconsin, Oshkosh DAVID M. JONES

Divided Democracy: Cooperation and Conflict between the President and Congress. Edited by James A. Thurber. Washington: Congressional Quarterly, 1991. 323p. $17.95 paper.

Congress, the President, and Public Policy. By Michael L. Mezey. Boulder: Westview, 1989. 255p. $15.95 paper.

Divided government, wherein one political party does not control the presidency and both houses of Congress, has increased since 1955. From 1955 to 1992 it prevailed during 26 years (68% of the time), compared to only 8 years (14% of the time) from 1897 to 1954. Two recent books do much to help us understand the nature, the causes, and the consequences of divided government, and include suggestions for remedying the current political gridlock between Congress and the presidency.

The Thurber volume is a superb reader containing 13 essays addressing important questions about cooperation and conflict between the Congress and the president. It is a very balanced work covering considerable ground, and it is designed to avoid presenting any single viewpoint or dominant ideology. Some of the contributors believe the system is working well, while others feel reform is needed. Views of both contemporary Madisonians and latter-day Hamiltonians are presented, while examining various arguments that ultimately center on efficiency versus representativeness and leadership versus democracy.

The range of topics is impressive. Thurber presents an overview of the roots of divided democracy and in a second essay examines the impact of budget reform on governance. Mezey contributes two articles, one that places Congress in the center of a presidential system and another that underscores the policy limitations of Congress. Insightful and somewhat contrasting views are presented by Pfiffner on the general problem of governance, Davidson on how divided government operated in three congressional eras, and Oleszek on the salient features of congressional policymaking in a divided government environment.

Other contributions focus on the congressional challenge to presidential regulatory relief (Foreman), war powers (separate pieces by Deering and Fisher), the removal power (Fisher), Congress and Nicaragua (Brenner and LeoGrande), and a macro transformational perspective (Dodd).

This is an excellent reader that merits adoption in a variety of courses. Some needs that should be addressed in the second edition are more material on the electoral

origins of divided government (Jacobson's work), the role of the media in influencing both mass public and elite perceptions, the role of interest groups, and much more information on how political coalitions can be built to overcome stalemate. The latter topic is mentioned by Oleszek, but higher-caliber case study information along the lines of the Brenner and LeoGrande piece on Congress and Nicaragua would strengthen this very timely and important reader.

Mezey's work, by contrast, is a provocative, thoughtful, and well-written theme text that not only covers the origins, nature, and consequences of divided government but also proposes some broad solutions. Mezey is concerned that the status quo is all too frequently characterized by either stalemate or poor public policy. To build his case, he analyzes political institutions and public policy by offering some guidelines for both democratic and managerial policy criteria. He sees the current constitutional framework as a balanced system, compared to the representational system he favors for the United States. The latter is characterized "by a more dominant policymaking role for an executive operating through a political party or a coalition of political parties that commands a dependable majority in the legislature" (p. 15). He further believes that due to changing circumstances, we now have a constitution against government.

Mezey describes a transformed presidency and an evolved Congress, and follows with an analysis of the machinery of presidential domination. He emphasizes the failure of presidential domination, with either stalemate or bad policy outcomes. He finds the current system's efforts to address the problem as inadequate, including the use of commissions, automatic devices like Gramm–Rudman–Hollings and war powers, the legislative veto administrative strategies, and presidential imperialism.

Mezey's analysis is stimulating and informative, but his book falls short in the final chapter on what can be done to get the Congress and president to work together. Although his normative argument that we move toward a representational system is intriguing, there is little chance that Congress will give up its current policy turf to the president, even with Mezey's insistence that his representational Congress will still debate issues, exercise executive oversight, and provide service representation. Mezey's list of reforms is too broad and unrealistic, including public finance, higher legislative salaries, stronger legislative parties, and selective adoption of features of the parliamentary system like a mandatory question hour.

He emphasizes that "capitalism remains part of the problem" (p. 214) and that the system has too many independent actors but follows this with a generalized call for new arrangements to facilitate government action and a focus on reteaching the ideals of citizenship. He is also too idealistic in calling for candidates to cease using anti-governmental themes, thus pandering to the electorate's tax antipathy and to avoid using a mix of anti-tax rhetoric with spending promises to the audiences they are addressing. A presidency with a leading policy role of a more collective nature, supported by administrative resources and political resources of a reinvigorated national political party, is not likely to occur.

In short, there is a gap between Mezey's insightful analysis of the factors underlying divided government and his far-reaching solutions. If there is to be reform at all, it is more likely to be incremental and along the lines of Pfiffner's essay in the Thurber reader. Pfiffner suggests refining the checks and balances system a little by "continuing the use of the informal legislative veto, modifying the War Powers Resolution, and changing the rescission process—[to] provide for some accommodation of conflicting interests" (p. 57). Pfiffner also notes that there is no simple mechanical or process fix. In other words, "there is no substitute for leadership, trust, and comity" (p. 57).

University of Arizona HENRY C. KENSKI

The Expendable Future: U.S. Politics and the Protection of Biological Diversity. By Richard Tobin. Durham: Duke University Press, 1990. 313p. $45.00 cloth, $17.95 paper.

Declining biological diversity has been characterized as one of the most serious threats to global sustainability. Species are declining at such rapid rates that many are becoming extinct before they can be scientifically identified. In addition, the consequences of these extinctions are not readily apparent and are open to scientific conjecture. What *is* known, however, is that extinction is irreversible.

While at first glance, declining biological diversity may appear as strictly an ecological problem, Tobin accurately portrays the issue as distinctly political. Although the loss of species is a worldwide matter, Tobin limits the scope of his investigation to the United States' efforts to curb increasing extinctions, citing the lead role in protection of biodiversity taken by the United States. Tracing the history of the Endangered Species Act and its amendments, Tobin forcefully argues that decisions are based as much on political motives and realities as upon biological data.

Rather than merely presenting a legislative history, Tobin reports efforts to preserve biological diversity as dynamic and changing. As priorities evolve and administrations change, the tactics and procedures employed by federal agencies and Congress also shift. Tobin examines this dynamic component through in-depth analysis of the Fish and Wildlife Service's ability to act effectively in reducing extinctions caused by people. In addition to simply assessing the agency's administrative power, Tobin presents detailed studies of the listing process, designation of critical habitats and development and implementation of recovery plans. Again stressing the political component in each, he asserts that as the costs (real or imagined) are recognized, the agency's ability to act is constrained either by Congress, more powerful agencies, or both. Furthermore, the service has encountered so few successes that it often acts to avoid conflict, preferring to compromise with other federal and even state agencies. These political maneuverings often result in a less-than-desirable situation for the species in jeopardy.

To complement his examination of the processes involved in implementing the Act, Tobin presents several useful case studies. Although most are no more than a few pages, these examples are especially valuable because they illustrate the political nature of preventing extinctions caused by people.

While Tobin's presentation of declining biological diversity as a political issue and his thorough analysis of

the Endangered Species Act are quite well done, *The Expendable Future* is not without flaws. For instance, Tobin's lack of assertiveness in his final assessment of efforts to preserve endangered species is especially glaring. Despite his statements that the program has generated few (if any) real successes (p. 254), Tobin asserts "depending on one's hopes and expectations, efforts to protect endangered species in the United States are either a modest success or a massive failure" (p. 257). Although the formal mechanisms for protection may have prevented some species from reaching extinction, several protected species have become extinct; and of the few that have recovered, most have done so independently of federal efforts. To regard the act as even a modest success seems too generous.

The low emphasis placed on prescriptions to improve the preservation of biological diversity is also disappointing. Tobin does have some valid suggestions for improving the overall effectiveness of the act; but these recommendations, like his conclusions, are quite timid. Tobin devotes only two pages to his six proposals, geared mostly to improving implementation of the Act. As is evident in his analysis of the Act, better implementation may not be enough to guarantee protection of imperiled species. Although Tobin does move in the necessary direction with two of his proposals (shifting of the burden of proof and the creation of a new agency for the preservation of biological diversity), I would have liked to have seen some more innovative solutions.

A final troubling aspect of the study is Tobin's avoidance of the larger issue of sustainability. Of key importance in this larger issue is the preservation of ecosystems as effectively functioning units. Vital habitat is also essential to the preservation of biological diversity. The linkages between sustainability of ecosystems and the maintenance of species diversity are quite strong. With the increased emphasis the social sciences have given sustainability in recent years, it is truly disappointing that the concept of sustainability is not incorporated into *The Expendable Future.*

These reservations aside, Tobin has contributed valuable work to the understanding of the regulatory process. Not only does he provide a thorough account of efforts to preserve endangered species, but his work provides excellent analysis of the actions and interactions of Congress and the various agencies. Through Tobin's constant attention to framing the protection of endangered species as a political issue, the reader is able to gain insight into important factors and dynamic processes involved in obtaining social goals.

University of Arizona DAVID COLNIC

The Decline of American Political Parties: 1952–1988, By Martin P. Wattenberg. Cambridge: Harvard University Press, 1990. 203p. $10.95 paper.

The Demobilization of American Voters: A Comprehensive Theory of Voter Turnout. By Michael J. Avey. New York: Greenwood, 1989. 152p. $37.95.

Despite being in the same general area and dealing with related—and at times overlapping—concerns, the conceptualizations, the use of data and supporting materials, the arguments being made, and the intent of these two volumes are more notable for their dissimilarities.

Martin P. Wattenberg has supplemented the earlier edition of his influential and well-received *Decline of American Political Parties* by adding a chapter analyzing the 1988 and 1984 elections within the overall context of the book's thesis. Wattenberg's argument, based principally on survey data from the National Election Studies, is that the declining association with the political parties and their decreasing impact on the vote is a result of a "a growing neutrality in the electorate's perceptions of the parties" (p. 66). An alternative explanation that negative feelings fuel the public's increasing distance from the parties is rejected. The consequences of the trends developed are that political parties

> are considered less relevant in solving the most important domestic and foreign policy issues of the day. In the voters' minds, the parties are losing their association with the candidates and the issues that the candidates claim to stand for. . . . Major new issues that arise will probably not help the parties rebuild their base of support unless voters are convinced that the parties can serve a meaningful function that candidates alone cannot. (p. 89)

Do the elections of 1984 and 1988 support this evaluation? Basically, yes.

Wattenberg argues that a quiet revolution occurred during the 1980s, due in large part to clear—even dramatic—policy differences between the two parties, the ability of voters to apply retrospective judgments of direct relevance in the elections, and the party-building efforts of Ronald Reagan. Republican party identification increased, Democratic identifiers continued to decline, and the margin of difference in partisan identification between the parties narrowed to its lowest level since the first evaluations in 1952. (Although the Democrats continued to maintain an edge among likely voters, this was reduced to roughly one percentage point, again a Democratic low point and Republican high point for the period covered.) For the first time also, Independents outnumbered Democrats. One consequence is that the Republican presidential candidate can concentrate his appeal on the party's core, making his or her election less dependent on attracting independents or Democrats. The proportion of George Bush's total vote from Republican party members was the highest recorded since the introduction of the party identification measures.

Nonetheless, such a realignment has not led to revitalized partisan identifications. The shifts have not penetrated to the congressional level. Split-ticket voting continues, as does rule by divided government: "Such a realignment is hollow when the two parties continue to have a weak image in the public mind and an uncertain role with future of American government. Realignment has thus been muted in the candidate-centered age in comparison to the electoral upheavals of the past" (p. 135). The decline of partisanship has not been reversed. The public continues to experience difficulty in relating the parties to the work of government and in contributing to the resolution of the most pressing of the society's problems. It is a bold argument. It should again win friends for the book and add to its reputation as one of the most original of contributions to the thinking in the area.

Michael J. Avey has written quite a different book. Avey develops a theory of voter turnout meant to be richer and more comprehensive than any presently available. He places emphasis on the role of political elites and policy choice in the mobilization and demobi-

lization of electorates as being more relevant as keys to voter involvement than demographic indicators. His argument is that the parties do not adequately represent the interests of the working class and the poor in particular. If one party (or both) did, participation could rise to 80% of those eligible:

> The strongest motivation to vote is seeing concrete benefits resulting from elections. . . . mobilization involves providing or promising benefits. Demobilization is accomplished by increasing costs or decreasing benefits. The focus of analysis in turnout should be the intentions of political elites to bring people to the polls or keep them at home. The political elites control both the costs and benefits of voting. Successful political mobilization appeals to the interest of the target population, makes the participation of the target populations easier, and empowers the participation of that population. (pp. 27–28).

Avey then refines and applies his theory primarily within the limits of secondary source materials available. The canvass is broad and the objective ambitious.

There are problems with the approach, however. The model itself is general and nonpredictive, more of a synthesizing perspective than anything else. The inclusion of the findings of such a range of studies can result, on occasion, in a terse and choppy presentation. A more elaborated and fluid development might help the reader and give the book a broader audience. The secondary research used to flesh out the model more often than not employs a limited time frame or a narrowly prescribed focus or (alternatively and less frequently) a broad, encompassing categorical overview. Each presents a problem in integration. In addition, the author may take the socieconomic model too literally as the principal alternative target for attack. As I see it, it illustrates empirical associations and tendencies, some more debatable than others (e.g., the relationship between an increase in formal education and turnout or what such a variable may provide a proxy for). It is not the most flexible or inclusive of approaches nor the ultimate explanation for the range of factors that influence participation. It is, however, an empirically well-developed mapping of socieconomic status conditions associated with voter involvement.

The author's broader conception does perform a service by putting politics back into the equation; and his emphasis, much like Wattenberg's, on the relevance of policy stands in attracting voters is useful. Also, his wide-ranging knowledge of the appropriate social science research (in particular, the variety of historical studies called upon) provides a useful point of reference for others in the area. A good deal of effort, hard work, and intellectual energy has gone into the construction of this analysis.

In sum, then, the two books have different strengths. Both address questions of significance (each in its own way); and while the audiences and uses may well be different, both deserve attention.

Northwestern University WILLIAM CROTTY

Interpreting the Constitution: The Supreme Court and the Process of Adjudication. By Harry H. Wellington. New Haven: Yale University Press, 1990. 196p. $22.50.

The issue for Wellington is how to reconcile the Supreme Court's power of judicial review with representative democracy: "One difficult question that emerges [from a review of the Court's history] is how we in a democracy can tolerate regulation without representation" (p. ix). Wellington's answer is a blend of ascertainable common law adjudication and pluralistic politics. The Supreme Court's particular view of the Constitution will and should prevail, he writes, "when it is proven politically digestible." What make it digestible "turns in part on the Court's prior interpretation" (p. 158).

Of course, in arriving at this conclusion, Wellington adeptly dissects other attempts to reconcile democracy and judicial review. For example, he argues that Robert Bork and the originalists–intentionalists' search for interpretive authority is in vain. The Constitution was a compromise among conflicting intentions—the results of politics, not consensus. To find a clear intention in that result is illusionary. Equally troublesome as a source of high court authority are the contentions of the participation interpretists such as Justice Stone in *Caroline Products* or John Hart Ely. The robust judicial review needed to protect the democratic process and keep participation open is not as neutral as the participation interpretists have asserted. To identify a participation malfunction and (certainly) to cure that malfunction necessitate value judgments. Indeed, it is not uncommon to see a correct process leading to injustice. The Frankfurter–Thayer judicial restraintists fare no better in Wellington's view. They simply put too much faith in the Legislature's loyalty to the Constitution.

Wellington is inevitably led to the common law approach to constitutional interpretation to enhance the high court's authority and at the same time to impose sufficient restraint. In line with Alexander Bickel, Wellington contends that the Supreme Court's legitimacy will come only from the purity of its deliberative process. The Court must go about deciding cases in its own fashion distinct from executive or legislative decisional methods. Thus, authority will surround the justices' constitutional interpretations. Further, as an integral part of the high court's decisional sequence, the adversary process will provide the nexus between law and politics. To succeed at the Supreme Court level, attorneys must argue that their clients' interests are the public's interests. The interaction among the justices as they weigh the interests urged upon them by the attorneys constitutes the necessary negotiations as to which claim best coincides with the public interest. Of course, much is at stake. High court decisions still possess a serious finality; and, of course, they have immediate consequences. The losers pay a price.

However, the validity of Wellington's common law argument rests on the "digestibility" of the Court's rulings. Thus, the important ingredient becomes public morality, or what might be called *pluralistic politics*. The Court must weigh the consequences of its constitutional interpretation against a background of political noise (e.g., public protest, legislative response, media coverage, academic reaction, group activity) and within the confines of *stare decisis*. Continuity in the law is provided by *stare decisis*: "Like cases should be treated alike" (p. 12). Change is prompted by the political background noise. To illustrate the feasibility of his approach, Wellington skillfully analyzes the current abortion controversy. By tapping public morality the difficulties the Court has had in reconciling *Griswold*, *Roe*, *Stanley*, and

Webster would have been eased, and the results would have been more digestible.

This common law method of judicial review "best explains American constitutional law, and, . . . while far from perfect, it is normatively superior to rival approaches" (p. 78). Perhaps. Wellington assumes that there is a clear and discernible U.S. morality or a set of common values and attitudes that lawyers can argue and isolate but observant justices can identify: "For the Court and the lawyers who argue before it, reasoning from these commonly held attitudes should be an important method for interpreting the values—the public morality—that are a source of law" (p. 107). Further, he assumes that the path of the law through *stare decisis* is, indeed, straight and narrow, although often uphill without switchbacks, detours, or alternate routes. Also, he assumes that the justices by training and inclination are objective decision-makers without agendas who are able dispassionately to weigh competing versions of public morality. Wellington is either overly optimistic or politically naive.

Although the conversational style of the book and the sequence of chapters sometimes distracts, Wellington's ideas remain provocative and his solution worthy of contemplation. Except for his argument that the Court's authority should rest on its unique decisional process, I remain skeptical of his solution to the incongruency between the Court as constitutional interpreter and representative democracy. I am not convinced that the Court's common law results will be superior to, or more digestible than, what the so-called originalists, textualists, or structuralists might devise. Nonetheless, his book is a good read and should not be ignored by those of us concerned with the threat of "government by the judiciary."

Washington State University CHARLES H. SHELDON

Mismanaging America: The Rise of the Anti-Analytic Presidency. By Walter Williams. Lawrence: University Press of Kansas, 1990. 179p. $19.95.

Walter Williams, a former policy analyst in the Office of Economic Opportunity, has written this book in anger and frustration. His central theme is that the critically important need for expert policy information, analysis, and advice at the highest levels of U.S. government has been countered by a deleterious decline in institutional analytic capacity throughout the executive branch and a serious weakening of the policy management capability of modern presidents. Both of these developments, he argues, have contributed directly to inept presidential governance and unsound public policy. Williams is quite emphatic in his view that the executive branch's analytic and managerial structure is in a serious state of disrepair and badly in need of a major institutional fix.

Williams firmly attributes the decline in the analytic capacity of the executive branch to Ronald Reagan, the first modern anti-analytic president, who launched a successful eight-year war on policy information and analysis and destroyed the capacity of White House analytic staffs to serve the institutional presidency. Faulty policy analysis, Williams argues, led directly to the massive budget and trade deficits during the Reagan years, lost opportunities in education policy, and the saving and loans scandal. Williams is equally firm in dating the decline in managerial capability from the end of the Eisenhower administration and claims that organizational competence has now become the pivotal missing ingredient in the modern presidency.

The institutional fix proposed by Williams is not dissimilar to what the Brownlow Report attempted to do more than 50 years ago and what various post-Watergate reformers have been advocating for more than a decade now: that is, redefine the proper role of the presidential staff in the context of an organizational theory designed to achieve a reasonable balance between neutral competence and responsive competence in the Executive Office of the President (EOP). A major part of Williams's fix is his plea to restore a strong policy analysis capacity to the White House; and, while fully recognizing that policy analysis can be abused, overused, and fallible, he argues fervently that high-quality policy analysis does facilitate effective decision-making and is a necessary condition for the policy changes needed to combat current economic and social problems. Williams's prescriptive approach to the problem of presidential staffing is a welcome contribution because of the central focus on policy analysis in his design for a new-look EOP; and that is what makes this book distinctly different from, and more interesting than, other post-Watergate prescriptions for change. It introduces policy analysis as an significant but hitherto ignored variable in the debate about presidential staffing and ensures that it will be ignored no longer.

Williams's other principal concern—the lack of organizational and managerial mastery in the White House—is heavily dependent on Eisenhower revisionism. Eisenhower was an organizational virtuoso, according to Williams; and things have not been the same since. Contrary to the views of Stephen Hess and others, Williams argues that presidents *must* be managers but that Eisenhower was followed by a succession of anti-bureaucratic (Kennedy and Johnson) or antigovernment (Nixon, Carter, and Reagan) presidents who lacked in-depth experience with large-scale public organizations and undercut the organization and management capability of the executive branch by politicizing and centralizing policymaking power in the White House. It is, however, hard to reconcile the author's criticism of post-Eisenhower presidents with his admiration for Eisenhower's managerial skills. It was, after all, Eisenhower who began the politicization of the EOP by being the first president to appoint non-career heads to the Bureau of the Budget and National Security Council staff and Eisenhower, again, who showed his successors how much scope presidents had to expand the presidential staff. While Williams extols Eisenhower's "hidden-hand leadership," he does tend to ignore Eisenhower's hidden-hand staffing practices that pointed the way to the assault on the doctrine of neutral competence in later decades.

In some respects, the excesses that often accompany historical revisionism get in the way of Williams's thesis. His perspective on Eisenhower's organizational virtuosity is open to question, as are the numerous generalizations about the damage done by Kennedy's organizational leadership. In particular, the author's assertion that Kennedy mounted "a full, frontal attack on the presidency and on institutional commitment" (p. 112) is singularly unpersuasive.

Notwithstanding controversies over presidential revisionism, this book has much to commend it; and if one

accepts Williams's optimism about the possibilities of reform, then his "guiding propositions" (chap. 7) for structuring and staffing the EOP represent the most detailed and thoughtful blueprint for the role and function of the presidential staff since Brownlow. Adherents of what Williams calls "the superrealistic approach" to presidential staffing are, however, unlikely to share that optimism and will probably remain convinced that responsive competence has a much tighter grip on the nature of the U.S. presidency than Williams is willing to credit.

Australian National University JOHN HART

FDR and the News Media. By Betty Houchin Winfield. Urbana: University of Illinois Press, 1990. 277p. $34.95.

Those of us who thought we had learned all we needed to know about FDR and the media from studies by Graham J. White and by Richard W. Steele (and earlier from the research of Elmer Cornwell and others) will find fresh insights and data in this work. A professor in the School of Journalism at the University of Missouri, Columbia, Betty Houchin Winfield deals with the subject in broad compass but is adept also at analyzing Roosevelt's flair for dealing with radio and photograph opportunities, as well as with journalists.

Much has been made of FDR's charming manner and buoyant voice in his fireside chats, his skill at repartee in his press conferences, his mastery of the campaign speech. But along with all this, there was in the White House an extraordinary amount of careful planning, attention to detail, and sheer professionalism. Roosevelt had the help not only of the expertise of veteran journalists like Steve Early, the communications talents of Eleanor Roosevelt, and the writing skills of brilliant ghostwriters like Samuel Rosenman and Robert Sherwood. He enormously expanded the media impact of the White House through enlarged and invigorated techniques for reaching the populace in general and special targets in particular.

Indeed, Winfield, like a good political scientist, emphasizes this system rather than merely the impact of one man. Even more, she pictures graphically the overall context in which the Roosevelt administration operated—from the early emergency days, to the radicalized political environment of 1935–36, to the political and institutional difficulties FDR encountered during his second term, and finally to the whole new environment of world war. It was the ability of FDR and his White House to adapt to the momentous changes around them that account for his ultimate effectiveness both as Dr. New Deal and Dr. Win-the-War.

But it is also this expanded system and FDR's manipulation of it that worry Winfield as a believer in a truly free press. Roosevelt was a master of withholding information, as well as providing it; setting his own timing; playing favorites with friendly reporters while promising not to do so. He wanted to make the White House reporters his collaborators. When some correspondents rejected the enticing embrace, he treated a few of them as enemies. With his extensive network of personal friendships and the expanded influence of the White House, he could reach into the interstices of press and radio, exploiting rivalries, rewarding friends, thwarting adversaries.

Roosevelt was not loath to take on barons of opinion opposed to the New Deal, such as William Randolph Hearst, Colonel Robert McCormick, and a galaxy of other publishers. More than any president before or after, he attacked the concentration of control within the media—especially after publishers like Hearst began to buy up radio stations. When newspaper magnates charged the New Deal agencies with imperiling freedom of the press, he answered that their monopolistic power was the real threat. Thus, he triggered a great debate that sadly has not been continued in recent times.

The author ends up rather ambivalent (or should one say *fair-minded*?) in her overall estimate of FDR's impact on the media during the New Deal years. On the one hand, he provided massive amounts of information about New Deal programs, guided reporters through the complexities of novel policies, brilliantly explained his programs to the public through intimate chats and fighting campaign speeches. He was a great teaching president—the best kind of leadership. On the other hand, there was that "system" that enveloped the reporters, tended toward indoctrination, inhibited the press as "watchdog."

Perhaps a good balance was set in the New Deal years between these two tendencies of teaching and control. World War II, however, upset that balance, enormously magnifying administration control of the press, sharpening the question—as we proceed to enter more wars—of wartime controls at the hand of less benign and less democratically minded chief executives than Roosevelt. To her study of both the New Deal and war years Winfield has brought exacting scholarship, a sophisticated eye, and a penetrating understanding of the implications of news management for democracies, especially during wartime.

Williams College JAMES MACGREGOR BURNS
University of Richmond

City Choices: Education and Housing. By Kenneth K. Wong. Albany: State University of New York Press, 1990. 218p. $44.50 cloth, $14.95 paper.

A contribution to the study of urban politics and public policy that addresses a theoretical question, this book also presents a logical problem. The question is whether recent policy decisions in Milwaukee and Baltimore demonstrate the importance of politics over economics. In particular, the author poses a "political choice" model to subsume an "economic constraint" model derived, he says, from the work of economists and political scientists such as Paul Peterson (*City Limits*, 1981). The evidence developed to answer the question consists of case studies of the implementation of federal programs in the two cities. He defines two of the case studies as illustrations of redistributive issues (compensatory education and rent subsidies) and two as growth issues (vocational education and community development block grants). As one might expect, the author answers his central question in the affirmative.

Policy options from a political economy perspective increasingly revolve around the central trade-off between redistribution for the poor and economic development. Political systems as different as the emerging

nations in Eastern Europe and financially hard-pressed cities in the United States all face that choice. In cities like Baltimore and Milwaukee, local officials need investment and growth in order to reverse the decline in their economies. To them, redistribution looks like increased costs to industry leading to a foregoing of investment while dividing a decreasing pie. Against that view are the political and moral appeals of redistribution.

Wong says that the previous literature now raises "the economic constraint" as the imperative: city officials must go for development. If that is true, where does politics come into play? The author finds that one type of political process, *clientele participation*, did not work because the mobilization of African-American interests in the two cities did not have an effect. Another political factor, *institutional diversity*, did.

The impact of institutional diversity provides the crucial evidence for Wong's thesis. To find whether political factors do more than explanations based upon economic imperatives, we must look to evidence in situations where the two factors predict different outcomes. Cutting his analysis to the core, this evidence occurs in federal programs fostering redistribution in the cities. Here the economic constraint model predicts program stability or decline, whereas the political choice model predicts at least slight program growth. Wong finds that local officials implement redistribution programs in education (program growth), despite the economic factors, because of their political independence and professional values—the institutional diversity factor. To his credit, Wong recognizes a similar finding in the prior literature (J. David Greestone and Paul Peterson's *Race and Authority in Urban Politics*, 1976). Based on this finding, the author poses an integration of political and economic factors in his political choice model.

Logically, federal funds serve to relax economic constraints at the local level. They should ease the difficulty with the development–redistribution trade-off. Consequently, issues of expending federal funds may not present the best test of the limits of the economic constraint model. If the economic imperative is so overwhelming that it leads to a violation of federal guidelines, then the economic constraint model may be truly predictive. Alternatively, if our conclusion is that politics play a role only when resources come from exogenous sources, the influence of local politics may, indeed, be limited. Most scholars who understand economics do not doubt the importance of politics in, for example, establishing the regime that structures economic relations. Perhaps the problem is with the common definition of *politics*.

Wong has achieved a good discussion of important policy choices in two interesting cities and placed his discussion in a theoretical context. This book will be a useful addition to reading lists for courses in urban politics, local government, and public policy.

Florida State University CHARLES F. CNUDDE

COMPARATIVE POLITICS

Computers Inc.: Japan's Challenge to IBM. By Marie Anchordoguy. Cambridge: Harvard University Press, 1989. 258p. $25.00.

Marie Anchordoguy's *Computers Inc.* provides a bird's-eye view of the workings of the Japanese political economy by examining one of the vanguards of the Japanese success story, its computer industry. Focus is given to the myriad government industrial policies in support of computers, including import and foreign investment controls, subsidies, low-interest loans and loan guarantees, tax incentives, the promotion of cartels and industrial concentration, and cooperative research and development projects.

Special attention is drawn to one of the often-overlooked ingredients in this success story, the Japan Electronic Computer Company (JECC). The JECC would purchase hardware from computer makers and then rent it out to users at rates 40% below that of the domestic makers' major competitor, IBM. The JECC would provide secure markets for the manufacturers, boost their market share, and allow them to move down learning curves and gain vital scale economies. In the 1960s, the JECC was responsible for 65% of all Japanese computer rentals.

This well-informed, well-researched volume draws much of its empirical richness from indispensable interviews with government and industry officials. Anchordoguy also has a flair for revealing summary statistics. For instance, it is calculated that in the 1960s, Japanese government subsidies and tax breaks amounted to 46% of the industry's investment in research and development and plant and equipment; in the early 1970s, the figure rose to 57%.

Anchordoguy's primary objective is to describe the actions and policies of the JECC, the Ministry of International Trade and Industry, and Nippon Telephone and Telegraph, as well as such computer makers as Fujitsu, Hitachi, and NEC. The impressive advances made by the industry are then described. While strong on description, this work does not offer rigorous explanation. Industrial advances are not closely tied to specific actions or policies (admittedly a difficult task). No attempt is made to weigh the respective contributions of the different governmental and industrial actors. In fact, this work specifically steers away from such explanatory tasks. This is unfortunate, since much of the debate over the Japanese political economy revolves around the relative influences of statist, corporatist, Marxist, and market factors. It is also unfortunate because Anchordoguy's research contains the raw material for testing theory.

This volume takes note of numerous shifts and changes in Japanese policy toward computers. But Anchordoguy does not go sufficiently beyond her bird's-eye view of the industry to take full stock of these changes. Her own data indicate the momentousness of these changes. The figure for government subsidies and tax breaks compared to private investment in research and development and plant and equipment dropped to 25% in the late 1970s. And JECC's share of computer rentals plummeted to 11% in the early 1980s. Now, more than ever before, Japanese studies must appreciate the dynamism of the country's political economy. Insight must be provided not just into where the country is coming from, but into where it's headed.

University of Illinois, Chicago GLENN R. FONG

The State and the Industrialization Crisis in Turkey. By Henri J. Barkey. Boulder: Westview, 1990. 220p. $28.50.

It has not been very long since political scientists began to demonstrate renewed interest in the so-called field of political economy. Studies of the formulation and the consequences of economic policies, and of the interaction between the political and the economic processes of society, and the outcomes they produce, by using techniques of structural analysis, policy analysis, and economic analysis, constitute one manifestation of this interest. Henri J. Barkey's examination of Turkey's industrialization process falls within the growing tradition of such studies.

Barkey begins his analysis with a review of the evolution of import-substituting industrialization (ISI), which has been characteristic of countries that have chosen to industrialize during the twentieth century. Whereas it has proven relatively easy, he argues, to generate support for the adoption of a policy of import substitution, the policy itself seems to run out of steam and exhaust itself after a while. This exhaustion "has been credited with worsening economic and concomitant political crises that ultimately led to the praetorianization of society and the unraveling of many countries' political systems" (p. 14).

Barkey challenges the conventional wisdom that the ISI paradigm is exhausted for economic reasons, arguing instead that it fails for political reasons and that its success "rides more on the strength of state autonomy and less on its inherent economic weaknesses" (p. 23). The second chapter of the book is devoted to the explication of the argument that rent-seeking behavior (which a strong, autonomous state would have been able to resist but weaker states have been unable to control) has led to the exhaustion of the ISI strategy. The analysis of the emergence of rent-seeking behavior under ISI and its political consequences is excellent. Attributing the eventual failure of ISI to the absence of an idealized version of the autonomous state impervious to sectoral, group, or class interests, however, is less than convincing. It may also have no empirical referent. I suspect that referring to state autonomy tacitly assumes a state that is relatively homogeneous, very tightly integrated, and highly coordinated in its actions, which, as recent experience has shown us, has not existed even in totalitarian societies.

The more interesting empirical question is *who* influences the activities of the state—and how and why—under the conditions of ISI? Chapters 3 and 4 review the evolution and the implementation of ISI in Turkey, and the following chapters provide some answers to these questions. Chapter 5 is a concise and complete review of

the economic policies that have been followed by Turkey during and after the 1960s, that is, during the persistent application of ISI policies. The statistical information presented provides an invaluable reference for those who may want to compare the Turkish case with that of other societies.

Next, Barkey raises the question why economic policies like the promotion of exports were not adopted to alleviate such major problems as the extreme shortage of foreign currencies? The answer (which is political, he argues) lies in the "unwillingness to change and in the state's inability to act decisively to counteract domestic distortions" (p. 104). Chapters 6 and 7 examine the cleavages within the private sector and state–business relations in Turkey. The aim here is to identify the main features of a background that produces indecision in the business community and the state's inability to act decisively against "domestic distortions." Both chapters are well researched, detailed, perceptive, and well written. To my knowledge, they constitute some of the best available analysis of the politically relevant cleavages in the Turkish private sector and the multifaceted and complicated relations between the Turkish business world and the Turkish government. As such, they are mandatory reading for those interested in Turkey and highly recommended for students of the political economy of other countries.

Having come full circle, the conclusion reiterates the earlier idea that politics of indecision (deriving from the inability of a divided business community to produce policy choices and the inability of a weak state to liberate itself from the pressures of the business community in order to formulate appropriate policies to alleviate the economic and political problems that ISI generated) paved the way to the emergence of a bureaucratic–authoritarian regime. Whether this is entirely accurate requires further debate in light of the fact that the economic reform policies were initially devised under an elected government; that the military did not take over until after eight months; and that within a period of two years, the military tried to set the country on a path of restoration of the old policies, which was reversed only after an elected government took office.

More problematical is the fact that expressions like *autonomous state* and *autonomous state apparatus* are not sufficiently specified. Sometimes, what is meant appears to be a government that is above party politics. At other times, a bureaucracy that is free from political guidance comes to mind. Similarly, how an autonomous state would have reacted differently to the problems generated by the strategy of ISI and how it might have behaved differently in terms of economic policy are not spelled out. These reduce the effectiveness of the theoretical argument considerably.

Barkey's book may be evaluated at two different levels. At the normative level, he seems to favor ISI and an autonomous state that he thinks would retain it. Here, his argument is not sufficiently cogent and needs to be thought through in greater detail. At the analytical level, on the other hand, what we have is an excellent analysis of the consequences of import substitution on the economy, social structure, and political processes, buttressed by a description of the process by which an export orientation and integration to the international economy have come to prevail recently in Turkey. At this analytical level, Barkey's work merits not only praise but the attention of anyone who is interested in the study of the political economy of economic change and economic stabilization programs.

Istanbul University ILTER TURAN
University of Iowa

The Moral Collapse of Communism: Bland As a Cautionary Tale. By John Clark and Aaron Wildavsky. San Francisco: Institute for Contemporary Studies Press, 1990. 431p. $24.95.

The rapid and unexpected collapse of communism in Eastern Europe has raised some fundamental questions. Why did these systems collapse? Are we dealing with mistakes by individuals, or was it a shortcoming of such systems in general? Furthermore, what are the implications of this collapse for communist systems in general?

Focusing on the Polish experience, Clark and Wildavsky argue that the main reason for the disintegration of these political systems in Eastern Europe is to be found in the nature of their communist political economies. Given the close relationship between politics and economics in communist systems, such a collapse was inevitable. The failure of such systems to tolerate political pluralism and their reliance on central planning, rather than market prices, doomed them to failure. To correct this situation, the authors argue that the neo-Marxist theory of the state under capitalism needs to be turned on its head; for in reality, this theory provides "far better predictions and explanations for communist political economies than for the capitalist systems at which their criticisms are aimed."

Turning to the states of Eastern Europe, the authors argue that these states were ruled by a single class, which exercised control of the economy. Indeed, it is the fusion of power in countries such as Poland that destroyed the competition so necessary to highlight errors and the diversity necessary to correct them. As a consequence, efforts to reform such systems by administrative measures only made the situation worse.

This is a controversial but invaluable book. For too many years, neo-Marxists and their critics have tended to talk by, rather than to, each other. This book, however, provides a clearly articulated set of criticisms of the communist experience and could serve as the basis for a dialogue between neo-Marxists and their critics. In what ways is the Polish or East European experience different from that of the Third World, for example? Is the Clark and Wildavsky critique relevant only for a Leninist regime? Or is it possible to design a Marxist, non-Leninist pluralistic system that would avoid the shortcomings they outline? These issues are too important to be ignored.

The book could also serve as a useful tool in a graduate seminar on (former) communist systems by serving as a bridge between the old and the new. Not only does it highlight some of the deep-seated problems the new political systems in Eastern Europe are struggling with, but it also focuses attention on the key question of just what caused these systems to fail. Even if not all readers agree with the conclusions suggested by Clark and Wildavsky, they will certainly find the

well-documented, carefully articulated arguments difficult to dismiss out of hand.

Woodrow Wilson Center for International Scholars DALE R. HERSPRING

Oil and Politics in the Gulf: Rulers and Merchants in Kuwait and Qatar. By Jill Crystal. New York: Cambridge University Press, 1990. 209p. $37.50.

Crystal's book was written before the August 1990 invasion of Kuwait, but it is a solid piece of research whose merit exceeds ephemeral journalistic considerations. The book should appeal to several kinds of audiences: regionalists interested in the Middle East and Persian Gulf states; international relations specialists who want to know more about this strategic area and would like some solid local history upon which to anchor their postwar analyses; and theorists of state development. These last should pay particular attention to this work, for its title is not designed to catch their attention. Although the book does not pretend to make a deep theoretical contribution, there is much primary source material (based on archival research) of great interest.

The study is divided into two sections, one on Kuwait and the other on Qatar. The book establishes the extent to which Kuwait is a product of the international balance of power that has protected it from both Iraqi and Saudi threats. The development of the society and economy, especially the labor market, are also examined and related to the occasional regional disturbances that have threatened the monarchy. The need for an outside, world-class power to maintain the sovereignty of the Kuwaiti state is a repeated theme in the country's history. The headlines of the postwar period (e.g., the expulsion of Palestinians) are the tried-and-true recipes by which the regime has dealt with potential disloyalty and rebellion on many occasions in the past. Recent events are remarkable for their scale, but no one of them is new to Kuwait's politics.

Crystal's book shows the strong analytical influence of Hanna Batatu's *Old Social Classes and the Revolutionary Movements of Iraq* (1978), though it does not approach Batatu's work in scope. Like Batatu, Crystal gives a clear account of the influence of tribe and clan in two modern states and how these have interacted with the development of a class sytem that grew up with the penetration of the world oil industry. Unlike Batatu, Crystal does not focus on the development of pan-Arabist ideology or political parties, largely because their role has been more subordinated to the politics of tribe and clan in Qatar and Kuwait than in Iraq. Jealousy, fear, rivalry, greed, and shifting alliances are present in all political systems; but tribal rivalry and the lack of state institutionalization in Kuwait and Qatar give these features a particular flavor that Crystal illustrates quite well.

Crystal argues that economic development in both Kuwait and Qatar shifted a traditional balance of power between the primarily political power of the sheikhs and the primarily economic power of the merchants. Imperial backing from abroad and growing oil revenues helped ruling families in each state to consolidate their control of the state apparatus. Largesse with oil revenue broadened rulers' base of support against contentious family members and rebellious merchants. The resulting "distributive state" collected the bulk of its revenues from international trade and then distributed them to the citizenry, foregoing the need to develop an extensive domestic tax base. The merchants were either paid off directly or given other benefits, such as strongly protectionist policies, to secure their loyalty. In Kuwait, the merchant class was stronger and better organized than in Qatar: the push for representation to check the monarch has been stronger there. The more extensive ruling family in Qatar was more prone to squabbling, which was often exacerbated by Saudi meddling. Qatar has therefore been less stable internally than Kuwait.

Theorists of state development will be most intrigued by the book's analysis of class conflict. The political economy of state development in both countries was determined by such factors as the pearling industry, access to long-range trade routes, the effect of nomadism on labor supplies, debtor–creditor relationships between workers and merchants and, at the bottom of the hierarchy, the existence of a slave class. The interaction of slaves, free workers, merchants, sheikhs, and international oil companies is provocative reading, even if the theoretical implications are unexplored.

This book should properly be titled *Oil and Local Politics in the Gulf*. The international politics of oil are left virtually untouched. This is unfortunate, because there are some issues that oil histories have not explained. Examples are Kuwait's exclusion from, and Qatar's inclusion in, the famous 1928 Red Line agreement limiting oil production in the entire Saudi peninsula and Mesopotamia and Gulf Oil's decision to drop out of the Iraq Petroleum Company consortium and participate in Kuwait. But Gulf Oil is not even mentioned in the book; and the politics of the U.S. company's working in tandem with the British company is totally unexplored, as are the nationalizations of the foreign concessions in the 1970s. Crystal's book excels primarily because of its treatment of domestic politics.

State University of New York, Albany GREGORY P. NOWELL

The Hidden Nations: The People Challenge the Soviet Union. By Nadia Diuk and Adrian Karatnycky. New York: William Morrow, 1990. 284p. $22.95.

In Search of Civil Society: Independent Peace Movements in the Soviet Bloc. Edited by Vladimir Tismaneanu. New York: Routledge, 1990. 193p. $35.00.

Given the blistering pace of political change in the present-day Soviet Union and in Eastern Europe, it makes sense for Western publishers to bring out books devoted to that region quickly. *The Hidden Nations* represents such a "fast book." Completed in mid-1990, it appeared before the end of the year.

The aim of Nadia Diuk and Adrian Karatnycky's study is to recount "the great awakening of peoples that is occurring throughout the Soviet Union", (p. 32). Toward that end, they focus upon what they call the "hidden nations," that is, the non-Russian peoples of the Soviet Union. It should be noted that the authors have written a "popular" account, geared at a broad readership. In addition to serving as an essay in contemporary history, the book also advances public policy recommendations. This is to be expected from two experienced Washington hands: Diuk is Soviet and East European specialist at the National Endowment for

Democracy, while Karatnycky is research director of the AFL–CIO Department of International Affairs.

As an essay in contemporary history, *The Hidden Nations* is a solid contribution to the mushrooming literature on what used to be called "the nationalities question." In addition to scrutinizing the Soviet press, the authors conducted three research trips to the Soviet Union during 1989 and 1990 in the course of which they visited a number of the Soviet republics. The authors generally come across as well informed, and I found few factual errors in their wide-ranging account.

Diuk and Karatnycky's public policy recommendations could be summed up as an appeal for common sense on the part of Western governments. Power, they maintain is rapidly devolving to the republics. It is thus the republics, not the Soviet "center," that should be the chief object of Western attention and largesse. They also note, appropriately, that since the mid-1970s there has been occurring a significant out-migration of Russians from the Soviet periphery and that this migration pattern objectively serves to facilitate the devolution of power to the republics.

The authors contend that the West should offer "consistent moral and material support for those indigenous movements in the USSR that favor democracy and reject chauvinism" (p. 248). The West, they believe, should attempt to encourage and to strengthen the civil societies that are emerging in the republics (albeit at different speeds) and that could soon result in a number of the republics' becoming authentic Western-style democracies.

While *The Hidden Nations* exhibits numerous strengths, it also suffers from two important failings. First, there are no notes. A brief Sources section at the end of the volume fails to compensate for this glaring lapse. In the absence of notes, there is obviously no way a reader can check the accuracy of the many direct quotations that appear.

The volume's second failing is a conceptual one. The authors consistently advance the thesis that the Soviet Union must be seen as a "Russian empire" and that ethnic Russians objectively play the role of an "imperial nation nonpareil" (p. 37). For the authors, the continuities conjoining prerevolutionary Russia and the Soviet Union far outweigh any discontinuities. While Diuk and Karatnycky frequently employ the term *totalitarianism*, it seems that, for them, totalitarianism serves as a "mask" for the reality of ethnic Russian rule. "For the non-Russians," the authors assert categorically, "Marxism–Leninism is an alien ideology", (p. 39). This implies that for ethnic Russians Marxism–Leninism constitutes a form of national ideology.

This thesis cannot withstand careful analysis. The late nationalities specialist, Hugh Seton-Watson, was wont to argue that the historic tsarist empire should not be seen as an ethnic Russian empire. The tsarist empire, he noted, was based on the principle of autocracy, not any principle of ethnic Russian colonial superiority. All peoples living in the empire therefore enjoyed the emperor's protection regardless whether they were Russian by speech or religion. It was only toward the end of the nineteenth century, Seton-Watson argued, during the reigns of Alexander III and Nicholas II, that the tsarist government began to adopt a conscious policy of Russification, a course justified not in the name of the monarch but in that of the Russian nation.

If the tsarist empire cannot, except in its closing years, be termed an ethnic Russian empire, much less can the Soviet Union be so considered. As Carl Linden has pointed out: "The [Soviet] party–state in its strict conception acts not in the name of those it rules but as a proxy of the future 'communized' humanity freed of political rule and no longer separated into nation–states. By its ideology the Soviet party–state is the revolutionary replacement of the Russian nation–state, not its continuation", (*The Soviet Party–state* [1983], 95). Marxism–Leninism hardly serves as a "veil" for ethnic Russian imperial ambition.

Since the Soviet Union cannot be considered a Russian Empire, it is clear that ethnic Russians cannot be termed an "imperial people." As Alain Besançon noted in 1983, the position of Russians in the Soviet Union is dissimilar to that of, say, the English or the French in the British and French empires. Besançon maintained:

> To have an empire one must have a privileged people, an essentially military means of conquest, and limited goals. The Russian people has no privileges. It has "advantages" certainly, as the surest ally of Communism. . . . [But] these advantages are not rights. Moreover, they are compensated for by heavier obligations, exemption from which is considered by the Russian to be a privilege of the non-Russian nationalities. A Russian who enjoys privileges as a Communist owes his privileges to his Communism, not to his Russianness. (In ed. Robert Conquest *The Last Empire*, [1986] 10–11)

Unfortunately, Diuk and Karatnycky fail to see what Seton-Watson, Linden, and Besançon see so clearly. With the exception of this conceptual lapse, the authors have written a useful book that can help us better to understand the dramatic "awakening of peoples" presently taking place in the Soviet Union.

In contrast to *The Hidden Nations, In Search of Civil Society* is an example of a "slow book." A number of the constituent chapters were completed by early 1988, but the volume itself did not appear until late 1990. Despite the dated nature of some of the material, however, the book retains considerable interest for specialists.

The goal of the collection (whose editor and principal author is Vladimir Tismaneanu of the Foreign Policy Research Institute) is to focus upon the "grass-roots, unofficial, nongovernmental peace movements" (p. viii) that emerged in the Soviet Union, Poland, Hungary, Czechoslovakia, and East Germany during the course of the late 1970s and the 1980s, and to treat these movements as case studies in the emergence of "civil society" in Eastern Europe. This exercise is a worthwhile one, since it is clear that the "year of miracles" (1989) in Eastern Europe and the formation of "sovereign" Soviet republics did not occur ex nihilo. Tismaneanu and his coauthors are correct in affirming that the "islands of autonomy" that were staked out, often with great difficulty, in Eastern Europe during those decades bear a direct causal relationship to the collapse of communist regimes at the end of the 1980s.

In his illuminating contribution to the volume, Hungarian social critic and human rights activist Miklos Haraszti theorizes that Eastern European societies must pass through three distinct "eras" before they are able to cast off the communist yoke. The first stage he posits is *post-Stalinism*, during which the party–state reduces repression to a "necessary" level and permits a limited degree of consumption and of economic decentralization. During this stage, one observes the emergence of independent initiatives not directed by the party–state.

The second stage in Haraszti's scheme is *posttotalitarianism*, during which an authoritarian regime is increasingly placed on the defensive by a burgeoning civil

society. The party–state's legitimizing ideology, Marxism–Leninism, suffers a complete collapse; but the party structure remains in place.

The final stage—which occurred in Eastern Europe after Haraszti had predicted it in early 1988—is *postcommunism*, under which political democracy is installed. "What is significant," Haraszti observes, "is that without the evolution of civil society in the preceding two phases, the transition cannot be successful" (p. 86). Haraszti's scheme could, one suspects, also be of help in predicting future developments in non–East European communist societies.

The emergence of independent peace movements in the Soviet Union and Eastern Europe was preceded by "islands of autonomy" created by rock musicians and their fans (the "Lennonists," after John Lennon), environmentalists, and those seeking the preservation of historical and cultural monuments. The formation of independent peace movements was, however, especially significant, since these entities served to challenge the ruling communist regimes on closely guarded turf; the campaign for "peace" (i.e., the inducement of Western unilateral disarmament) had been a key element in the foreign policies of those regimes. The at-times-brutal contest between the independent peace movements and the regimes makes a fascinating story, as do the fractious ties that developed between the movements and various peace organizations in the West.

To sum up, *In Search of Civil Society* is a collection of stimulating essays serving to broaden our comprehension of the processes that led to the sudden collapse of communism in Eastern Europe and that could presage its imminent breakdown in the Soviet Union.

Hoover Institution JOHN B. DUNLOP

Politics, Economic Development, and Industrial Management in India. By Howard L. Erdman. Hanover: University Press of New England, 1989. 246p. $35.00.

Howard Erdman's book is a well-developed case study of a typical industry in India from its conceptualization stage to the present. Erdman justifies the selection of Mysore Chemicals and Fertilizers as a valid case on the ground that its checkered performance is representative of the overall industrial performance in the nation. The objective of this painstaking exercise (as the author himself claims) is to reach those who are either interested in the field of policy analysis or keen to move away from macro studies to the micro case studies that offer a narrow—albeit focused—view of political phenomena. The political phenomenon that Erdman explores is the effect of the political environment on the process of industrial decisionmaking.

The case study of Mysore Chemical and Fertilizers is comprehensive and provocative. Several dimensions of the "political environment" are carefully explored by the author. Under the institutional dimension, Erdman analyzes India's complex federal system and the interrelationship between the center and the state of Karnataka. The policies of the central government can—and do—cripple industrial initiatives. The author also makes a genuine effort to highlight the drawbacks of the Indian style of "mixed economy" by bringing in issues of the joint and semipublic sectors. Erdman interestingly points out that although the government's power in barring people from doing what it deems undesirable is clear, its power is limited when it comes to making people do what it deems desirable.

Under the social and psychological dimensions, the case allows the author to venture into two fascinating areas: caste politics and personal politics. Erdman concludes that the domestic political environment is important but that at the same time the "individual matters." While Erdman raises these issues in several places, he fails to explore them systematically. Caste identities play an important role in the selection of the chairman. But do they also play a role in the management of a company? To what extent are these positions used as tools to muster support for the ruling party? Is the politicization of management a cause or a consequence of the company's failed performance?

The historical account of Mysore Chemicals and Fertilizers from 1960 to 1987 is a powerful case study in itself. However, considering its richness, the framework in which the case is placed is disappointing. Rather than developing a theoretical framework by reviewing one primary body of literature, Erdman plays "touch and go" with the names of hosts of scholars. The resulting format is less like a scholarly work and more like a graduate seminar. This format is also prevalent in the concluding section, where the only conclusions drawn are that every model and every phenomenon identified in the introductory chapter is present in this case. Little attempt has been made to assess their comparative significance, to link the case with other similar cases in India, or to relate them to other studies in the field. Although the author claims that the book is useful for comparative purposes, the burden of finding its comparative worth is left on the reader.

While the case study is undoubtedly interesting, it is not an analytically rigorous work. The absence of any specifically stated propositions, operationalization of concepts, or presentation of proofs mars the study's value for wider audience. Much is left to the reader's speculation. The lack of any hard data on a subject where data abounds is also disturbing.

The readability of the book is greatly hampered by the author's preference to abbreviate almost everything, including the names of people. Often, four or five abbreviated names appear in one sentence.

Overall, the chief strength of the book is the case study itself, which is not only interesting but insightful as well. Even though it does not break new ground, it is meaningful to those interested in Indian politics.

University of South Florida RENU KHATOR

The Three Worlds of Welfare Capitalism. By Gosta Esping-Andersen. Princeton: Princeton University Press, 1990. 248p. $37.50 cloth, $12.95 paper.

Modern Welfare States: Politics and Policies in Social Democratic Scandinavia. By Eric S. Einhorn and John Logue. New York: Praeger, 1989. 340p. $55.00 cloth, $17.95 paper.

Age, Class, Politics, and the Welfare State. By Fred C. Pampel and John B. Williamson. Cambridge: Cambridge University Press, 1989. 199p. $34.50.

As their titles suggest, the three books reviewed here concern the social welfare state. Of the three, *The Three Worlds of Welfare Capitalism* constitutes the most vigorous

attempt to move beyond conceptual categories that now dominate the field. The author, Gosta Esping-Andersen, replaces notions of the welfare state that emphasize total amounts of expenditures on social programs or the degree of redistribution that the system realizes with a classification of programs according to whether the programs are universalistic and enable persons to maintain livelihoods without reliance on the labor market. The purpose of social rights, Esping-Andersen says, is to "permit people to make their living standards independent of pure market forces" (p. 3). She argues that with respect to living standards, social rights are supposed to enable citizens to be more than mere commodities. They should lead to the "decommodification" of citizens.

With this idea of the purpose of social rights in mind, she draws distinctions between three types of welfare state. First, the *liberal* welfare state mixes nonuniversal means-tested programs for the poor with programs for all who contribute to them based on a social insurance foundation. Second, the *corporatist–statist* welfare state emphasizes social insurance programs that benefit those who pay in—a larger number than in the liberal state but still excluding some sections of the population (e.g., homemakers). Third, the *social democratic* welfare state emphasizes programs that are universalistic (i.e., independent of whether people do or do not pay in) and also grant benefits that are tied to a middle-class style of living. This third type of welfare state thus enables an able-bodied person of working age who so wishes to live independently of the labor market for long stretches of time (e.g., when unemployed, sick, or disabled or to pursue education or raise children).

Esping-Andersen's book is intriguing in its suggestion of fresh hypotheses about the welfare state. First, she uncovers little correlation between the gross national product of a nation and the type of welfare state a nation has; her comparison of 18 Western countries shows that the politics of a nation is a far more potent explanatory variable. Second, tax revolts against the welfare state tend to be strongest not in nations for which the welfare state poses the greatest tax burden (the universalistic systems) but in those that spend the least on welfare. Also important is the connection she draws between the commitment of a system to universalism and the dependency of such systems upon full employment in order to garner the necessary financial resources to fund the system. The result is that though they may seem on the surface to be indifferent whether people work, universal welfare systems are instead geared toward attempting to increase employment with the consequence that these systems are associated with higher levels of employment than the other two types of welfare systems and often achieve full employment. These comprise a few of the ways in which Esping-Andersen's approach invites rethinking a wide range of conclusions that presently have wide currency in the field.

Yet drawbacks also exist. The author mentions at the start that her need to compress a lot of information into a small space for the purposes of developing measures for comparison might lead some to feel that her descriptions of the various welfare states are superficial and perhaps misrepresentative. I had this feeling on some occasions with respect, for example, to the West German and U.S. welfare systems. Also, her emphasis on independence from the labor market as a key element of the definition of the universal welfare system needs to be spelled out in greater detail. After all, the universal welfare system itself places limits in the amount of time people will be supported without working. What defends these limits placed on support if independence from the labor market is a vital criterion? Also, the financial viability of a universal welfare system may well depend upon a work ethic operating in these countries that infuses people with the belief that it is *not* acceptable to be independent of the labor market except in special situations. Quite a few anomalies exist, unfortunately, that Esping-Andersen, in this otherwise excellent work, leaves not only unanswered but mostly unaddressed.

A companion to Esping-Andersen's book is Eric S. Einhorn and John Logue's *Modern Welfare States*. The book describes in detail the welfare systems of the universalistic type as they operate today and as developed historically in the Scandinavian nations. Although the book's explanations are impressionistic in nature rather than backed by systemic evidence, they are nonetheless imaginative and worth consideration. According to Einhorn and Logue, the universalistic welfare state arose in Scandinavia due to causes reaching well beyond economic development—which is generally in accord with Esping-Andersen's conclusions. A key factor behind its emergence, in Einhorn and Logue's view, was the popular belief system—in particular, the belief in solidarity, that is, a sense of belonging to, and identifying with, others and a willingness to tie one's fate to that of others. This variable, imbedded in the political culture of the nations, loomed of greater importance than economic variables.

Einhorn and Logue's analysis reaches even further. It also focuses on the decline of the consensus favoring the extensive welfare state. The decline of consensus developed in Scandinavia following the mid-1960s. Here economic variables do move more center-stage. The authors suggest that the affluence of the age "undermined the appeal of the social welfare system. Economic insecurity seemed increasingly remote" (p. 20). Political variables also played a role, in that in the late 1960s and early 1970s there emerged value issues (environmental conditions, nuclear power, entry into the European Community that cut across traditional class lines in a manner as to reduce the cohesion of the vote for the Left. Most importantly, however, Einhorn and Logue observe that a universalist welfare system (as Marx said of a capitalistic system) may contain the seeds of its own demise. A universalistic system builds upon a feeling of solidarity that gives rise to the system and is needed to sustain it, the authors argue. Yet by its very nature, the operation of a universalistic welfare system works to undercut solidarity values. The very purpose of the system is to enable individual citizens to be economically independent of one another, whereas in prior times ties to families and friends were required to attain material security. "The welfare state," say Einhorn and Logue, "diminishes the reciprocal responsibility of each individual for his or her family, neighbors, and workmates. . . . It is ironic that the collectivist means that conservatives always feared would destroy individualism have, in fact, encouraged it" (pp. 267–268). An important and intriguing hypothesis, it unfortunately remains little more than that, since beyond their logic, the evidence the authors summon comprises little more than a quote from a Swedish actor.

The third book, Fred C. Pampel and John B. Williamson's *Age, Class, and the Welfare State*, is also of consid-

erable interest, although its denser prose makes it more difficult to read than the other two books. If the book by Esping-Andersen reveals the impact that conceptual understandings can have on conclusions about the welfare state, Pampel and Williamson do the same with methodological issues. Their study of the welfare state differs from the conventional comparative methodology in that it focuses on longitudinal rather than cross-sectional data for the nations under study and simultaneously examines the several constituent parts of the welfare state separately over time, rather than only the whole or a single program within the whole. From this different methodology emerges a series of conclusions that conflict with those derived from the conventional methodologies. First, political variables once more come to the fore in explaining change in expenditures. Although economic growth remains a critical variable for change in expenditures on the largest programs (social insurance and health care), the political context is found to be an important mediating variable. Particularly powerful are changes in the percentage of the population that votes and the degree of electoral competition that exists. Moreover, for the smaller programs (e.g., public assistance, family allowances, and unemployment benefits), Pampel and Williamson find that economic growth is unimportant. More influential in the development of these programs are changes in the scope of union power in the nation and also, in the case of family allowances, the percentage of the population that votes.

University of Arizona JOHN E. SCHWARZ

States and Urban-based Revolutions: Iran and Nicaragua. By Farideh Farhi. Champagne: University of Illinois Press, 1990. 147p. $29.95.

Of all the conceptual challenges facing the comparativist, the study of revolutions is perhaps the most difficult. In the quest for comparability and equivalence, the student of comparative revolutions needs to consider the total millieu of the revolutionary situation—politics, culture, economics, sociology, and social psychology. All too often, this has been an impossible task. To complicate matters, most scholars tend to focus on successful revolutions, rather than failures. Social scientists might do well to accept Thomas H. Greene's painful but audacious assertion that it may be easier to carry out a revolution than to understand it.

The author of the book at hand is well aware of the immensity of these conceptual and analytic difficulties. Yet Farideh Farhi is courageous enough to attempt a comparative macroanalysis of two urban-based revolutions—the overthrow of the Iranian monarchy and the Samoza regime in Nicaragua.

Using a case study approach, Farhi takes Theda Skocpol's theory of revolution as a starting point and proceeds to develop her own explicit framework for application to the Iranian and Nicaraguan situations. She presents a cogent critique of Skocpol's approach, which she finds wanting in understanding societies that lack agrarian bureaucracies and peasant insurrections. In Farhi's view, Skocpol's theory can become more pertinent to recent revolutions if focused on the changing balance of class forces occasioned by the uneven development of world capitalism and by a broader view of ideology stressing the importance of ideas, actions, and interests. Farhi's specific concern is to understand the interaction of class and state structures within the global political and economic system. Her findings can be summarized under three headings.

First, the impact of sociostructural and world historical forces on the economic sytems of Iran and Nicaragua produced a coalescence of urban groupings able to play an oppositional role against two economically interventionist regimes that favored development policies based on the export of primary products. The disoriented urbanization process created a readily mobilizable population.

Second, the coalescence of the urban groupings was actualized because of the disjunction between the ruling elite and the intermediate classes. Political parties were prohibited and the rising professional classes were antagonized. The coalescence of opposition groups was also related to the effective use of the existing religiocultural networks and ideologies based on religion, which offered the revolutionaries a medium of nationalist expression.

Third, the breakdown of the state machinery and its inability to respond to popular mobilization was a consequence of the peculiar structure of the two regimes, a world milieu of superpower rivalry, and confusion among U.S. policymakers.

In a concluding section, Farhi identifies certain similarities between the state-centered crises that launched the Iranian and Nicaraguan revolutions and those that triggered the upheavals in Mexico (1910–20), Cuba (1956–59), and the Philipines (1986). In the end, she returns to pay homage to Skocpol, her original source of inspiration, while underlining the difficulties inherent in applying her hypotheses to Iran and Nicaragua. One finds Farhi's sense of loyalty somewhat exaggerated, in view of her major departures from Skocpol that render the latter's theories almost unrecognizable. Farhi's focus on urban mobilization, ideology, urban oppositional groupings, and the vulnerability of peripheral states to internal and global pressures make her work significant in its own right.

Yet Farhi's commitment to her particular approach tends to deemphasize or hide such salient factors as the role of revolutionary leadership and the explosive interactions of culture and class. There is little in this study about the epicentric role of Ayatollah Khomeini and the Sandinista leadership in contributing to the success and shape of the revolutionary outcomes. In the Iranian case it would have been helpful to note the peculiar convergence of class and cultural cleavages that produced a revolutionary environment in which the urban *lumpenproletariat* was readily mobilized, since it suffered relative deprivation both in cultural and economic terms. On the crucial revolutionary role of the intermediate classes, the author might have consulted Leonard Binder's seminal work on Egypt (*In a Moment of Enthusiasm: Political Power and the Second Stratum in Egypt*. Chicago: University of Chicago Press) that goes beyond Gramsci to Marx and Mosca to demonstrate how a section of civil society can arouse in itself and in the masses a moment of enthusiasm and acts as the general representative of the whole society to achieve a partial revolution.

Nonetheless, Farhi's work is a fine example of how a doctoral dissertation can be transformed into a good book—a welcome addition to the literature on comparative revolutions.

University of Southern California R. HRAIR DEKMEJIAN

Coffee, Contention, and Change in the Making of Modern Brazil. By Mauricio A. Font. Cambridge, MA: Basil Blackwell, 1990. 351p. $49.95.

Comparative historical sociologists, including Barrington Moore, Theda Skocpol, Charles Tilly, and Fernando Henrique Cardoso, have contributed much to the study of economic, political, and social change. Mauricio Font's analysis of the dynamics of change in the state of São Paulo leading to the Brazilian Revolution of 1930 fits into this analytical tradition.

Font emphasizes the impact of changes in Brazil's agrarian export sector that contributed to the "onset of industrialization and the demise of the Old Republic in Sao Paulo" (p. 270). Differing with some of Brazil's most respected sociologists, including Florestan Fernandes, he challenges the often-advanced argument that "development and industrialization in Sao Paulo emanated from processes of capitalist accumulation linked to large planters" (pp. x–xi). Font contends that the emergence of independent agriculture and the associated competitive economy, arising outside the large *fazenda*, contributed significantly to this process of change. His assertions are sufficiently well documented in primary and secondary historical materials to merit close examination.

Font emphasizes two dimensions of São Paulo's independent agrarian economy in the 1920s, 1930s, and 1940s: (1) coffee production on small farms of less than 50 hectares, or 20 thousand trees and (2) the cash crop production—predominantly on small farms—of maize, beans, rice, cotton, and sugarcane (pp. 14–18). Like *planters* in the *fazenda* system, small landholders in the independent coffee sector produced for export, while the independent producers of other crops targeted internal markets. The two activities—often combined on the same farm—arguably produced a class of farmers and a competitive economy that operated relatively independently of the large coffee producers and their commercial agents. Their success gradually eroded the social and political influence of the planters. Font concludes, "By the early 1920s an alternative coalition made up of a growing mass of small and medium holders, land developers, new commercial intermediaries, and professional politicians was challenging the economic pre-eminence of established large planters" (p. 29).

These developments, Font asserts, contributed much to the demise of the old republic and the emergence of industrialization. The shortcomings of the *fazenda* coffee economy and the emergence of an "alternative agrarian economy rooted in a mode of production different from the very large established estates" created "expanded opportunities for capitalist industrialization and development" in São Paulo (p. 107).

For political scientists, Font's documentation of the process in São Paulo by which the "state" gained autonomy from the "established elites" is especially important, both factually and theoretically. He demonstrates that the political influence of the planters declined as a consequence of the emergence of an "alternative statist coalition" composed of "independent agriculturalists" and "fledgling industrialists" who benefited from São Paulo's growing export economy (p. 271). At least in the Brazilian case, state autonomy increased when society became more pluralistic.

Font enhances the value of his book by demonstrating how his findings challenge or support those of other scholars. For instance, he refutes André Gundar Frank's thesis that "specialization in the production of primary products dooms countries and regions to subordinate positions in an international division of labor and thereby to underdevelopment and a rigid social structure" (p. 272). Instead, Font shows how "export [agrarian] sector expansion can lead to substantial forms of social change and development" (p. 273). Also, Font illustrates how the development of an "alternative economy" in São Paulo that "fueled the domestic market, provided cheap inputs to other sectors of the economy, and made possible the continuous experimentation with small-scale accumulation and entrepreneurship" can be characterized in terms of Albert Hirshman's notions of "backward" and "forward" linkages. Additionally, he contrasts the *fazenda* system and the "alternative economy" in terms of Alain de Janvry's notions of "disarticulation" and "articulation." These conscious attempts to compare and contrast his analysis with that of other scholars enhance the theoretical value of the book.

Surely, this book will evoke criticism from scholars whose work it consciously challenges. It cannot be ignored. The debate will not be definitively resolved, however, since both Font and those whose findings he has questioned can find "facts" in the historical record to confirm their respective claims. The critical dialogue enjoined by Font, however, cannot help but clarify our understanding of the dynamics of economic, social, and political change.

Font does not address how changes in the global political economy affected Brazil's 1930 revolution. Erosion of the *fazenda* system, at least in part, was the product of the decline in coffee prices. In the late 1920s and early 1930s, the world supply of coffee steadily increased as producers from other countries entered the market; at the same time, coffee consumption slowed. These reversals, coupled with the Great Depression, undoubtedly contributed to Brazilian revolution. Font's neglect of these and other international factors weakens his analysis.

Still, Font's book contributes to our understanding of Brazil and to the theoretical literature on the dynamics of economic, political, and social change.

Brigham Young University W. Ladd Hollist

Fire in the Rain: The Democratic Consequences of Chernobyl. By Peter Gould. Baltimore: Johns Hopkins University Press, 1990. 163p. $19.95.

The Truth about Chernobyl. By Grigori Medvedev. New York: Basic Books, 1991. 274p. $22.95.

Five years after the nuclear and environmental disaster at Chernobyl dramatically captured the world's attention and in light of new information showing that the magnitude of this catastrophe was, and continues to be, much worse than had been thought, two excellently written and genuinely gripping accounts are available that attempt to tell the story behind the worst nuclear accident in history. In many respects, Chernobyl will forever represent a truly reflective type of Promethean apologue whose moral and ethical messages extend beyond the geographic, politico-ideological, or cultural boundaries of any one nation—certainly beyond the chain of cataclysmic events that so memorably took place on 26 April 1986.

Peter Gould's *Fire in the Rain* not only tells the story behind the Chernobyl accident (which he has written in an informative, scientifically demystified, and most absorbing style) but also raises a number of keenly perceptive questions about the extent to which the dissemination, manipulation, and suppression of information about the accident and its effects may have been influenced by myriad politico-economic considerations, and, perhaps most significantly, by the degree to which a country is dependent upon atomic power. Gould, who, during the research for this book, covered well over five thousand pages of official reports, eyewitness statements, scientific treatises, and media accounts, convincingly tries to capture this important correlation between a nation's dependency upon atomic power and the degree to which information about the Chernobyl accident and its effects was manipulated or suppressed for no less than 26 West and East European countries (pp. 112–19). At minimum, the nuclear accident at Chernobyl created its own complex information crisis from which all nations can and must learn, and Gould's examination of the range of governmental reactions and responses—especially how and when information was made public in both Eastern and Western Europe—is impressive and instructive.

Grigori Medvedev's *Truth about Chernobyl* also tells the story leading up to the accident and its subsequent cover-up. Medvedev was chief engineer at Chernobyl when it was being constructed during the 1970s and at the time of the 1986 accident was deputy director of the Central Directorate for Power Station Construction in Moscow (a department of the Ministry of Energy), where he was in charge of construction at nuclear power stations. Within days of the accident, Medvedev returned to Chernobyl to investigate the magnitude of the disaster officially.

The Truth about Chernobyl is an engrossing and masterfully written account based upon interviews with the directors, engineers and others who worked at the plant, the firefighters and helicopter pilots who valiantly tried to extinguish the fire, eyewitnesses to the explosion, the doctors and nurses who treated radiation victims and risked their lives to save others, and many key people who died from radiation within weeks of the accident. Medvedev's book is, without question, the most authoritative statement on the Chernobyl accident to date. Indeed, in the preface Medvedev notes that he sought to answer a number of crucial questions he believes have been on the minds of many in both the Soviet Union and abroad: By what route did we arrive at Chernobyl? What actually happened, especially on the night of the explosion and over the next few days? What caused the disaster? What were its consequences? What were the lessons of Chernobyl? What does the future hold? (p. x).

Publication in the Soviet Union of *The Truth about Chernobyl* in 1989 was itself somewhat of an eventful test of glasnost, Medvedev's uncensored account quite legitimately taking its place among the growing number of precedents by which we assess the "new thinking" behind Gorbachev's media and information policies. Published in the United States to coincide with the fifth anniversary of the accident, *The Truth about Chernobyl* is skillfully translated by Evelyn Rossiter and contains a genuinely important forward by the late Andrei Sakharov.

What are the lessons and democratic consequences of Chernobyl? To what degree has the economic and political fallout from the accident increased public opposition to the future of nuclear development in the Soviet Union and abroad? Should nuclear power continue to be developed? If so, who should define and direct the course, tempo, and extent of such development? By asking such questions, both Gould and Medvedev raise a number of crucial socio-scientific issues that certainly predate Chernobyl and once again highlight fundamental concerns that continuously remind us "that humankind is still trying to come to grips with the fantastic, powerful forces which it has brought into being, and is still only learning to use them for the sake of progress". (Mikhail Gorbachev, speech 18 August 1986). In many respects, Gould articulates this delicate relationship among the physical, living, and human worlds as simply and effectively as anyone can: "The problems of atomic energy programs are not scientific problems in any strict sense. They are moral problems, and they arise from the apparent difficulty many have of moving from the demonstrating of the inevitable to the deliberation of the variable" (p. 145).

Among the most important lessons of Chernobyl is the question it raises about how governmental bureaucracies, politicians, and scientists (whatever their political or ideological suasion) manage an atomic or ecological crisis that knows no political or ideological boundary. Both works also raise a number of questions essential to the future of nuclear energy throughout the world: Who should be involved in making decisions regarding the development of atomic power, the location and construction of nuclear power stations, and the disposal of radioactive waste? In sum, who should define the issues and make the decisions that will, without doubt, have an impact upon the delicate balance among the physical, living, and human worlds? And whatever direction the development of nuclear power takes in the future, Andrei Sakharov (himself an advocate of nuclear energy) warns us, we must not leave the answers to these crucial issues solely to technical experts—still less to bureaucrats (p. viii).

In sum, both *Fire in the Rain* and *The Truth about Chernobyl* are excellently written works of gripping detail that are, without question, of essential benefit to anyone interested in the history of nuclear power development in the Soviet Union, the way nuclear crises and information about such crises are managed (or mismanaged), the impact the accident had upon the countries affected by Chernobyl's radioactive fallout, the lessons and democratic consequences of the world's worst nuclear disaster, and the future of atomic energy in both the Soviet Union and abroad.

Soviet Academy of Management JOHN MICHAEL CARFORA

The Economics of Political Violence: The Effect of Political Instability on Economic Growth. By Dipak K. Gupta. New York: Praeger, 1989. 329p. $49.95.

Gupta's book is a wide-ranging, high-quality work in the best traditions of the new political economy. The author is clearly well-trained in both political economy (including far more than public choice and economics) and statistics. The book contains an interesting and original model of political rebellion and a careful, methodologically sophisticated study of two aspects of the

political economy of violence in 104 noncommunist countries: the effect of violence or rebellion on their economies and an attempted estimate of the basic behavioral model of individual behavior. Its gravest fault can be easily overcome by a willful reader: the literature review sections are too long. The informed reader will want to skim these chapters (primarily chaps. 2 and 3) quickly and get down to business. And the business is rewarding.

Gupta begins with a general theoretical model of political violence. In doing this he builds on the obvious (and by now generally accepted) insight that the great sociological deprivation theories of political rebellion (e.g., Merton's) did not tell us when individuals will *not* engage in violence. To do this, Gupta uses the best ideas from the rational choice school, along with other psychological and sociological factors. He notes that to get to reasonable levels of participation in a rebellion by rational individuals, one has to get beyond a classical, self-interested model of rationality. His moves in doing this may not be to the liking of all informed public choice types; but it is clearly in keeping with the earlier ideas of Brian Barry and others in their critiques of the Downsian voter.

To get beyond these earlier discussions, Gupta makes a smart move: he introduces the notion of *ideology* as a *preference for benefits* flowing to the groups to which the individual belongs (p. 142). (One might wonder whether the model would not be richer if it were defined in terms of groups with which the individual personally identifies and groups that the individual would like to deprive.) This permits the author to introduce such sociological variables as group consciousness, cohesiveness, and mobilization into his theory. Mobilization, Gupta notes, really requires an organizational component, as well—and some expectation of positive gain. The latter clearly relates to counterrevolutionary sanctions or coercion. All this is nicely integrated in the model put forward in Chapter 5.

Gupta then employs calculus to show the marginal conditions that must be satisfied for participation to be rational. These are used to develop models of likely cycles of violence in systems with specific parameters. Some of these are later estimated with the data. In the arguments that flow from the model, it would appear that the author had an eye toward asking questions regarding the current U.S. experience, namely, considerable continued deprivation for our minorities and yet relative absence of serious political violence. But these questions are not developed in the book as it moves to an aggregate political economy level to investigate the effect of violence on economic growth.

Data from 1948 to 1952 from the *New York Times* are presented for 104 countries, but a far more restricted subset of years is statistically manipulated to do the data analysis. The analysis is quite broad and includes simulations of income redistribution patterns to generate economic growth, the prediction of political violence in countries, the loss of income attributable to instability, and so on. The data analysis is extraordinarily interesting, even when some of the results are only marginally significant, and others not surprising. The program of statistically testing a micro behavior hypothesis with macro system data is problematic, but, at least, the author seems aware of the pitfalls. The limitations of the results are reflected in the relative thinness of the policy conclusions.

These disappointingly thin levels of significance can be looked at in at least one very positive light. Given the strength of development of the approach and the statistics developed, students of political economy, political violence, and political stability have been given food for thought that should serve them in good stead for the development of the next generation of studies on these subjects. And if these students' tastes do not lead them to begin with the material presented in this book, they are likely to be embarrassed by their critics.

University of Maryland, College Park JOE A. OPPENHEIMER

The Battle for Britain: Thatcher and the New Liberals. By Stephen Haseler. London: I. B. Tauris, 1989. 195p. $24.50.

Cutback Management in Public Bureaucracies: Popular Theories and Observed Outcomes in Whitehall. By Andrew Dunshire and Christopher Hood. New York: Cambridge University Press, 1989. 253p. $65.00.

Local Government and Thatcherism. By Hugh Butcher, Ian G. Law, Robert Leach, and Maurice Mullard. New York: Routledge, 1990. 171p. $48.00.

These three works on Thatcherism and policy will be valuable for scholars of either British policy or comparative conservative movements in the 1980s. Haseler has written a polemic essay from a neo-radical perspective, while Dunshire and Hood and Butcher and his colleagues both provide full sets of arguments and data on the effects of government policy changes. These two are complementary: both adopt official figures and match them to policy goals, but Dunshire and Hood take the Whitehall level while Butcher and his colleagues examine the effect at the local government level. It was, of course, conflicts with local government spending and taxation that partly undermined Mrs. Thatcher's final administration of 1987–90.

Haseler presents a clue to the ability of the radical Right in Britain to co-opt members of the center during the 1980s. He has shifted from the Labour party and Greater London Council to the Radical Society, Social Democratic Party, and Alliance. Yet the essay presents not opposition to, but startling agreement with, the Thatcher government.

The steam engine of this readable essay is the liberalism of the early nineteenth century radicals, Cobden and Bright. The Victorian values of Mrs. Thatcher and those of Haseler overlap in the social agenda and economic prescription, if not in partisan standard. Cobden and Bright stood for free trade and the abolition by mobility of the old, stultifying, feudal class relationships; they lamented that the industrial revolution had made mill-owners and merchants wealthy only to find them sycophantic in face of the feudal aristocracy.

The journalist in Haseler might appreciate William Safire's distinctions between the *libright*, or libertarian strain (Adam Smith) of liberty in social and economic markets and the *tradright*, or traditional strain (Edmund Burke) based on a natural order of governing through the upper social class. The passing of the conservative leadership to a younger "one of us" (self-made man) certainly fits the social change emphasis of the volume, which polemicizes *against* the paternalistic, "allegiant" society (as portrayed by Philip Norton) and *for* a more

U.S.-style open social order. It is weaker in qualifying the argument in the light of real policy in the United States and the United Kingdom, and there is no explanation or critique of public choice theory.

There is little analysis of Mrs. Thatcher's partisan "handbagging" of other institutions, such as the Greater London Council (replaced with unradical appointive boards). Similarly, her rock-solid support for the police and intelligence services, her political (if not budgetary) support of the armed forces, and her vociferous opposition to the European Community's development, are all testimony to a traditional conservatism of sovereignty, security, and state power abroad.

One would like to see more systematic dissection of radical thought and more consideration of the institutions that have felt the handbag (e.g., the legal, medical, and educational professions in the final administration).

The Dunshire and Hood volume is a major, grant-supported work attempting to test hypotheses about cutback management. The chapter introductions are trenchant and witty for such a heavy volume of data, and there are many nuggets among the hypotheses, although tracking the supportive findings requires frequent cross-referencing. The authors themselves properly point to limitations of the official data: voluminous dross, rather than a few solid cases.

One of the themes is the deep retrenchment in the sheer number of civil servants, which they properly locate in 1976–85, beginning under the International Monetary Fund–influenced Labour government of 1976–79. This 20% reduction, they point out, is the third greatest in the twentieth century (after Geddes' Axe of the Depression and the demobilization following 1945). Even then, a third of the recent cuts occurred in factory privatization, rather different from the earlier deep cuts in nonindustrial white-collar positions.

Their examination of the effects of bureaucratic vulnerability provides a welcome extension of theoretical work by Charles Levine and by T. Beck Jorgensen (among others) and a particularly welcome empirical testing. Their amusing trialogue between credos on the consequences of bureaucratic slimming adopts Leaner Means Fitter as the official religion, with Leaner Means Anorexia as the commonest dissent. The third (*gatto partismo*) implies that a leopard cannot change his spots.

The researchers are sensitive to secular economic trends: the loss of blue-collar jobs to electronic transfers and automation merely found its political expression, not its driving force, in Thatcherism. They argue that the scope for major cuts has been taken already but that a plate-tectonic shift of popular expectations will keep the contract state rather than the direct service state as the dominant model. They leave open the possibility of a future government finding a higher plateau of direct services. Generally, Dunshire and Hood enrich our debate over the consequences of Thatcherism considerably.

Butcher and his colleagues are concerned with "radical consumerist reforms," or "surrogate market mechanisms" introduced into local government but not blinded to the "arithmetic politics" of pluralism. In the process, the authors are keenly aware of the paradox of proactivity to roll back the state; they evaluate the process as highly disintegrating and partisan, an aspect that is perhaps underemphasized in the other two volumes. It may have been a theory of public choice that led to the poll tax, but its effects were both partisan and particularistic. They consider the challenge to local government not only a recasting of the social order (as would Haseler) but also as refashioning the local political process. They find three models of local political authority in the 1990s: *contract*, embracing both changes of order and process; *business–corporatist*, combining collectivist (economic development) values with particular political interests; and *enterprising* (adoption of user fees to expand services), which is antistatist but serving particular interests.

All three volumes should find a place on our shelves: Haseler for the intelligent center of scholars and persons; Dunshire and Hood's and Butcher and his colleagues' volumes are both treasure troves for public administration scholars. Haseler is rather laudatory of the Thatcher administrations, Dunshire and Hood less so, and Butcher and his colleagues more critical. One specter they all detect is that the central models of administration in the 1990s will be heavily influenced by the popular, plate-tectonic shift harnessed by Thatcherism. The two data-driven volumes also indicate that the Thatcher administrations have not removed the value of official statistics.

University of Northern Iowa JEREMY R. T. LEWIS

Administrative Argument. By Christopher Hood and Michael Jackson. Aldershot, UK: Dartmouth, 1991. 221p. $49.95.

Over 40 years ago, Herbert Simon denounced much of the conventional wisdom of public administration, calling most of the field's popular precepts mere "proverbs" lacking substantial theoretical foundation or systematic empirical support. Simon called on scholars interested in administrative matters to apply the rigorous tools of "normal science" to those proverbs, in order to separate fact from fiction and thus improve the field. Today Christopher Hood and Michael Jackson conclude that the research program that Simon proposed and in which he has long participated has failed to eliminate many of the traditional proverbs from the lexicon of administrative theory. This is not because of the merit of these proverbs, according to Hood and Jackson, but because their persuasiveness is not based upon normal science. In light of this, Hood and Jackson choose not to examine the validity of administrative arguments but to describe, compare, and catalogue them in an attempt to understand their acceptance and resilience. In doing so, Hood and Jackson try to explain how and why certain administrative doctrines are adopted in certain places at certain times.

Almost half of the book consists of short descriptions of 99 doctrines gleaned from the literature. Each is discussed in terms of different kinds of justifications, including sigma-type (representing a concern for efficiency), theta-type (concerning fairness and accountability), and lambda-type (ensuring reliability, robustness, and adaptability). In examining three historical case studies of the adoption of administrative doctrines (the British Northcote–Trevelyan Report of 1854, the U.S. Brownlow Report of 1937, and the New South Wales, Australia Wilenski Reports of 1977 and 1982) Hood and Jackson identify six attributes that constitute an "acceptance factor." These attributes concern the rhetoric, presentation, and timeliness of the doctrines rather than

their internal content. Hood and Jackson claim that many in mainstream public administration fail to appreciate the rhetorical, symbolic, and metaphorical importance of their ideas and that this has limited the extent to which their advice has been sought and their ideas accepted.

The authors are quite successful in making a case for the rhetorical importance of administrative argument. They have also provided readers a handy summary of many diverse and often contradictory ideas about administration. Political scientists, however, will probably be disappointed that Hood and Jackson put little emphasis upon the role that interested parties have in urging the adoption or rejection of particular doctrines. The substantive impact that the implementation of doctrines may have on politically important interests is given little systematic treatment. This will be hard for many readers to understand.

Another difficulty concerns the rationale behind the components of Hood and Jackson's "acceptance factor." These attributes are said to fit the three cases examined in the book; but (as the authors remind us) advocates for doctrines tend to be selective about their choice of evidence. Perhaps Hood and Jackson are similarly selective about what they report. Certainly, their "doctrines" about persuasiveness at this point are little more than proverbs, lacking much evidence or theoretical underpinning. Only time will tell whether their ideas will be accepted.

Texas A&M University JAMES C. CLINGERMAYER

The State of Martial Rule: The Origins of Pakistan's Political Economy of Defence. By Ayesha Jalal. Cambridge: Cambridge University Press, 1990. 362p. $44.50.

"Midnight's children"—India and Pakistan—share a common history. Both were born out of an undivided British India in 1947. That Muslims and Hindus were not simply two separate religious communities but also two distinct nations was the argument of Mohammed Ali Jinnah, father of Pakistan and its first head of state. Of the nearly 100 million Muslims of undivided India, constituting one-fourth of the country's population, 65 million became citizens of Pakistan. About 35 million were left behind.

Whatever happiness a Muslim homeland created in 1947 was soon gone. Jinnah died within a year of the partition. Liaquat Ali Khan, second in command, was murdered two years later. Kashmir, a Muslim-majority state, did not join Pakistan; and a war with India did not settle the issue. Pakistan could not even formalize a constitution until 1956; and in 1958 the military stepped in with a martial law regime. The *first* national elections took place as late as 1970; and the bliss of their first free elections soon turned into the nightmare of yet another partition as East Pakistan became Bangladesh, decimating the two-nation theory and shattering Pakistan's army in a war with India. A shattered army did give way to civilian rule; but eight years later, Prime Minister Bhutto was hanged by a new military ruler, who was himself killed in a place crash in August 1988. A shaky democracy has been operating since then. Pakistan has lived under six constitutions and the military has ruled for almost half of the country's life. In contrast, with a brief exception, India has managed to retain a democratic political system which, though weakened over the last 15 years, has survived elite misbehavior, political assassinations, and mass violence.

Why has Pakistan been largely authoritarian, ruled by a military–bureaucratic coalition? This question is at the heart of *The State of Martial Rule*. Pakistan's unending political bunglings and misfortune placed on its scholars a painful burden of contrast with its neighbor. Moreover, dissent and critique are not always appreciated in authoritarian systems. If India had gone through similar tragedies or had crushed freedom of expression, it would have been very difficult to be a self-respecting political scientist or historian in India. For a long time, only one Pakistani scholar writing on the country's politics, Khalid bin Sayeed, was able to meet international standards. Others either engaged in diatribes written from faraway diasporas (e.g., Tariq Ali) or, if left in the homeland, wrote ill-constructed valentines and laments. Happily, this situation has started changing. Over the last decade, some notable commitments of intelligence and candor have been in evidence. Ayesha Jalal belongs to this new generation of scholars.

To understand Pakistan's authoritarian post-1947 history, she argues, one has to investigate the nation's troubled childhood when a civilian experiment was tried out. Four factors militated against democratic rule. First, the Muslim League (the party leading the movement for Pakistan) had virtually no organizational base in the land mass that became Pakistan. The league was strong in Muslim-*minority* areas of British India, whereas Pakistan was created in the Muslim-*majority* areas. (India was blessed with the huge, nationwide organizational machinery of the Congress party.) Second, Pakistan had to construct not only a party organization but also a state. The state machinery of British India was lodged in Delhi. Pakistan's government was "without any ground under its feet or a roof over its head" (p. 29). Its share of spoils from British India also left it abysmally poor. Third, Pakistani Muslims may have shared a religion; but they came in varying ethnic, linguistic, and cultural hues. The provincial pulls began to overpower any cement that Islam might have provided. Fourth, Pakistan felt a hostile regional environment—India on one side and Afghanistan on the other. A war with India over Kashmir almost as soon as the nation was born stressed the seriousness of the defence threat.

What was the overall result? Lacking a well-organized political party and faced with centrifugal internal forces and external threats, Pakistani leaders—even the best of them—concentrated on state building, not party building. The state had to survive and be coherent first; party building was a long-run task. Overpowering short-run imperatives led to the eclipse of elected institutions and the rise of state institutions. Once in place and bolstered by powerful international allies like the United States, (which found in Pakistan a Cold War ally) the military–bureaucratic coalition developed structural hegemony in the system.

Painstakingly researched and elegantly written, Jalal's book adds to our knowledge. Her analysis of how ill-equipped the state was in its childhood forces us to qualify substantially the conventional wisdom that the historical inheritance of India and Pakistan was identical. Only in the ideational sense was the British legacy similar. Institutional legacies had important differences.

Still, one is tempted to ask whether *both* party building

and state building could not have been undertaken at the same time by Pakistan's founding elite. Comparative literature on democratization suggests that leaders in their moments of popular legitimacy (the "honeymoon period") can act on their visions and undertake difficult initiatives. After independence, leaders of national movements enjoy enormous legitimacy, which gives them room for maneuver: they are not entirely prisoners of their circumstances. Were Jinnah and his colleagues party builders at all? How important was party building in their view of Pakistan? Jinnah's politics suggests that he was a brilliant constitutionalist but not a mass politician or a party builder. Even before independence when times were less unkind, Jinnah did not organize the masses like Gandhi and Nehru did. Had a nationwide Muslim League ever been built, there would have been no organizational vacuum in postpartition Pakistan in the first place. In addition, right after the breakup of the country in 1971, political leaders had yet another opportunity to build a party, since the defeat in Bangladesh had devastated the military.

Jalal gives us a powerful account of how the politics was, rather than how it might have been different. To be sure, it is not easy to deal with counterfactuals rigorously; but if alternative routes were possible, the burden of explanation would have to shift from structural difficulties to a fault of vision. It may well be that Pakistan's political future was "insufficiently imagined."

Harvard University ASHUTOSH VARSHNEY

The Dialectic of Change. By Boris Kagarlitsky. New York: Verso, 1990. 393p. $69.00 cloth, $22.95 paper.

The merit of this book is that it considers the current strategy and theory of socialism in a comparative and interdependent perspective: "*The triumph of the Left in the West is inconceivable without changes in the developing countries and in Eastern Europe* and the political struggle which is happening there is subject to the same general dialectic of reform and revolution" (p. 151; emphasis original). The author, currently executive committee member of the Russian Socialist party, seeks to reconstruct Marxism; and the major theme of the book is that reform is now universally the appropriate tactic for the socialist Left.

This viewpoint is substantiated with reference to Western Europe (particularly Britain), Eastern Europe (specifically Poland), and the USSR. The book is a collection of essays around a general theme. It is well referenced to literature in several languages and the argument is buttressed with pertinent examples; however, it is repetitive and rambling in places. The "dialectic of reform" searches for a social base to underpin the various revolutionary and reformist wings of contemporary workers' and socialist movements. The author condemns the politics of the "old working class traditionalists" in Britain (clustered in the trade unions and the Left of the Labour party), on the Continent (the Eurostalinists in various unreformed Communist parties), and in the East (the Brezhnev wing of the Communist party if the Soviet Union).

Kagarlitsky condemns their revolutionary stance and rebukes their leftist anti-reformism as a protest against technical progress, precluding political change and entailing retrogressive economic protectionism and autarky. This is a somewhat uncharitable representation of the Left: for instance, the British Labour party opposition to membership in the European Economic Community was not reactionary but was based on the consideration that socialist political policy would be inhibited by an enlargement and strengthening of the economic market and would minimize change through political control of the state. Students of British politics will find Kagarlitsky's argument interesting but faulty. There is little discussion, for instance, of the growing influence of feminist and ethnic politics on the Left. The more radical attempts by the Left to use local government organs to secure socialist policies (such as Ken Livingstone's leadership of the Greater London Council) are also ignored. While advocating socialist reformism through the "new working class," Kagarlitsky ignores the major structural change and dealignment in British politics articulated in the social democratic movement led by Williams, Rodgers, Jenkins and Owen. Such social democracy, like the Swedish kind, does not fit with the author's political viewpoint but must be considered to be one possible outcome of the structural analysis advocated here.

In reviewing changes in the Soviet Union, Kagarlitsky takes the side of Western Sovietologists such as Hough, Lewin, and Ruble (though they are not mentioned by name) in focusing on the social consequences for politics of the changes in class structure that took place under Brezhnev. Unlike the majority of Soviet reformers, the author rejects totalitarianism to describe Soviet politics and adopts an interest group approach. The Brezhnev period, he concludes, was "undoubtedly one of the most prosperous and successful periods in Soviet history" (p. 339). The conservative basis of Brezhnev's politics was an alliance of the old working class and the entrenched bureaucracy, whereas the reformers had a social base among the young, skilled manual workers and qualified non-manual employees. This political cleavage provides the social dialectic of change in the Soviet Union. The chapters on contemporary Soviet politics are well referenced, informative, and make useful reading for upper-level classes on Soviet government courses.

What, then, is Kagarlitsky's vision of the contemporary socialist project? He insists that socialism cannot exist without political democracy, workers' self-management of production, nationalization and planning, and market relations in the economy (p. 14). In discussing the inadequacy of the Polish leadership of Solidarity, the author revives interest in the notion of a "political vanguard": it is "impossible to do completely without a strategic vanguard" (p. 207). The discussion, however, is insufficiently theorized and inadequate as policy. How are nationalization and planning to be reconciled with workers' management? And what form does the latter take? Kagarlitsky warns against the "fetishism of the market." But where does fetishism depart from consumer need fulfillment? For a self-styled Marxist analysis, there is, surprisingly, no discussion of the market as a system of political power. How does the "socialist market" overcome the private appropriation of surplus value? And if consumers' preferences are to direct investment through the market, what is the political capability of socialist planning? There is no discussion of why state ownership in the form of nationalization is preferable in allocation and efficiency terms to private ownership.

Kagarlitsky's book is interesting and will provoke

discussion in circles concerned with reconstructing Marxism. What remains unclear is just how much, if anything, of the theoretical underpinning of classical Marxism theory of value, classes, revolution is to be retained and how it informs contemporary socialist strategy. Most, if not all, of Kagarlitsky's conclusions would be shared in varying degrees by many non-Marxists and even anti-Marxists (e.g., C. A. R. Crosland, who is not mentioned in the book). Though there is little new here for the specialist on the societies described, Kagarlitsky's comparative approach and sociological insight have novel features that single out his book as one of the best on contemporary European society to have emanated from the contemporary Soviet Union.

University of Cambridge DAVID LANE

Everyday Politics in the Philippines: Class and Status Relations in a Central Luzon Village. By Benedict J. Tria Kerkvliet. Berkeley: University of California Press, 1990. 326p. $45.00 cloth, $13.95 paper.

Until not so long ago, the literature on peasant politics tended to focus either on bold, mass rebellion or on meek, acquiescent subordination. The new "everyday forms of resistance" literature transcends that dichotomy, broadening and deepening our understanding of political behavior. *Everyday Politics in the Philippines* is an excellent complement to James Scott's now classic *Weapons of the Weak* (1985). The two also coedited *Everyday Forms of Peasant Resistance in Southeast Asia* (1986).

Kerkvliet's *Everyday Politics in the Philippines* is of significant interest far beyond an area studies audience, since it addresses fundamental questions about how subordinated people understand the power relations in which they are enmeshed. Political values and preferences cannot be assumed or imputed from "public" actions or socioeconomic position alone. Kerkvliet brilliantly demonstrates that ideas about justice matter a great deal to those on the brink of survival. Based on 17 years of intimate ethnographic village study, he shows how rural workers and peasants act politically based on strongly held normative values, though always constrained by their available resources, as well as perceived risks and opportunities.

The conventional approach to rural politics in the Philippines highlights vertical patron–client relations and political factions, complicated by religion and kinship. These institutions are often associated with a harmonious political culture, celebrated by ritualistic electoral "fiestas." This certainly captures part of the picture: the rural poor do seek out patrons, and elections in the Philippines are rarely the arena for class- and issue-based contestation. Yet there are several problems with this approach. It cannot account for repeated waves of mass, class-based political mobilization, sometimes nonviolent, sometimes armed. Moreover, the "political faction" approach assumes that when rural workers and peasants are not in open rebellion, they are therefore acquiescing to sharp class and status differences. This study shows, in contrast, that the rich clearly do not have ideological hegemony.

For Kerkvliet, "politics consists of the debates, conflicts, decisions, and cooperation among individuals, groups, and organizations regarding the control, allocation and use of resources and the values and ideas underlying those activities" (p. 11). For most of the villagers during the "war era" (1941–52), this meant active support for the anti-Japanese resistance and later the Huk rebels. (See Kerkvliet's definitive *Huk Rebellion*, 1977.) The government responded with tenancy reforms that the villagers considered meaningful; but, since then the increased capitalization of agriculture sharply aggravated landlessness. Most poor people feel worse off, and they certainly do not blame themselves. This does not mean that they have turned to overt opposition to the government, however. Kerkvliet reports that the armed revolutionary movement that swept much of the upland countryside in the late 1970s and early 1980s has not made much headway in this central lowland region.

In everyday village politics, the political discourse of the majority strongly upholds rights to both security and dignity (although in practice they may conflict). Among the employers, landlords, moneylenders, and traders who most directly shape the terms of the majority's existence, some are seen as more unfair than others. Apparently small differences turn out to matter a great deal, such as the quality of the midday snack employers provide to rice transplanters. (Each employer is expected to provide according to their ability, but the poorest turn out to be the most generous.) Elites who treat people with more dignity are sought out as patrons, while those who are disrespectful provoke reciprocal treatment: pilfering of harvests, stealing of tools, rocks thrown in the middle of the night. Most everyday resistance is angrily spontaneous, but not all is individual. Mass collective action is chosen when people unite around common grievances that seem "winnable" and the government is seen as relatively unlikely to intervene.

The book convincingly argues that for most people most of the time politics is informal and stays within the village. Yet we also learn that some villagers repeatedly participate in regional and national movements to increase peasant representation and accountability in the political system as a whole, usually through nonviolent mass movements—first in the 1930s, then in 1946, and again in the late 1960s. In spite of martial law, the Association of Central Luzon Farmers emerged in 1981 to lead smallholder and landless movements in the region. The organization is clearly very important to villagers (pp. 141–43), but we learn relatively little about how it relates to the community. Kerkvliet readily acknowledges that villagers see local leaders and larger organizational allies as quite important but does not focus on their dynamics. Where are the "everyday politics of organization building?" To move beyond everyday resistance to understand the limits and possibilities for the broader democratization of the countryside, we will need to learn much more about how the organizations of the rural poor manage to survive and grow in such inhospitable environments.

Massachusetts Institute of Technology JONATHAN FOX

Multiparty Government: The Politics of Coalition in Europe. By Michael Laver and Norman Schofield. Oxford: Oxford University Press, 1990. 308p. $59.00.

Michael Laver and Norman Schofield offer us the first comprehensive survey of coalition government in nearly two decades. In their preface, the authors claim

that their most important objective is to "impart a real 'feel' for what is involved in the fascinating process of coalition bargaining" (p. vii). To say that they have succeeded would be an understatement. Laver and Schofield's book is at the same time accessible, pithy, down-to-earth, insightful, and, yes, even captivating. In its scope and quality, it has no rival in the literature on party coalitions.

The authors distinguish between two traditions in the study of coalition politics: the European politics tradition and game theory. The first label appears to represent mainly cross-national analyses of a conventional institutionalist bent. Somewhat surprisingly, this is the context in which Laver and Schofield place their own book. Yet their main concern is to help bridge the gulf that has developed between the two traditions.

In their first substantive chapter, Laver and Schofield discuss the conventional conception of political parties as unitary actors in coalition bargaining. They proceed to examine, successively, party objectives, the process by which governments are formed and the requirements they must meet, coalition membership, cabinet stability, coalition payoffs, and, finally, the affects of institutional and other constraints on coalition bargaining.

Each chapter provides a crisp, fair, and comprehensive survey of the literature in its field. In addition, some chapters present new data or launch novel theoretical departures. All chapters are broad-ranging and well written, but some are more innovative than others. Thus, the conception of parties as unitary actors comes under much more probing examination here than anywhere else in the literature; the process of cabinet formation and investiture is laid out in unusual institutional detail; multidimensional models of coalition bargaining are discussed at length and confronted with new data; and structural constraints on coalition bargaining are for the first time discussed systematically. In each of these areas, Laver and Schofield's book makes a significant contribution. Other chapters, such as the discussion of cabinet stability, are less original and therefore also less interesting to the initiated reader.

Among the greatest virtues of Laver and Schofield's book are the clarity of its arguments and the elegance of its prose. No reader will feel intimidated by the authors' conceptual apparatus or notation. And Laver and Schofield's prose is refreshingly irreverent and down-to-earth. Consider, for example, the following discussion of the lure of different cabinet portfolios:

> "The Prime Minister, after all, tends to get far more of the limelight, to consume far more dinners in interesting and exotic places and to be driven around in the back seats of bigger cars than other members of the government. . . . Other government posts may have their own attractions. The Minister for Foreign Affairs, for example, gets to travel widely, to gather quite a lot of the limelight and to risk less of the odium that attaches, say, to a Minister for Social Welfare forced to cut old age pensions in a budget crisis" (p. 40).

The research frontiers of coalition studies are being moved rapidly these days. As a vehicle for readers who wish to reach those frontiers with speed and in comfort, Laver and Schofield's book has no competition. Yet in some areas the authors could only begin to address issues that may transform the study of party coalitions. The effects of intraparty politics and institutional structure will doubtless receive much more intensive scrutiny in the years ahead, not least from adherents of the "new institutionalism." The assumption that political parties are driven by policy concerns, that is by the pursuit of largely public goods, will ultimately need more rigorous defense. In the meantime, all students of political parties or legislative behavior should allow themselves the privilege of being educated and entertained by Laver and Schofield's fine book.

University of California, San Diego KAARE STROM

Hawke's Law: The Politics of Mining and Aboriginal Land Rights in Australia. By Ronald T. Libby. University Park: Penn State Press, 1989. 208p. $16.95 paper.

Libby's account of the rise and fall of national legislation to expand aboriginal land rights and control of mining in Australia during the first period of Australian Labor Party (ALP) government under Prime Minister Bob Hawke (1983–87) contrasts with dependency theory and "privileged position" interest group explanations of the mining industry's influence on Australian public policy. In his view, these explanations fail to describe what actually transpired. The withdrawals of national land rights legislation and the mining industry's influence in that withdrawal were conditioned (in places he writes "determined") by factional politics within the ALP.

Libby's book is strongest when he takes on prevailing explanations attributing Hawke's decision to drop national land rights legislation to the mining industry's economic strength, without regard to its strategy for fighting the legislation or the political context in which it was pursued. He demonstrates that once the ALP came to power, the relatively resourceless aboriginals became insiders in the process of drafting the legislation, while the mining industry was not even consulted. The mining industry fought the legislation with classic outsider tactics, mobilizing public opinion (already disposed against additional concessions to aboriginals) through a publicity campaign in the state that would be most affected, Western Australia. Followers of water battles in the U.S. West may find intriguing parallels with the contests between irrigated agriculture on the one hand and groups pressing environmental concerns and Indian water rights claims on the other. For those interested in policy formation, Libby's descriptions of the legislation-drafting process and the Western Australia miners' campaign are interesting and useful, illustrating how the political context affects the strategies political actors select and their prospects of success.

Libby's documentation of that political context is another strength of the book. Premier Brian Burke's Labor government in Western Australia rested on shaky electoral ground: Labor had succeeded the Liberal party in state government by winning seats in the mining and pastoral districts that normally vote Liberal and whose voters strongly opposed land rights legislation. Labor seats from Western Australia, in turn, provided the crucial margin in the national parliament; Labor losses in Western Australia could topple Hawke's government. Burke led an ALP faction that revolted against the national party's platform in favor of land rights legislation that would relinquish important aspects of Crown control of mining to the aboriginals. Hawke responded by supporting Burke's position, even though the plat-

form had been adopted unanimously at the biennial national conference just three months earlier.

Perhaps ironically, the information Libby provides about the political context weakens the support for his conclusion that ALP factional politics determined the mining industry's influence, rather than the other way around. His conclusion reflects his choice about what to regard as *context* and what to regard as *action*, but his choice is not always persuasive. He contends that the Burke–initiated factional split within the ALP was the context within which the mining industry acted; but there is at least as much support in the book for the view that mining's importance in Western Australia, the recognition of that importance by voters, and the industry's campaign to publicize voter support for the mining industry and opposition to land rights legislation constituted the context within which Burke decided to reject the national party platform on aboriginal land rights.

Libby diminishes the mining industry's role in Burke's decision by showing that public opinion already opposed expanding aboriginal rights and that the industry's advertising campaign strengthened that opposition only marginally (p. 82). However, the same poll data show that 87% of Western Australians recognized favorably the mining industry's contribution to the state's economy (a year after the conclusion of the industry's campaign to promote itself as the "backbone of the nation") and other poll data reveal that the strongest opposition to land rights legislation was in the mining districts Burke was in greatest danger of losing (p. 93). Libby points out that the Liberal party mounted a vigorous challenge to Burke during the period and concludes from this that the mining industry's role was smaller, but notes elsewhere that the mining industry had historically been aligned with the Liberal party. Burke threw the party platform overboard, brought the mining industry into the process of drafting land rights legislation for Western Australia that was more to the industry's liking, defused the land rights issue for the Liberal party, and got himself and Labor reelected a year later. Libby is persuaded by all of this that the mining industry's campaign was less successful and its influence less strong than commonly believed. Readers may not be quite as persuaded.

Another conclusion seems inadequately supported by the evidence Libby presents. Although it dropped national land rights legislation in 1986, the Hawke government failed to go farther and remove the aboriginal veto over mining from the 1976 land rights law governing the Northern Territory. Libby describes this as a "setback" for the miners (p. 153). In 1984 the industry faced a Labor government unanimously in support of national aboriginal land rights legislation. In February 1986 miners emerged no worse-off than they had been before Labor was elected; the status quo ante had been restored. The fact that the ALP did not change existing law to improve the situation of an industry that historically supported its opposition might not be appropriately characterized as a setback.

Students of comparative federalism will find this book intriguing. Although it is beyond Libby's purpose to do so, the fight over land rights legislation could be fashioned into an interesting story in intergovernmental relations as well. Objections to national legislation had a states' rights dimension that the Liberal party employed to gain support in Western Australia and others denounced as having racist undertones. Something about that seems familiar.

Indiana University, Indianapolis WILLIAM BLOMQUIST

Social Justice for Women: The International Labor Organization and Women. By Carol Riegelman Lubin and Anne Winslow. Durham: Duke University Press, 1990. 328p. $45.00.

Women, International Development, and Politics: The Bureaucratic Mire. Edited by Kathleen Staudt. Philadelphia: Temple University Press, 1990. 320p. $39.95.

Bureaucracies have remained paramount to how societies function, and their behavior frequently reveals contradictions within the societies themselves. It is no surprise that as women in advanced capitalist societies have joined the ranks of bureaucracies, the "bureaucratic mire" in which they find themselves has a particular substance and character. Lubin and Winslow and 15 contributors to Staudt's edited volume wade through this mire to examine how bureaucracies have treated women who are part of their organizations. Contributors to the Staudt volume evaluate the success of bureaucracies in designing and implementing development projects targeted for women; Lubin and Winslow trace the struggles to enact international labor laws for women. Both volumes explore the effectiveness of women's politics within and outside bureaucracies in relation to the development of programs and policies that address women's needs, the institutional and political contexts in which international and national bureaucratic agencies operate, and the socioeconomic dynamics of the agencies themselves.

With rare exceptions, the conclusions reached are the same: women have been able to make few inroads within or outside bureaucracies in bringing about changes that would benefit women. The reasons for this "grim scenario" (as Staudt herself describes it, p. 8) are not surprising. Existing patriarchal structures and attitudes prevent women from being taken seriously within agencies. Few women, therefore, reach positions of decisionmaking in which they can formulate or implement projects targeting the improvement of women's status. Women often internalize patriarchal attitudes and behaviors. Those that do are more likely to achieve more powerful positions within bureaucratic agencies. They often attach little importance to precisely the policies that women resisting patriarchal structures would like to see implemented. Occasionally, men committed to incorporating women into more autonomous positions of policy design and project implementation come to occupy positions of great power within bureaucracies but are eventually ousted by board members or governmental authorities threatened by their radical stances and the slow success rate of development projects for women.

The slow success rate of development projects for women can be attributed to a number of factors: bureaucracies or societies within recipient countries have their own patriarchal ax to grind; class differences among women exist within host and recipient countries so that women's programs often exclude most women or confine them to traditional domestic roles; so few funds are channeled toward these projects that they are likely to be doomed to failure; and an essential conflict exists

within host and recipient countries about how monies can be used best. Most policymakers believe that the dire circumstances of many countries make it more important to channel funds into general development projects that could affect greater numbers of people. On the other hand, those concerned with incorporating women into development projects point out that general development projects frequently fail because women have not been taken into account and that women will never be able to transform their subordinate position unless projects are geared to enhance their self-esteem and economic and political power. Hygiene, nutrition, marketing, and agriculture are but a few areas in which women play central roles but are often completely ignored in the design and implementation of policy.

Women shoulder greater economic responsibility throughout the world due to the rising rate of rural migration to urban areas, including cities in other countries, and the shrinking base of agricultural subsistence. Nevertheless, the authors' focus on the bureaucratic nexus as the place to examine issues is far too narrow. The double jeopardy of trying to design new policies within Western-style bureaucracies and then implementing them in multiethnic recipient countries whose bureaucracies and cultures may be vastly different in their structure and operation has always stymied the successful implementation of projects and the effectiveness of international laws. No doubt women experience greater subordination and frustrations within bureaucracies due to existing gender relations. However, bureaucracies themselves are responsible for the failure of many of the projects described. Innovative ideas are rarely accepted or welcomed by bureaucracies. The kinds of conflicts these authors describe are typical of any bureaucracy and can involve men as much as women. Occupying middle management positions means occupying contradictory locations of great tension in which implementation of normative policy is permissible, but provides little control or power to introduce new ideas or enforce one's point of view. Bureaucracies have never been autonomous, although they may be pervasive and dominate public life. Bureaucratic policies are frequently linked to prevailing political ideologies and economic policies (particularly those of more powerful governments) and the demands of national or international capital.

In the Staudt volume, Cathy Small's article is exceptional in the thoughtfulness and subtlety of its analysis. Writing on the development of the Moving Forward Organization, a women's self-help organization that redistributed village wealth into the hands of women in the Kingdom of Tonga, South Pacific, she concludes that the success of the project, administered by a bureaucracy largely in the hands of women, was due to the nature of existing indigenous social structures and processes and that the role of the bureaucracy was comparatively irrelevant. Women have always occupied a special place in Tongan kinship hierarchies. Their status permitted them to extract economic obligations including property from men, activate marriage alliances, and played a determinant role in "influencing power and claims to titles" (p. 270), including that of chiefs: "Because of these often complex social dynamics, not only women, but all Tongans had a real investment in the superior status of women as sisters" (p. 270). Small stresses that, "In the end, it is these historical conditions and indigenous dynamics—rather than the structure or content of the development enterprise—that underlie the popularity, the character, and the success" of the Moving Forward Organization (p. 279).

Small's article leads one to question the notion of development itself, as does Kathy E. Ferguson in "Women, Feminism, and Development." Ferguson summarizes my objections to the major currents that run through the Staudt volume. It is a credit to Staudt that she includes Ferguson's perspective. Ferguson points out that tremendous tension exists among the contributors to the volume between concern with the equitable distribution of the goods of development and with development programs' often acting to the disadvantage of women and concerns about development itself—who benefits from it and who is excluded, silenced, or controlled. In the former instance, an overwhelming degree of attention is paid to development agencies as primary actors, and to development as "something that the aid bureaucracies *do* to the local people" (p. 299). Ferguson recommends a need to shift control to local communities and to question the kind of control exerted by the development agencies and process. She concludes that although local communities are not virtuous and have their own patriarchal biases, "moral and political priority in making decisions about development should reside with those whose lives are most directly affected by these decisions" and that "self-empowerment of local people" should be a primary goal (p. 299). In these terms, development would take on a more politicized character in which attention would focus not so much on aid agencies but, rather, on the goals of local peoples, including the indigenous struggles of women. She also squarely confronts the problem of women working in bureaucratic agencies and recommends, in line with her thinking about development itself, that these women should be perhaps more engaged in changing their own social structure, forming alliances outside the bureaucracy, establishing grass-roots links with local constituencies and activists, and questioning their own government's roles in maintaining global inequities.

Lubin, a veteran of the International Labor Organization (ILO), and Winslow trace the history of women's participation in British and North American trade union movements and their roles in the founding and development of the ILO. The book dramatically contrasts the efforts made, mostly by women, to improve the status of women workers through the use of the ILO, with the relatively few gains that were actually achieved in the interest of women workers. The authors' painstaking research of the history of women's participation in affecting ILO policies is admirable. However, the book lacks a theoretical orientation that would unify these nuggets of information. The task of discovering who these women were was daunting and the book's structure attests to the problem scholars encounter who wish to revise historical accounts to include women who were significant actors. The book will nevertheless primarily serve as a reference. We are left only with blow-by-blow accounts of debates and a grasp of the issues at stake in each debate. The stark contrast the authors draw between the nature of modern feminism among Western women and traditional machismo among women from non-Western or less-developed countries is far too general and oversimplified, although, unfortunately, it permeates many works on gender relations in society.

Both books find their strength in taking traditional international organizations and policies and reevaluat-

ing them in terms of gender relations. However, both suffer from overemphasizing the category of women and losing sight of the social, institutional, historical, and political context in which these gendered relationships unfold. Staudt's volume includes selections useful for classes on development and issues of gender; Lubin and Winslow's volume will be a valuable resource for labor historians.

University of Wisconsin, Madison LINDA J. SELIGMANN

Social Policy and the Welfare State in Sweden. By Sven E. Olsson. Stockholm: Swedish Institute for Social Research, 1990. 238p.

The Political Theory of Swedish Social Democracy: Through the Welfare State to Socialism. By Tim Tilton. Oxford: Clarendon, 1990. 280p. $39.95.

The Swedish Experiment in Family Politics: The Myrdals and the Interwar Population Crisis. By Allan Carlson. New Brunswick: Transaction, 1990. 235p. $39.95.

Suppose that we could measure the attention that foreign scholars have paid to the domestic arrangements of different countries and that we deflated this measurement by population size: my bet is that Sweden would rank first on such an index. Presumably, it is Sweden's success in reconciling economic efficiency and social equality that accounts for its notoriety. Sluggish productivity growth and widening social inequalities in the last 15 years raise questions about the durability of this historic achievement. Perhaps it has become more difficult for Sweden and other countries to reconcile efficiency and equality? However, these recent additions to the literature on the Swedish experience remain within the traditional problematic of comparative welfare politics. Leaving the conditions of capitalist growth aside, they ask: Where has the impetus behind the development of the welfare state come from? and Why have reformist forces prevailed in Sweden?

While Sven E. Olsson seeks a comprehensive account that synthesizes different arguments advanced by students of comparative welfare politics, Tim Tilton seeks to break new ground by emphasizing the importance of ideas of the Swedish Social Democrats. For Olsson and Tilton alike, the development of the Swedish welfare state is inextricably linked to the political dominance of social democracy. Allan Carlson's more narrowly conceived study of the intellectual origins of Swedish family policy seconds Tilton's emphasis on ideas; but for Carlson, the prime movers are intellectuals who stand outside the ideological mainstream of the Labor movement.

Olsson provides the most careful and comprehensive presentation of the postwar development of the Swedish welfare state available in English (perhaps in any language). The analytical significance of his descriptive analysis is to emphasize the composite and multifaceted character of the Swedish welfare state. Olsson shows that the Swedish welfare state combines selective state intervention in labor markets and means-tested social assistance with universalistic welfare programs, social insurance with socialized consumption, and income maintenance with income redistribution and periodizes its development in terms of the introduction of new elements or changes in the relative importance of different elements.

This complexity leads Olsson to argue for a multivariate explanatory approach, emphasizing the interaction of demographic, economic, and political factors. In key passages, Olsson asserts the central importance of politics. Except for the chapter on the pension reform of 1946, however, his book contains precious little discussion of the politics of policy formulation and legislation. For Olsson, "politics" boils down to the balance of power among class actors (pp. 249–50). Though I share his preoccupation with class power, I would argue that Olsson's approach does not adequately address the "translation" of class power into policy outcomes.

Through a careful and insightful analysis of the writings and speeches of the major intellectual figures of Swedish social democracy, Tilton successfully refutes the unattributed but allegedly common (p. 40) assumption that the Swedish Social Democrats lack a coherent ideological foundation. His case for the political significance of ideas and ideology is less compelling. Tilton argues that ideas serve to guide strategic political action, to enhance political support through ideological appeals, and to generate concrete policy initiatives. Though he suggests that postwar welfare state activities broadly correspond to social democratic ideas, he never engages in a systematic analysis of how, where, and when certain ideas come to be translated into (more or less successful) strategic choices, ideological appeals, or policies.

Tilton asserts that "structural explanations . . . fail to do more than outline the general potential for reformist politics" (p. 9), but he does not specify what it is that structural explanations cannot account for. Furthermore, the inadequacy of structural explanations does not in itself vindicate Tilton's history-of-ideas approach, for there are plenty of other ways to bring human agency into the explanation. Most importantly perhaps, Tilton never addresses how the ideology of Swedish social democracy differs from that of other reformist Labor movements in Western Europe. If all reformist Labor movements adhere to the notion of a "socially controlled market economy," for instance, this notion can hardly be invoked to explain the political success of the Swedish social democrats. There is no question that the Swedish Labor movement has been shown to have a remarkable capacity to generate innovative policy proposals; the question is how this capacity of policy innovation is related to the basic elements of social democratic ideology identified by Tilton.

Carlson's detailed study of the intellectual context and political impact of Alva and Gunnar Myrdal's *Crisis in the Population Question* (1935) effectively probes the role of intellectuals and ideas. Carlson shows how the Myrdals wrested the population issue away from conservative forces (linking the need to reverse the population decline of the previous three decades to neo-Malthusian concepts of voluntary parenthood, sex education, and birth control, as well as the feminist theme of women's full participation in the labor market) and demonstrates how their reformulation of the terms of debate led the leadership of the Social Democratic party to adopt a pro-natalist position. He proceeds to describe the Myrdals' participation in the Population Commission appointed by the government in 1936 and shows how the Commission's policy recommendations conformed to their views. Concluding that the Myrdals "gave decisive direction and shape to the developing Swedish welfare state" (p. 174), Carlson argues that they achieved such influence because of their skillful use of

social-scientific research and, above all, social-scientific jargon.

For all the merits of his approach, Carlson ends up exaggerating the influence of the Myrdals. Certainly, their view that the government should socialize the economic costs of parenthood prevailed from the late 1930s onward; but as Carlson himself notes, the Myrdals argued strongly for in-kind benefits (clothing, foodstuffs, housing, etc.) to ensure that state aid would improve the conditions of children—and on this score, their success was at best partial. The introduction of family allowances and the other major reforms of the 1940s represented a victory for the cash benefits line advocated by Gustav Möller, the minister of social affairs, in opposition to the paternalism that the Myrdals shared with the conservative approach to welfare policy (see Bo Rothstein, "Managing the Welfare State," *Scandinavian Political Studies*, 1987). As for women's employment and public day care, they did not become prominent features of the Swedish welfare state until the late 1960s. Family- and gender-related policy initiatives in the 1960s and 1970s might perhaps be seen as the final realization of the program that the Myrdals formulated in 1933–39, as Carlson suggests (pp. 200–202), but this delay suggests that Carlson's assessment of their influence must be qualified by taking into account the rise of the women's movement and the demographic and economic conditions of the new reformism. In other words, the role of intellectuals and ideas in the development of the welfare state must be situated within the broader explanatory framework set out by Olsson.

While Tilton and Carlson largely ignore the institutional structures and administrative politics of the welfare state, Olsson introduces this theme through his discussion of recent government efforts to render the public provision of welfare more efficient and more responsive to "consumer demands." Olsson does not articulate very clearly how he conceives the causal significance of institutional structures, however. Also, his treatment of recent developments does not adequately bring out conflicts of interest between public sector employees and their unions on the one hand and taxpayers, welfare consumers, and private sector unions on the other. These conflicts lie at the core of the current difficulties of Swedish social democracy.

Cornell University JONAS PONTUSSON

Insurgency and Terrorism: Inside Modern Revolutionary Warfare. By Bard E. O'Neill. McLean, VA: Brassey's, 1990. 171p. $19.00.

Attempts to devise analytical models for the general study of insurgency have invariably proved unsatisfactory, chiefly because it is such a multifaceted phenomenon, the combination of whose elements can vary greatly from case to case. A rather more modest enterprise has been undertaken by Bard O'Neill, namely, that of offering a framework setting out the main areas of analysis that must be included in any reasonably comprehensive study of insurgency. This framework is useful both for the individual case study and for comparative analysis.

So much of a partisan nature has been written about this subject that one must first commend the author for his admirable efforts at objectivity. Apart from one reference to "guerrilla-infested zones" (p. 130), O'Neill's approach is dispassionate and scholarly; he avoids the temptation to sensationalize his subject by unduly inflating the role of terrorism in insurgencies and he does not over-liberally attach the label terrorist to insurgents.

After a superficial survey of post-1945 insurgencies (in which East–West rivalries are arguably attributed too much weight vis-à-vis North–South themes), O'Neill follows with a general analytical framework. Readers are presented with seven types of insurgency (anarchist, egalitarian, traditionalist, pluralist, secessionist, reformist, and preservationist) and then introduced to four basic insurgent strategies (conspiracy, protracted popular war, military focus, and urban warfare), the first three of which are based partly on the Russian, Chinese, and Cuban revolutionary experiences. The framework also directs analysis to the environment of insurgency (both physical and human), popular support, organization and unity, external support, and government responses.

This kind of exercise has been done before, at least for terrorism. However, in the most stimulating part of the book, O'Neill goes beyond a simple listing and discussion of factors and types, to discuss the *relationships* between the different factors. For example, he looks at how government responses to insurgency can involve attempts to change its physical environment, as in the case of Hassan II's erection of Saharan sand walls in Morocco's response to Polisario.

Clearly written and succinct, the book will be of considerable value in encouraging university students and government advisers to adopt a more comprehensive approach to the study of insurgency. For scholars already immersed in studies, however, the insights offered by the book will be more limited, and the unevenness of O'Neill's acquaintance with the countries he cites as examples more evident. He knows the Middle East but is extremely unreliable on Latin America. For example, it is alleged that both the Montoneros and the Tupamaros lacked extensive apparatuses (p. 96); yet their building of substantial urban clandestine apparatuses was, in fact, a cause of rapid collapse in each case. It is claimed that the military is of little use against urban guerrillas (p. 128), yet clearly the military, not the police, destroyed urban guerrillas in both Argentina and Uruguay. It would appear that O'Neill has relied occasionally upon case studies prepared by graduate students and government officials (p. 12) and thus perpetuates mistakes in work that has not been subjected to criticism from real area specialists.

There is a weakness, too, in O'Neill's typology of insurgency. In particular, his egalitarian type is too broad, since it fails to discriminate between socialist insurgencies against a domestic establishment and "national liberation" struggles directed principally against foreign opponents. Moreover, in a book lacking a bibliography, some important sources are completely ignored. Guevara's *Guerrilla Warfare* (1961) is cited but there are no references to his subsequent texts revising the *foco* theory. One must also question the judgement of an author who devotes as much space to the distinction between active and passive support for insurgency as he gives to discussing the significance of the political system as an environmental factor.

In places, the analysis is distinctly low-level, and some of the points are embarrassingly bland. For example, in relation to popular support for insurgency, O'Neill writes, "The larger the groups that feel relatively de-

prived, the greater the possibilities for mobilizing mass support and sustaining a widespread insurgency" (p. 74); and we are informed that for insurgents "sanctuaries in adjacent states are preferable to those a considerable distance away" (p. 119).

Yet, as a teaching aid, the author's sustained efforts to be systematic and neutral in his approach to insurgency make this a worthwhile introductory text. Teachers may want to question some of the categories and definitions employed; but it is a strength of O'Neill's framework that it is sufficiently flexible to allow for revision. It will be widely used in universities and military colleges.

University of Warwick RICHARD GILLESPIE

The Social Origins of Democratic Collapse: The First Portuguese Republic in the Global Economy. By Kathleen C. Schwartzman. Lawrence: University of Kansas Press, 1989. 224p. $29.95.

In 1910 a coalition of industrialists, workers, peasants, and liberal politicians overthrew the Portuguese monarchy and established the (first) Portuguese Republic. Throughout the next 16 years no less than 45 cabinets steered the unsteady republic through a succession of crises and coups. Finally, in 1926 the military restored "order," proclaiming the Estado Novo in the name of the church and nation.

Kathleen Schwartzman's purpose in *The Social Origins of Democratic Collapse* is to offer a comprehensive historical explanation for the rise and decline of the Portuguese Republic and, more broadly, to investigate the constraints that the world system places on democracy in a semi–peripheral nation. At the outset, she rejects Howard Wiarda's thesis that the authoritarian corporatism characteristic of the Estado Nova is a more or less permanent feature of the Iberian peninsula. Portugal's democratic interlude, although short–lived, "cannot be treated as an empirical aberration that preceded the authoritarian regime. It has to be taken seriously" (p. 25). Schwartzman also rejects analyses, such as Juan Linz's, that are narrowly restricted to political factors: "The potential for psychological or institutional reductionism and tautological explanations rests . . . on the erroneous assumption that the . . . political arena is autonomous" (p. 9).

Schwartzman ambitiously seeks to bring together two sets of literature that have unfortunately tended to be isolated from each other: (1) literature on the socioeconomic and class bases of dictatorship and democracy and (2) world systems literature. Following Barrington Moore and Nicos Poulantzas, she focuses on relations between agrarian elites, the nascent industrial bourgeoisie, peasants, and industrial workers. Although not entirely ignoring political factors, she emphasizes the structural preconditions for democracy. However, she further argues that Portugal's class structure can only be understood within the context of the world system. Of particular relevance here is Portugal's unique semiperipheral status within the early twentieth-century international division of labor. At this time Portugal was highly dependent on the core states (in important respects itself virtually a colony of Great Britain) yet simultaneously presided over a sizable empire.

The centerpiece of Schwartzman's conceptual framework is Antonio Gramsci's concept of a class project, namely, that stable parliamentary democracy requires the emergence of a hegemonic class faction (either industrialists or, perhaps, in the extreme periphery, rural gentry or landlords) capable of securing the acquiescence of subordinate classes to its own developmental project. Schwartzman reconstructs the various hypothetical possibilities for hegemonic class formation in early twentieth-century Portugal: agro-export, domestic agriculture, import-substituting industrialization, merchant capitalism. She argues that no fraction of the bourgeoisie was able successfully to forge such an alliance within the framework of parliamentary democracy.

The reasons for the inability to establish such an alliance can be found not only in the contradictions among the various segments of the bourgeoisie but also in Portugal's semiperipheral status within the world system. The crisis of global capitalism that culminated in World War I—especially the decline of British imperialism—precipitated a crisis for Portugal's traditional reliance on agro-export and merchant capitalism and its disarticulated economy. The crisis enhanced domestic centrifugal tendencies, thereby reducing the space for political compromise and rendering import-substituting industrialization (the logical next phase for Portugal) economically and politically unworkable. At the same time, the industrial working class was neither large enough nor radicalized enough to make its own revolution.

Ultimately, amid growing domestic and international chaos and the dissatisfaction of all classes and groups (including the trade unions) with the Republican politicians, the military stepped in to impose its own project. Unfortunately, while Schwartzman clearly demonstrates that the prevailing balance of class and international forces rendered a democratic project highly unlikely, she does not extend her analysis beyond the military takeover, although doing so might have provided additional evidence for her claims. What class interests, for example, were served in the Estado Novo?

The world system literature has often been criticized for not paying sufficient attention to national peculiarities. This carefully researched study effectively answers this criticism. While cultural and political factors undoubtedly contributed to democratic instability, Schwartzman's comprehensive analysis clearly shows the substantial relationship between the world political economy, Portugal's class structure, and the demise of the fledgling republic.

Hamilton College ALAN W. CAFRUNY

British Feminism in the Twentieth Century. Edited by Harold L. Smith. Amherst: University of Massachusetts Press, 1990. 214p. $27.50.

Journeys among Women: Feminism in Five Italian Cities. By Judith Adler Hellman. New York: Oxford University Press, 1987. 275p. $13.95 paper.

These two books undertake an examination of aspects of the women's movement that have received little attention from scholars in the field. In *British Feminism in the Twentieth Century,* Harold Smith has collected articles focused primarily on the activities of British feminists during the early 1900s. The contributors to this volume provide new insights into the feminism of the Pankhurst era and guide us through the movement's transformations into the 1950s. Judith Adler Hellman, in her exploration of contemporary feminism in Italian cities,

makes the point that Italian feminists have developed programs that are either responses to, or reflections of, the particular traditions (especially of the Left) peculiar to their cities or regions. In both books the message is clear: we need to examine more fully the particular circumstances of feminist expression in order to understand the nature of feminist movements. Both books' authors underline the significance of time and space variables in the development of women's movement activities.

The Smith volume is, for the most part, a good collection of new work. Sandra Holton, in her examination of the romantic feminism of Emmeline Pankhurst, breathes new life into her discussion of the Pankhurst era by signaling the violent undertones of militant feminism and the potential for political assassinations that was part of the Pankhurst approach. The Pankhursts operated according to the principle that progress results from the actions of heroic individuals and cultivated heroines within their movement. But World War I intervened, and feminist violence was postponed, if not removed.

One form of violence that did emerge in the prewar years was the practice of wife beating. But as Jan Lambertz points out, sexual exploitation and cruelty to children proved to be much stronger rallying points for early British feminists, and the implications of unequal power relations between wives and husbands were not explored in early feminist analysis. This helps to explain the relative demise of feminism in the postwar era. The militancy at the turn of the century gave way to a moderate debate between proponents of equal rights and those arguing for women's special needs. By the 1930s, the force of British feminism had diminished and there was little progress for working women. Harold Smith's own contribution to this volume underlines the limits to legislative changes to improve women's status introduced by successive British governments in the post–World War I period.

The contributors are much less successful in explaining (or even describing) the development of feminism in Britain after the end of World War II. Contributors have explored women's role in the Labour party and Labour's feminist agenda, as well as images of women in the popular press and the work of Alva Myrdal and Viola Klein. But a stronger editorial hand could have integrated these now-scattered scholarly efforts into a more thorough (and needed) examination of the emergence of the contemporary women's movement in Great Britain.

Judith Adler Hellman's work provides us with a more thorough examination of the contemporary women's movement in Italy. Hellman opted for a research methodology that included participant observation of feminists in five Italian cities during 1977 and 1978. Whereas the Smith collection is rich in historical perspectives, Hellman's book opens our eyes to the culture-specific qualities of contemporary feminism and the intricacies of role interactions for Italian feminists.

Hellman explores the historical development and contemporary variations in Italian feminist thought and action, focusing specifically on the impact of leftist traditions in each of the five cities she examines. Her analysis is rich with descriptions of the special characteristics that distinguish the five cities in her study. For example,Turin is characterized by the traditions of a workers' city while Milan reflects the impact of student and New Left movements in the 1960s; and these two major centers, together with Rome, provided the models for feminist development in smaller centers throughout the country. Hellman's argument is that the issues developed and defined by feminists have varied according to location and that this has resulted in the emergence of a broadly defined social movement affecting women differently in different places.

But while Hellman refers in her conclusion to some general "laws of motion" common to social movements, her analysis would have been strengthened by a more consistent linkage of these common laws with her particular descriptions of feminist life in the five Italian cities. The reader is left wondering if the descriptions of differences may mask an equally significant pattern of similarities. Hellman argues that Italian feminism is unique, reflective of the leftist and Catholic subcultures that permeate Italian society. But she restricts her discussion to the Italian scene.

There are some problems with this volume. The author relies too heavily on acronyms throughout and should have provided a list of their references. Her interviews reflect attitudes in the late 1970s; but feminist thinking changed dramatically in the 1980s, particularly with the emergence of an antifeminist backlash. It is not clear why Rome, one of the cities Hellman signals as significant for the development of the movement, has been left out of this analysis. Finally, the development of the analysis for each of the five studies does not follow a common pattern and is thus somewhat hard to follow.

Nevertheless, this work is an important contribution to feminist literature and, considered together with the Harold Smith volume, suggests the need for more comparative analysis of feminist movements across, as well as within, countries. The Smith and Hellman works are not easily compared in their present format. However, the authors share some common perspectives and questions. These two books serve as timely reminders of the need for more rigorous cross-national comparative feminist research in the future.

University of Waterloo SANDRA BURT

Governing the Market: Economic Theory and the Role of Government in East Asian Industrialization. By Robert Wade. Princeton: Princeton University Press, 1990. 438p. $49.50.

Manufacturing Miracles: Paths of Industrialization in Latin America and East Asia. Edited by Gary Gereffi and Donald L. Wyman. Princeton: Princeton University Press, 1990. 416p. $49.50 cloth, $16.95 paper.

The newly industrializing East Asian countries are the focal point for two related debates in comparative political economy. The first concerns the role of state intervention in economic growth. Neoclassical accounts of East Asian growth do not neglect the role of the state; indeed, policy reform is at the forefront of those accounts. Yet the operative policies are the maintenance of macroeconomic stability; the provision of public goods; and trade, exchange rate, and pricing policies that allow firms to exploit comparative advantage. Sophisticated neoclassical approaches admit that the state may effectively intervene but only to allow relative prices of goods and factors to approximate those that would prevail in a situation of free trade. More targeted industrial policies that "distort" prices are seen as inimical to growth for two reasons. Not only do such policies reduce the efficiency of resource allocation, but they also encourage unproductive rent-seeking activity.

Robert Wade's outstanding book on Taiwan builds on previous work by Chalmers Johnson to articulate an alternative *governed market* theory of East Asian success. In contrast to neoclassical accounts, the governed market approach emphasizes the role of government in encouraging both high *levels* of investment and a sectoral *composition* of investment that varies from what would have been supplied by the market.

Wade distinguishes between Industrial policies that "lead" the market and reflect government anticipation of future comparative advantage and policies that "follow" the market by supporting existing activities of firms. The case for the general market approach does not hinge solely on government leadership, since strong state support for firm-level initiatives may also contribute to high growth. Nonetheless, Wade pays particular attention to government initiative, arguing that "government policies deliberately got some prices 'wrong', so as to change the signals to which decentralized market agents responded, and also used non-price means to alter the behavior of market agents" (p. 29).

Unlike Alice Amsden's (1989) important study of Korea, (*Asia's Next Giant*), Wade's work is more cautious in giving the neoclassical account its due. An entire chapter is devoted to detailing the areas in which Taiwan's economic strategy matches the neoclassical account. Yet the core of the book (chaps. 4–6) provides a veritable catalog of state intervention In Taiwan. In addition to outlining the pervasive role of state-owned enterprises and the use of the financial and fiscal system to channel resources, Wade provides a detailed account of the trade regime, the core of the neoclassical account of export-led growth. Though exporters enjoyed a free trade regime for inputs, the domestic market for final products was protected in a variety of ways well into the 1980s and exporters were granted supports that went far beyond a favorable exchange rate.

The neoclassical rejoinder is that these policies did not matter or that they even slowed growth from what it might otherwise have been. Wade's response is to try to shift the onus of argument back onto neoclassical turf through the sheer weight of the empirical evidence in the form of case studies of sectors, careful attention to the timing of policy initiatives, and exhaustive reviews of the instruments of government policy.

Unfortunately, this strategy is unlikely to succeed. Greater attention must be paid to the theoretical foundation of the governed market approach, particularly the link between state intervention and growth. There are interesting such hints throughout Wade's account, including technological externalities, information costs, market failure, spreading risk, and dynamic comparative advantage. But none are consistently developed. The next step is clearly to build on some of the empirical insights provided by Wade, Amsden and others to provide the microeconomic foundations of the governed market approach.

The second theoretical debate surrounding the East Asian newly industrializing countries concerns why they pursued the development strategies they did. This is the central theme of the Gereffi and Wyman collection and is a leitmotif in the Wade volume, as well. This second debate cannot be separated from the question of *what* economic strategies the newly industrializing East Asian countries pursued. Gereffi's introduction and Colin Bradford's contribution both emphasize that the common distinction between Asia's export-led growth and Latin America's import-substituting industrialization should not be overdrawn. Nonetheless, the difference between the two regions in regard to broad industrial policy is the main puzzle the book seeks to address.

The concluding essay in the Gereffi and Wyman volume spells out the explanatory options in a "nested hierarchy" that includes 17 clusters of variables, from "world-system dynamics" to culture and ideologies. The result is a list that fails to discriminate among competing explanations. The more successful essays take some particular cut at the issue. The most theoretically succinct contribution is provided by Gustav Ranis, who argues that the absence of natural resource rents in East Asia made the continuation of import-substituting industrialization infeasible, while the larger Latin American countries could finance its continuation by taxing mineral and agricultural exports.

Three essays get substantial mileage from comparative analysis of differences in industrial structure across countries. Stallings emphasizes the effects of government policy on the structure, conduct, and thus perceived costs of multinational corporations. Gereffi and Fred Deyo use differences in industrial structure as a starting point for comparative discussions of the political role of big business and the popular sector, respectively.

Robert Kaufman and Tun-jen Cheng provide the most nuanced political analyses in the volume; interestingly, both attempt to underline differences within their respective regions. Analyzing the historical experiences of six Latin American countries, Kaufman traces the economic policy choices of the immediate post–World War II period to differences in political responses to the Great Depression. Cheng spins out the economic policy implications of single-party rule in Taiwan and bureaucratic authoritarianism in Korea, emphasizing the more interventionary policy style associated with the latter style of rule.

Robert Wade also places strong emphasis on the political foundations of Taiwan's economic development. He rightly notes the significance of authoritarian political structures and cohesive, insulated decision-making structures but mistakenly chooses to characterize the political system as "corporatist." This characterization fits Taiwan poorly and is, in fact, contradicted by the thrust of Wade's account. Wade concludes that Taiwan differs from Japan precisely in the weakly developed political linkages between the state and the private sector—an interesting puzzle for a model that stresses the "governance" of the market.

Wade's analysis of the policy regime is more developed than his analysis of policy choice. He appears reluctant to provide any theory of policy. Yet there are numerous interesting hints here, particularly about how the strength of the party and the relative weakness of business allowed for a policy equilibrium that provided economic opportunities for business but limited the chances either for rent-seeking or for speculative economic activities.

Wade has written an important book. It Is the best book to date on any of the newly industrializing East Asian countries and should be read by anyone interested in the comparative politics of growth. The Gereffi and Wyman collection is necessarily less focused but provides a solid example of the gains to be had from cross–regional comparisons.

Harvard University STEPHAN HAGGARD

INTERNATIONAL RELATIONS

Eunomia: New Order for a New World. By Philip Allott. New York: Oxford University Press, 1990. 427pp. $27.95.

Philip Allott is ambitious in his goal for *Eunomia* (Greek, "good self-ordering of society"), hoping that it will "find a place in the tradition of the most general works of social theory of the past" (p. xxii). The book deserves to be taken seriously by intellectuals of many bents, for it cannot easily be pegged as a work of international politics nor even of political philosophy in any ordinary sense. It may influence generations of intellectuals who must grapple with our increasingly crowded playing field of life-threatening issues that are both the product of, and exacerbated by, humanity's outmoded political order.

The author was for many years a British diplomat; and his wide-ranging and imaginative interpretation of what's wrong with what we are forced to call international politics (forced, as the author reminds us, because of the poverty of the language that has emerged from our experience) was inspired by his conclusion that "British diplomacy had for centuries played a leading part in making a world-system whose peculiar rationality could also be seen as a form of madness" (p. xii). It is Allott's intent to make the reader understand the full extent of the madness and to demonstrate that to understand it thoroughly is to become empowered to overcome it.

Yet to put it that way in a sentence is to reduce the richness of the learning Allott brings to his creative work—from linguistics, philosophy, psychology, sociology, and law. This work continually articulates in the most startling and original ways what many of us may recognize as obvious when we see it stated thus. Then we note that the obvious is generally clouded except for the inchoate flashes of insight that we who work as scholars in this field may sometimes have into the truly crazy way in which the people of the earth have organized themselves for their global interactions. We usually suppress those insights, supposing them to be irrelevant—or, worse, unrealistic—flights of fancy distracting us from analysis of a still-brutish state of nature largely unredeemable at human hands.

Not so Philip Allott, who has spent 15 years painstakingly developing the ideas (and, one suspects, polishing the gleaming, frequently poetic prose) that have gone into this treatise. He had concluded during his time at the Foreign Office "that the old international order was neither a natural phenomenon to which humanity had simply adjusted its behavior nor a fortuitous aggregation of countless past events of human interaction. . . . The international system itself is nothing other than a structure of ideas; and it has been made nowhere else than in the human mind. The international order forms the minds of those who make the international order. The masters of the world of tomorrow are the slaves of yesterday's ideas" (pp. xv–xvi).

This conception of the problem has led him to begin his analysis with words themselves as they relate to thought about society, to reason, to human will, to power, to the search for order, and more. Then follows analysis of the constitution of society, only toward the end of which Allott begins to consider modern international society. The last section treats human well-being in the context of international order, concluding by depicting what humanity might become in the near future. The approach is frequently dialectical; the work is complex, original, protean, ironic. It is an example in the world of scholarship of the "preposterous idealism" Vaclav Havel would bring to political life (not to demean as other worldly and irrelevant to the "real" world of politics a work organically rooted in a solid understanding of humanity's psychological and social reality).

Familiar terms to the international relationist (e.g., *sovereignty, international organization, international law, deterrence*) take on new and multifaceted meanings as their place in the enslavement of both our minds and our ability to determine our planetary future is revealed. The end result is not a policy or a formula but merely a revelation of some of what we are capable of becoming.

On our atrophied social development beyond the state, Allott writes: "The self-conceiving of international society in the modern period . . . is a strange, not to say a tragic, story . . . which can be told in a single sentence. Tempted for a while to conceive of itself as a society, international society chose instead to regard itself as the state externalized, undemocratized, and unsocialized" (p. 240). On the nuclear threat, he writes: "A society which conceives its survival as depending on threats of self-destruction is a society which misconceives its nature as a society. A society which conceives its own self-destruction among the possibilities which it might choose is a society which misconceives its potentiality as a society" (pp. 252–53). These do no more than hint at the richness of the analysis. The book's full impact can only come by working one's way organically, as Allott has done, from "the word" in the beginning right up to "well-being" at the end.

Temple University LYNN H. MILLER

Elements of International Political Theory. By Michael Donelan. New York: Oxford Unviersity Press, 1990. 212p. $59.00.

Readers of this book may at first be puzzled or even irritated by its idiosyncracies. Instead of an argument, the author, who has taught international relations at the London School of Economics for many years, offers a dialogue between representatives of five major traditions: natural law, realism, fideism, rationalism, and historicism. This dialogue draws on the writings of philosophers and statesmen representing each tradition. The writing is old-fashioned and literary. A book so contrary to the objective style of contemporary political science would seem to have little chance of attracting the attention of international relations specialists, at least in this country.

This would be unfortunate, for several reasons. Donelan's book carries on, in a masterful way, the style of British international theory taught by Martin Wight and Hedley Bull. With its emphasis on history, morality, and

philosophy, this kind of scholarship provides an alternative to the policy-oriented empiricism of U.S. social science. The latter has its virtues; but a sense of history, moral subtlety, and philosophical depth are not among them. Donelan has thought long and hard about his subject and can offer unexpected and genuinely illuminating insights into it. For example, he includes as one of the five major traditions fideism, a moral theology that divides the world into good and evil and articulates foreign policies of conquest or conversion based on this premise. The crusading spirit of the fideist is quite distinct from the compassion characteristic of natural law, or from the realist's prudent egoism.

Equally interesting is the author's extended exploration of historicism, another perspective that seldom appears in typologies of international thought. Against the dogmatic foundationalism of the other schools, the historicist insists on the particularity of moral and political experience. The historicist rejects the realist's mechanistic conception of politics, the natural lawyer's essentially static view of human nature and society, and the theoretical abstractions of the rationalist. Because what is happening now cannot be understood without knowing what happened before, real knowledge of international relations is historical knowledge. For the historicist, human affairs are best understood as a story.

Donelan's discussion of historicism usefully clarifies a confusion in current debates about "communitarian" theories of politics and international relations. We must, he argues, distinguish between the nationalist, who naviely thinks his own culture best, and the theorist of nationalism, who celebrates the idea of the national state rather than the actual culture of a particular nation. While the nationalist yearns for the independence of his nation, the theorist advocates national self-determination as a general principle of world order. But this ideology is itself a kind of nationalism: a projection on to the world stage of the values of a liberal culture.

As these sketches of fideism and historicism suggest, there are more voices in the great debate on international relations than those implied by the familiar categories *realism* and *idealism*. An enterprise like Donelan's raises four obvious questions, however. First, how should a perspective or tradition be defined? Is it, as the historicist might argue, a complicated and continuing story that cannot really be captured in the categories of a typology? Or is it an intellectual construct given boundaries by its own internal logic? Donelan seems not to have made up his mind on this question, which is indeed a hard one. Second, which are the really important traditions, and how many are there? Without criteria of importance and difference, these questions are unanswerable. Practically speaking, however, Donelan's fivefold typology is more useful than the realist/idealist dichotomy or the dominant British trilogy (realism, rationalism, and revolution) invented by Martin Wight.

Third, how skillfully is each tradition portrayed? Readers will inevitably disagree on the hits and misses here. I found the portraits of fideism and historicism more illuminating and exciting than those of realism and natural law, but this may say more about my own understanding than about the author's. I have the most doubts about rationalism, which, as Donelan constructs it, seems less an independent view than a composite of elements drawn from realism (the premise that mankind is permanently divided into separate states), natural law (that the relations among these states are governed by rules), and historicism (that these rules are based on the customs and agreements of the states themselves).

Finally, the reader will want to know which perspective is best or, at least, where the author himself stands. Donelan identifies himself as a rationalist, but he also provides a genuine dialogue that invites the reader to judge who has made the best case. It is precisely this refusal to push his own position that draws one into the conversation as a participant, not just an observer.

University of Wisconsin, Milwaukee TERRY NARDIN

The Power of Legitimacy among Nations. By Thomas M. Franck. New York: Oxford University Press, 1990. 303p. $32.50.

In the international system, rules are generally not enforced; yet they are mostly obeyed. International law, despite being primitive and decentralized, still generates habitual compliance with a vast array of sophisticated international legal rules, so much so that compliance often passes unnoticed and taken for granted. But why do powerful states obey powerless rules? What explains this incentive for states to comply with international precepts and standards? For Thomas Franck of the New York University School of Law, the answer resides in the power of legitimacy.

According to Franck, legitimacy embraces that quality of a rule or rulemaking institution that exerts normative influence toward compliance with its intended audience, largely because it functions in accord with generally accepted principles of "right process." To be sure, governments of states are willful creatures, capable of formulating and coexisting in accordance with community norms and standards. But the law governing the international community must express the general will of those governments, and here is where legitimacy comes into play.

Franck's argument is premised on the thesis that international law is not "law" in the same sense that it applies domestically. Though different from its municipal counterpart, the law of nations still retains great significance. Franck views international law as somewhat analogous to the house rules of a club, and it is the desire to be an accepted member of that club that motivates conformist behavior, i.e., compliance. Franck seeks to determine the extent to which state behavior can be explained by reference to rules and principles that undergird them. To accomplish this, he sets out four indicators, or properties, of legitimacy of the rulemaking process in the international community: determinacy, symbolic validation, coherence, and adherence.

Determinacy makes a rule's message clear. Unambiguous understanding adds impetus to rule-conforming behavior and fosters reciprocity. Legitimacy's cultural and anthropological dimensions are defined by notions of *symbolic validation*, complemented by *ritual* and *pedigree*. Here, communication is key, with emphasis on authenticity of a rule or rulemakers.

Interestingly, Franck excludes *justice* from the ingredients making for legitimacy. He does so on two grounds: (1) justice applies more aptly to persons than to artificial polities like states; (2) justice remains conditioned on a moral foundation, which makes it all too susceptible to

convenient misinterpretation or subjective mishandling by the secular state.

Franck's argument is an engaging and compelling one. In its fundamental sense, a rule's legitimacy depends on its ability to exert compliance from states. In a voluntarist international community, such compliance pull is often evidenced by states' agreement to act in accordance with that rule. Obligation thus remains rooted in the concept of community. Put tersely, public support by the community of national governments remains essential to the effective functioning of international law.

Similarly, law furnishes a coercive but more cohesive order. The expectation of continuity induces compliance by generating a sense of obligation; or, as Franck puts it, "Legitimacy is the standard by which the community measures rules' capacity to obligate" (p. 206). Importantly, however, Franck is careful how he uses the word *law* in his analysis; he does not aim to suggest notions of a hierarchial international normative order. Rather, he seeks instead to portray actions of states within a community conception of obligation.

These insights represent important new thinking in light of the post–Cold War world. Franck rejects the prevalent view that state behavior is based wholly on self-interest, as narrowly conceived in power relationships. His argument is for a more complex appreciation of international relations—one that takes into account the commitment to rule-based principles and explains the willingness of states to cooperate, even when cooperation apparently might not serve national self-interests.

This is a lucid, profound study that deserves close reading by students and scholars alike. The author's points are well substantiated by historical examples and abundant authoritative citations. There is much to be gleaned about both the theoretical bedrock of international law and the intellectual construct of obligation that functions to persuade governments to comply with international rules.

A theory of legitimacy must probe ideas that are implicit, unacknowledged, and reflective of community will, as well as ideas that are explicit, articulated, and well recognized. This work laudably achieves that ambition. Indeed, viewed from the vantage point of the United Nations' forceful reaction to Iraq's brutal aggression against, and annexation of, Kuwait, Franck's study takes on even added importance. In short, Franck has given us a serious philosophical inquiry with a relevant message that makes eminent sense for contemporary international relations.

George Washington University CHRISTOPHER C. JOYNER

Cautious Revolution: The European Community Arrives. By Clifford Hackett. New York: Praeger, 1990. 239p. $42.95 cloth, $15.95 paper.

The Dynamics of European Integration. Edited by William Wallace. New York: Pinter, 1990. 308p. $54.00 cloth.

These volumes are timely and complementary analyses of various aspects of renewed European economic and political integration—the most immediately significant element therein being the creation of a single European market under European Community auspices by the end of 1992. Hackett's book is intended as a "primer" (p. vi) for those seeking to explore the history, nature, problems, and prospects of the European Community. The nature of his discussion, however, is sufficiently developed that even those familiar with the EC will benefit from his analysis. The Wallace volume, on the other hand, is intended for social scientists and others seeking a more detailed discussion of theories and patterns of integration that include, but also go beyond, the EC Integration is defined by Wallace as the "creation and maintenance of intense and diversified patterns of interaction among previously autonomous units" (p. 9). Where Hackett places his discussion of the EC in the wider global arena, including the implications for United States interests, the 18 contributors to the Wallace book (primarily European academics and policymakers) are more concerned about dissecting the formal and informal dimensions of European regional integration.

Hackett, executive director of the American Council for Jean Monnet Studies, sees the evolution of the EC as "one of the momentous events of the entire twentieth century" (p. 14) but an event that the U.S. government and academic community have tended to pay insufficient attention to. This book is thus intended as something of a corrective. While generally optimistic about the prospects for European integration under the EC, Hackett retains analytical balance. He does not, for example, dismiss emergence of a Fortress Europe, noting the possibility that "some member states, and some forces within them, will see a barrier-free (EC) market as an improved opportunity to exclude outside competition" (p. 100).

Nor does Hackett exclude the possibility that the ambitious goals of 1992 could be slowed by two major problems facing the 12 EC nations: an agricultural subsidy program (the Common Agricultural Policy) that eats up two-thirds of the EC budget and a significant lag in European high-technology capabilities as compared to the United States and Japan. On balance, however, the author tends to foresee progress toward economic integration, including the emergence of a European Central Bank and a single European currency, with a slower but significant evolution toward EC-wide foreign policy coordination.

Wallace, deputy director of the Royal Institute of International Affairs, is also somewhat critical of the academic community. In his perspective, however, the problem is that theories of integration (which flourished in the 1950s and 1960s with the work of such scholars as Ernst Haas and Karl Deutsch) have not been updated sufficiently to take into account new patterns of European economic and political integration. Thus, his edited volume seeks, among other things, "to begin to rebuild the conceptual debate on European integration" (p. 4). Two political scientists, Robert O. Keohane and Stanley Hoffman, however, criticize the bulk of the other contributions as having failed to deal "with major conceptual and theoretical issues, such as those raised by the integration literature of the 1950s and 1960s" (p. 276).

Keohane and Hoffman have a point: the Wallace book is heavily weighted toward an exploration of economic, technological, trade, and institutional patterns of European integration—seen as a system with a core and a periphery of states. The contributors are also interested in informal indicators of integration (e.g., cross-border population movements). The volume is a rich source of significant and not easily obtained data. While Wallace's

aim to regenerate a discussion of theories of integration does not quite materialize, the book's other objectives are met, Keohane and Hoffman's well-taken point notwithstanding. It should be added, however, that their own attempt to unravel the relevance of earlier theories of integration is not wholly satisfying. For example, they assert that "perhaps a new theory of joint European decision-making should be invented, discarding loaded terms such as "supranationality" and "spillover," and drawing instead on contemporary theories of strategic choice in collective situations, or recent attempts to understand institutional innovation" and then proceed to relegate commentary on this to a note and to explore "some of the insights of neofunctionalist theories of change" (p. 284).

Political science increasingly needs to be part of multidisciplinary explorations of national and international political–economic developments. These volumes provide both specialists and nonspecialists with lucid—at times provocative—analyses. Both are calling upon students and specialists to reintegrate the study of European integration into the mainstream of social science concerns. They deserve an audience.

University of St. Thomas ROBERT L. FARLOW

Great Britain and the United States: Special Relations since World War II. By Robert M. Hathaway. Boston: Twayne, 1990. 172p. $25.95.

Losing an Empire, Finding a Role: An Introduction to British Foreign Policy since 1945. By David Sanders. New York: St. Martin's, 1989. 349p. $45.00.

Signals of War: The Falklands Conflict of 1982. By Lawrence Freedman and Virginia Gamba-Stonehouse. Princeton: Princeton University Press, 1991. 476p. $49.00 cloth. $14.95 paper.

In the post–World War II era, British foreign policy has been shaped by two long-term trends. First, Britain's relative economic strength has continued to decline. Second, the structure of Britain's economic interests has so shifted that Europe is now the centerpiece of the British trading network. In combination, these two trends have contributed to a fundamental restructuring of British foreign policy over the past 45 years. This process has led to three significant results: the abandonment of virtually all security-related commitments in the periphery, the gradual Europeanization of British economic and defense policies, and the steady deterioration of the special relationship Britain once enjoyed with the United States.

David Sanders and Robert M. Hathaway have both written informative, insightful, and highly readable historical accounts of this entire period of fundamental change and its consequences. Their points of emphasis are different. Sanders examines the evolution of British foreign policy vis-à-vis the empire, Europe, and the United States and argues that the changing pattern of Britain's external trade has been the decisive factor shaping the evolution of British foreign policy. Hathaway confines his analysis largely to Anglo–U.S. ties and the evolution of the special relationship and suggests that changing perceptions of the Soviet security threat and the steady deterioration of Britain's relative military capabilities have been the keys. Ultimately, however, both authors reach the same fundamental conclusion, namely, that due to structural changes in the nature of British power and interests, the future of British foreign policy lies in the direction of closer ties with Europe and a more detached stance in terms of both the periphery and the United States.

Both books also implicitly wrestle with an issue that international relations theorists are presently calling the agent–structure problem. At its core, the controversy centers around the relative importance of decision makers when it comes to accounting for international and foreign policy outcomes. The evidence they assemble tends to be impressionistic and largely anecdotal in nature. Sanders seeks to examine the worldview of British decision makers in an attempt to determine whether "realpolitik logic" has shaped policy deliberations and ultimately concludes that worldviews are basically inaccessible empirically. Hathaway focuses upon the interpersonal relationships enjoyed by successive British prime ministers and U.S. presidents and, similarly, fails to find compelling evidence that they matter a great deal in terms of shaping policy outcomes. The one possible exception to this generalization occurred in 1962 when President Kennedy authorized the sale of Polaris missiles to Britain "in large measure because of Kennedy's personal commitment to British–American ties and his fondness for Macmillian" (p. 65). Reluctantly, both Sanders and Hathaway seem to agree that when it comes to the agent–structure debate, neorealists have the better of the argument.

Whereas Sanders and Hathaway focus on the broad sweep of British foreign policy in the postwar era, Lawrence Freedman and Virginia Gamba-Stonehouse limit their attention to a single bloody episode: the Falklands conflict of January–June 1982. The result is a remarkably detailed case study that will be of interest to a wider audience than simply specialists in British and Argentine foreign policy. Students of decision making, coercive diplomacy, crisis management, international mediation, deterrence, and war all will find something of value. In short, this work is a major contribution to the literature.

The authors have produced a carefully crafted historical account based upon the analysis of recently declassified Argentine documents, personal interviews with unnamed British and Argentine officials, and existing scholarly research. The great strength of this work lies in the access it provides to the policy deliberations that went on in Argentina, Britain, the United States, and elsewhere. Numerous verbatim accounts of various policy debates are given. This technique lends a genuine sense of drama to the text, especially as we read some of the more riveting exchanges. Scholars interested in civilian–military relations will find the reported telephone conversation between Argentine president Galtieri and the commander of Argentine forces in the Falklands in the final hours of the war particularly memorable. Unfortunately, the sources of these accounts are not always identified by the authors. This raises some obvious questions about the reliability and authenticity of the reconstructions. This flaw, however, is not fatal and does not detract from the broader purpose of the study.

Freedman and Gamba-Stonehouse do not attempt to derive a set of lessons from this conflict. The narrative, however, speaks eloquently to the impact that uncertainty, misperception, bureaucratic, and domestic politics, as well as the sheer unpredictability of events, can

have in shaping state policy and strategic interactions. This said, however, it is interesting to note that authors do not regard the Argentine decision to resort to force as a consequence of misperception. The Argentine government acted very much like a "rational actor"—carefully analyzing British interests in the Falklands, assessing the local military balance in the South Atlantic, speculating about possible international reactions to the invasion, and discussing the impact that occupying the islands likely would have on Britain's willingness to negotiate the transfer of sovereignty. The authors argue that based upon the evidence available at the time, the Argentine leadership reached a reasonable conclusion about the potential political utility of using force. The authors also demonstrate that there was nothing inevitable about Britain's decision to try and reclaim the islands. Indeed, a consensus on this point did not emerge within the British Cabinet until after the invasion actually took place.

Misperception did, however, figure prominently in shaping the final outcome of the conflict. During the last week in March the Argentine leadership became convinced that Britain had begun mobilizing its naval forces with the intent of reinforcing its position in the South Atlantic. In fact, however, no such decision had yet been made. On the basis of this misperception, however, Argentine leaders felt it was necessary to begin the invasion as soon as possible. Hence the military operation, which had been slated to begin on 15 May, was moved ahead a full six weeks. This was perhaps the most critical decision of the entire conflict. The authors argue that if Argentina had adhered to the original date, "the [planned] dispersal of the [British] fleet and the onset of the South Atlantic winter would have made . . . a response almost impossible" (p. 98). Ultimately, faulty intelligence, misread signals, and plain old dumb luck conspired to set the tempo of the engagement.

This point leads to a final observation. Like both Sanders and Hathaway, Freedman and Gamba-Stonehouse implicitly draw our attention back to the agent–structure problem; and once again, structural factors seem to have the better of the argument. This can be evidenced in the "deadlock" structure of British–Argentine negotiations prior to the invasion, the structural uncertainty characterizing both sides' evaluations of the other's intentions, motivations, and military mobilizations, and finally, the structure of the military confrontation itself. Taken together, all three books raise serious questions about the extent to which decision makers are really capable of steering events. They also point to the need for more explicit considerations of the agent–structure problem.

Virginia Polytechnic Institute and State University K. EDWARD SPIEZIO

China's Participation in the IMF, the World Bank, and GATT: Toward a Global Economic Order. By Harold K. Jacobson and Michel Oksenberg. Ann Arbor: University of Michigan Press, 1990. 199p. $32.50.

China and International Law: The Boundary Disputes. By Byron N. Tzou. New York: Praeger, 1990. 158p. $42.95.

The Chinese leadership's decision, beginning in the early 1970s, to rejoin the international community after a partially self-imposed period of isolation was greeted with enthusiasm by most other nations. The two volumes under review deal with two different questions addressed by the People's Republic of China's new outward-looking policy: global financial institutions and international law.

China joined the International Monetary Fund and the World Bank in 1980; and in 1986 it formally requested that negotiations be opened on its full participation in the General Agreement on Tariffs and Trade. The acceptance of the international financial order that was implicit in these actions reversed the largely autarkic economic policy China had pursued since 1949. China had extensive economic relations with the Soviet Union and Eastern Europe in the 1950s and cultivated commercial ties with Western Europe and Japan in the early 1960s. However, in both these cases the purpose of the external involvement was to build an integrated and self-reliant national economy. Chinese officials viewed keystone international economic organizations (KIEOs) as tools of capitalist exploitation, while on the other side, many a Western government sought to apply an economic embargo against China.

When China announced its intention to seek membership in the hitherto-despised KIEOs, there were mixed reactions. On the one hand, given China's huge size and population, its participation would represent real progress toward a global economic order. On the other, there were misgivings about how China's membership would affect the functioning of the KIEOs. Jacobson and Oksenberg posit four possibilities: (1) China would advocate major changes in the KIEOs that would benefit developing countries; (2) it would press for smaller changes that would allow its financial practices to be applied within the organizations; (3) it would demand to be treated as a special case; or (4) it would basically work within the framework of the KIEOs. In general, they believe, the fourth possibility comes closest to describing China's actual behavior. While China has been treated sympathetically, they argue, it has not been pandered to. Many of the allowances made for China are similar to those adopted for other large, less developed members, such as Egypt and Indonesia. At the same time, membership in the KIEOs has necessitated familiarity with the accounting techniques used by those organizations; and the Chinese have made certain adjustments to their own techniques. Pressure to release data so that China's applications for loans can be properly evaluated has not only made the Chinese system more transparent to outside observers but also expanded the number of Chinese who have access to data and who can therefore comment knowledgeably on policy choices.

The authors' apparent assumption that China's entrance to the General Agreement on Tariffs and Trade would be smoothly and expeditiously accomplished seems excessively optimistic. One wishes they had been more specific about the obstacles that must be overcome, since these have an important bearing on an assessment of China's participation in the world financial community. It is also unfortunate that the authors tend to treat China as a unitary actor. We know that there is substantial concern within certain segments of the Chinese leadership about even the modest amount of foreign debt that China has incurred. Some discussion of this and of what, if anything, it may mean for China's

attitude toward the international financial order would be helpful.

Byron Tzou's focus is how China has used international law to deal with its various boundary disputes. He finds that China has in general operated within the constraints of international law, choosing only two of the many methods available (negotiation and mediation) and rejecting other recourses such as inquiry, conciliation, arbitration, and judicial settlement. Partly, this is due to the traditional Chinese distaste for judicial proceedings, even when the grievance is legitimate; and partly it is because China was for many years not a member of bodies such as the United Nations and was therefore understandably reluctant to use the adjudication methods provided by these organizations.

Tzou, born on the mainland and educated in Taiwan, is scrupulously objective in his treatment of the various disputes. His book is a model of organizational clarity, with each dispute treated separately before general conclusions are drawn. However, contrary to the author's assertion (p. 56), there *is* a border dispute between China and Vietnam. While the disputed land frontier between the two countries involves very little territory, it has the potential for taking on greater importance in the context of generally poor relations between the two states. Furthermore, disputes over the ownership of the Paracel and Spratley Islands have caused major frictions between China and Vietnam. The need to counter Vietnam's claims to the latter has resulted in the Chinese military's increasing its allocation for naval modernization and in the army's garrisoning arid and rocky islands at substantial financial and human costs.

Although both of these books are written primarily for specialists, the prose is in both cases admirably crisp and clear. Each work represents an important contribution to our understanding of China's international behavior.

University of Miami, Coral Gables JUNE TEUFEL DREYER

Cross, Crescent, and Sword: The Justification and Limitation of War in Western and Islamic Tradition. Edited by James Turner Johnson and John Kelsay. New York: Greenwood, 1990. 236p. $42.95.

The provisions of the tradition of the just war have become familiar in recent years to readers of professional literature and even news magazine cover stories about the morality of this or that war or this or that tactic. For a resort to war to be considered justifiable, *jus ad bellum* criteria must be satisfied. The resort must be defensive, the goal proper, and other remedies tried first; only duly constituted authorities may resort to war and only if there is reasonable chance of success. In the conduct of war, the *jus in bello* requirements of proportionality and noncombatant immunity must be respected. These features of this dominant Western tradition are deftly explored in papers by James Turner Johnson, Jeffrey Stout, Stephen E. Lammers, and Robert L. Phillips. Only Stout offers an original argument, namely, that Aquinas placed protecting justice ahead of any presumption against violence or warfare.

But what about the Muslims? Do the 800 million people inspired not by Aquinas and Grotius but by the prophet Muhammed and his followers accept these constraints on warfare? Or have their philosophers and theologians developed constraint notions that parallel those of the Western just war tradition? To a Western reader, at least, the most useful papers in this volume (which grew out of a conference) are those by specialists on Islamic thought who wrestle with these questions.

Abdulaziz A. Sachedina explores the notion of jihad. Though *jihad* should be translated not as "holy war" but rather as "*internal struggle*" or "*spiritual effort*," Islamic thought does seem to license not only war in defense of Islam but also "offensive endeavor in connection with efforts to bring about the kind of world order that the Qur'an envisions" (p. 39). Charles Butterworth's examination of the writings of tenth-century Muslim philosopher Abu Nasr al-Farabi reveals a much more generous warrant for warfare than one would find in the principle authors of the Western tradition (pp. 83–84). And John Kelsay agrees that much Islamic thought licenses military operations "to extend the boundaries of Islamic territory" (p. 200).

Though writers in the Western tradition have struggled, with some success, to reconcile revolt against oppression with the criterion that allows only authorities to initiate warfare, both Tamara Sonn and Khaled Abou El Fadl argue that Islamic thought, generally more authoritarian, is much less tolerant of rebellion and irregular tactics.

The Western principle of noncombatant immunity has been violated massively in twentieth-century high-technology wars of societies versus societies. But the principle survives as an embattled ideal. Islamic thought, Kelsay says, is much less clear about which people are fair targets and which must not be directly targeted—with an apparently more lenient position on the killing of enemy (infidel) civilians and prisoners.

Kelsay also suggests that paralleling the Western just war tradition, "a specifically Islamic contribution to the rules governing the conduct of modern war is still very much in process" (p. 217). It surely says something about the evolving Islamic view on these matters that the governments of the three dozen countries where most of the planet's 800 million Muslims live have all agreed to comply with the major documents of international law that codify and universalize the just war tradition's constraints (the UN Charter, Geneva and genocide conventions, etc.). These documents and their acceptance by governments representing Islamic populations are scarcely mentioned in this anthology. But doesn't this twentieth-century official embrace trump any classical or modern Islamic views that diverge from universally accepted (though oft-violated) international norms?

Purdue University JAMES A. STEGENGA

The Declining Hegemon: The United States and European Defense, 1960–1990. By Joseph Lepgold. New York: Greenwood, 1990. 225p. $45.00.

Joseph Lepgold's study of U.S. NATO policy represents the latest contribution to a second wave of research into the dynamics of international hegemony. The first wave, beginning in the mid-1970s, explored the relationship between hegemony and cooperation in the world economy. *The Declining Hegemon* (like other second-wave works, including Paul Kennedy's *Rise and Decline of the Great Powers* [1988] and Aaron Friedberg's *Weary Titan* [1988]) shifts our attention from economics to security

affairs while examining the foreign policy problems posed by decline.

Echoing Kennedy and Friedberg, Lepgold emphasizes that hegemonic commitments tend to stick on the way down, despite intensifying trade-offs as relative resource constraints tighten. The heart of the book is an examination of the U.S. nuclear and conventional commitments to European defense between 1960 and 1980. The external context of U.S. policymaking changed significantly during this period (Soviet military capabilities steadily expanded, and the relative economic might of Western Europe grew substantially), yet U.S. policy changed only modestly. United States officials clung to extended deterrence even after the Soviet Union achieved strategic parity, attempting to bolster its credibility by devising new schemes for fighting a controlled nuclear war. Policymakers showed more flexibility on the conventional side. Monetary strains, congressional pressure, and growing European economic resources all prompted U.S. leaders to campaign for more equitable conventional burden sharing within the alliance. Nevertheless, these efforts to bring about marginal adjustments in the U.S. commitment were often pursued reluctantly and met with limited success.

Lepgold argues for a multilayered approach to explaining this pattern of delayed adaptation to decline. Systemic-level theories are, taken alone, inadequate to account for policy responses. Aside from their imprecision, systemic theories offer contradictory predictions. Realism suggests that declining resources should lead to a corresponding reduction in external commitments. On the other hand, deterrence theory points toward the importance of preserving commitments, even at the risk of overextension. A hegemon must be concerned that displays of weakness will lead allies to question its resolve or credibility, thus prompting defections.

In practice, executive branch officials have been most concerned about preserving U.S. credibility while congressional critics have been more preoccupied with overextension and burden sharing. Lepgold argues that U.S. policymakers have exaggerated the requirements of deterrence and alliance unity, tending to overestimate the likelihood of bandwagoning behavior. Thus, we must examine psychological beliefs, as well as systemic constraints in order to understand policy preferences.

Lastly, the ability to implement policy preferences is constrained by state structure, as well as by the nature of the issue area. Operating within a weak state structure, U.S. policymakers must sometimes accommodate congressional resistance. This constraint varies across issue areas, however. The president has greater autonomy over nuclear weapons policy than over conventional forces. Issue area also affects the leverage a hegemon has over its allies. Extended nuclear deterrence constitutes a public good, constraining the ability of the United States to limit its commitment or press for greater allied burden sharing. Conventional forces can be more easily controlled, and bargaining over relative contributions is more feasible.

Although dense and turgidly written, Lepgold's book provides a useful study of U.S. NATO policy. His attempt to apply the concept of hegemonic decline to security policy is welcome and interesting. The author's explanatory framework is, however, cumbersome; and his review of the relevant theoretical literature, while sophisticated, is overly long and eclectic. A more elegant and parsimonious approach to theory construction, along with a clearer consideration of alternative hypotheses and explanations, might have yielded predictions easier to generalize and evaluate. While the NATO case generally supports Lepgold's thesis of delayed adaptation, he himself notes several drawbacks. At some points, the United States seriously considered significant alteration of its European commitments. During the mid-1960s, for example, when McNamara flirted with a policy of no-nuclear-first-use. Lepgold acknowledges that NATO fails to provide a tough test of his argument: as an area of prime strategic significance, one would expect policymakers to adjust commitments elsewhere before altering ties with Western Europe.

Finally, while Lepgold concludes with a lucid discussion of recent and future U.S. NATO policy, he makes little attempt to relate the book's thesis to post–Cold War U.S. foreign policy. One wonders, in fact, whether an argument designed to explain the U.S. responses to gradual change in the bipolar structure of previous decades holds much relevance in the dramatically altered international environment of the present. Despite these qualms, Lepgold's book stands as a thoughtful addition to the growing literature on hegemony and foreign policy adaptation.

Drake University DAVID SKIDMORE

The Ways of Power: Patterns and Meaning in World Politics. By George Liska. Cambridge, MA: Basil Blackwell, 1990. 511p. $39.95.

The Ways of Power is an ambitious and panoramic analysis of global politics. Although the author's vision is grounded in classical realism, it is a highly sophisticated and self-conscious variant focusing on historical change of international systems and the actors that constituted them. Power, of course, plays a central role in the analysis; but its psychological and spiritual features are as central as are measurable capabilities. The state, too, is an omnipresent (though vaguely defined) element in the framework. For the author, any effort to equate the state with society must have "consequences fatal to community" (p. 205). Conflict and war are regarded as natural—even necessary—to give history an appropriate nudge "as a matter of natural selection of viable actors and agendas" (p. 228).

Actors and systems are seen as inevitably and naturally evolving through distinct stages in recurring cycles. Just as actors evolve from city–states to larger entities and then mature and weaken, so do systems of states expand and later contract. The author illustrates these sequences in system–actor development by reference to numerous historical periods and regions from ancient Mesopotamia to the present; but the stages themselves (like other typologies the author elaborates) are asserted, rather than justified, and so seem arbitrary. There are many ways to sort the same information, and the reader may feel somewhat overwhelmed by the trichotomies and triads that are heaped up. Indeed, on occasion, it appears that history is forced to shape itself to Liska's typologies.

In Liska's historical framework, structure and changes in structure are viewed as largely deterministic; and structure (and therefore behavior) are regarded as the products of a time–space continuum—what Liska calls "political physics." This is not, however, the Newtonian

physics of traditional and static realism but realism influenced by Einstein's relativity. Consequently, different actors and systems will simultaneously be at different points on the time–space continuum and will evolve at different rates of speed; and the behavior of actors will be largely determined by their position in systemic schisms along with their place on the rise–decline curve.

Like other realists, Liska does not regard history as equivalent to progress and believes it is folly for leaders to try to channel history in particular directions. But instead of simply denouncing "utopians," who seek to deny the realities of structure, Liska describes what he calls "politicized poetics." Global politics is repeatedly characterized by one of two types of tragedy. The first is a two-power conflict in which the contestants destroy one another "in casting out imagined or exaggerated evil" (p. 189). The second, which Liska calls "the tragic hero in interstate relations," is the actor who seeks hegemony in defiance of structural and normative impediments. Such an actor is doomed because it tries "to abrogate the laws of the relevant nature by escaping not only the chain of conflicts with equal powers but also and mainly the next turn in the cycle of rise and decline deputizing for birth and death among mortal states" (p. 190).

What is it that enforces the iron laws of history and so limits the potential for political genius? Unsurprisingly, it is that old realist standby, the balance of power. Liska's analysis of the balance is less original than his vision of a dynamic global structure; and that analysis leaves unresolved most of the logical and empirical issues associated with traditional balance-of-power theory. Nevertheless, he does admit that "perfect" equipoise is an ideal rarely met because of the dynamic nature of political systems.

The book first provides readers with the author's core concepts and typologies, then illustrates them with a galaxy of historical cases. Additional theoretical chapters follow; and the final section of the book applies the concepts and theories to a variety of issues in contemporary world politics. Although the book was completed before major events like the reunification of Germany and the war in the Persian Gulf, the analysis remains remarkably fresh. The Cold War, as the author interprets it, passed through the major cleavages characteristic of international systems (East vs. West, land vs. sea power); and the alternatives confronting the superpowers (e.g., a condominium of the two, the effort by one to create unipolarity, etc.) are, in Liska's view, those that great powers have faced many times before.

The Ways of Power is a genuinely original, timely, and important contribution to international relations theory. The influence and continued relevance of Aristotle, Thucydides, Machiavelli (theoretical chapters are labeled "discourses"), and other classics are repeatedly revealed in the analysis. Whatever one's view of the realist tradition, this volume is an authentic addition to it and extension of it. Ultimately, if the volume is not as widely read as it deserves, this will be largely a result of the author's unusually periphrastic style that obscures important points and renders virtually impenetrable whole paragraphs and pages.

Iowa State University RICHARD W. MANSBACH

Exporting Democracy: The United States and Latin America. Edited by Abraham F. Lowenthal. Baltimore: Johns Hopkins University Press, 1991. 422p. $55.00 cloth, $12.95 paper.

U.S. Policy in Central America: The Endless Debate. By Dario Moreno. Miami: Florida International University Press, 1990. 186p. $26.95 cloth, $14.95 paper.

The Ordeal of Hegemony: The United States and Latin America. By Guy Poitras. Boulder: Westview, 1990. 214p. $34.95.

These three books share some common underlying political assumptions largely shaped by the fevered and prolonged debate over Central America policy in the 1980s. Hence, they are already dated, somewhat overwrought, a bit passé, and quite one-sided in a number of their assumptions and analyses. In all three, scholarship too often yields to polemics and partisanship.

Dario Moreno's *U.S. Policy in Central America* originated as a doctoral dissertation at the University of Southern California. While containing some valuable analysis, its subtitle, *The Endless Debate*, shows how the book has been overtaken by events. Central America is now largely out of the headlines; the vigorous policy conflict of the 1980s is over; and thanks to Secretary of State James Baker's skillful political maneuvering, Central America and its problems have been largely finessed (albeit not "solved"). In addition, Moreno's analysis of the assumptions and processes of Reagan administration policy on Nicaragua and El Salvador is often partisan and misleading.

Guy Poitras's *Ordeal of Hegemony* has its origins in a well-known article (Lowenthal, Abraham F. "The United States and Latin America: Ending the Hegemonic Presumption" *Foreign Affairs*. 1976.55:199–213). This was a Carteresque article that served as a basis for some of the thinking of the new president's Latin America advisers. But that view, which implies a unilateral reduction of the U.S. role in the world (particularly in this hemisphere) has been repudiated overwhelmingly by the voters in the last three presidential elections. Moreover, with the decline of the Soviet Union and with Europe and Japan showing little interest, Latin America has been thrown back more strongly than ever into the arms of the United States, whether or not we (or they) wish it or not.

Abraham Lowenthal's edited *Exporting Democracy* is disappointing. Well funded and with several reputable contributors, it is nevertheless weaker and more dated than one would expect. First, the book has no statement of themes, introduction, or discussion or definition of what Latin America means by *democracy* (often closer to a Rousseauian than a Lockean conception). Second, while most of the authors in the book are committed Democrats, now that the democracy–human rights agenda has largely been taken over by Republican administrations, they are forced to be more critical of it. Third, the historical approach employed in the book emphasizes Latin America's often unhappy past experiences with democracy and the frequently mixed motives of U.S. foreign policy but contains no information on what is new in Latin America to provide a more solid basis for democracy (greater affluence, larger middle class, rising literacy, greater institutionalization, etc.); nor does it come to grips with the changes in U.S. policy that led us finally to conclude that democracy is the best basis for promoting U.S. security interests. We should

further say that the selection of contributors produced a quite uneven book, with several mediocre chapters and the glaring omission of many of the leading writers on the subject.

Actually, once one gets past the pretentious, inflated rhetoric, and partisanship, there is much in these three volumes to recommend them. Moreno usefully analyzes the domestic debate over Central America policy in terms of the familiar division between "Cold War internationalist" (which he identifies with the Reagan administration) and the "post–Cold War internationalists" (which he identifies with the Carter administration and the present Democratic government-in-waiting). He argues that both the Carter and Reagan administrations faced failure in their policy because of lack of foreign policy consensus since Vietnam and the democratization of foreign policy, which made domestic forces sufficiently powerful to moderate and even hamstring national policy. He dissects the rhetoric of both these recent more ideological administrations and then in the substantive part of his book shows how domestic politics prevented both from carrying out their agendas. This is solid material, some of it fresh and original; and Moreno's rendering of it is generally balanced. However, he still sometimes falls into the trap of repeating the tired and inaccurate clichés that the Reagan administration was "obsessed" with Nicaragua and that it only sought "military solutions"; moreover, his sources are similarly heavily weighted toward one side of the political spectrum and from persons in a previous administration not usually thought of as the high point of U.S. foreign policy success.

Poitras's book is an ofttimes thoughtful overview of U.S. policy in Latin America. His theme is the presumed decline of U.S. hegemony in the Western hemisphere. He analyzes the history of U.S. policy in Latin America and goes through different scenarios of the "decline" literature. He looks at U.S. security policy, Central America, economics and the debt issue, and U.S. intervention. However, he also repeats some of the hackneyed phrases of the 1980s, such as that the United States is a "slowly fading giant" and that it is "irresistibly drawn to intervention." Poitras has clearly been caught up in the political–ideological quarrels over Latin America during the past decade. But he is a senior scholar, his earlier study on *Latin America: The Politics of Immobility* is a fine book, and he is too thoughtful to be just another ideologue. Thus, Poitras repeatedly states his decline-of-hegemony thesis and then backs away from it as his own analysis leads him to other conclusions. For example, Poitras sees international debt as a major factor in indicating U.S. decline but is forced to conclude that the debt has actually increased U.S. power in Latin America. He concludes the book not on the hegemony theme or the presumed decline thereof but on the familiar note of calling for greater cooperation and the finding of common ground between the United States and Latin America. One suspects that the reason for his ambiguity is that he began the book during the white heat of the divisive Central American debate but finished it when the policies of the 1980s had begun to bear positive fruit or been moderated: the Nicaragua election, the U.S.–Soviet approchement, a succession of disarmament treaties, greater global cooperation, and even an arguably more mature U.S. policy in Latin America.

Lowenthal's edited volume is plagued by many of the same problems. It contains four historical chapters: Paul Drake on the Wilson period, Leslie Bethell on the early Cold War, Tony Smith on the Alliance for Progress, and Thomas Carothers on the Reagan Era. While containing valuable information and analyses, the common thread that runs through these chapters is hostility to most aspects of U.S. policy. The country chapters (Argentina by Carlos Escudé, Chile by Heraldo Muñoz, the Dominican Republic by Jonathan Hartlyn, Mexico by Lorenzo Meyer, and Nicaragua by Joseph Tulchin and Knut Walter) also have much worthwhile data and insight; but several are marred by talk of numerous "contradictions" and by excessively critical, although usually unsubstantiated, comments about recent U.S. administrations. The most refreshingly original chapter is by Elizabeth Cobbs on U.S. business in Latin America where she points out that business policy has often been far ahead of, and more enlightened than, U.S. official policy. As well, Cobbs's may also be among the few studies dealing with a glaring omission in the field—the relations of business to political development. Lawrence Whitehead penultimately discusses the international role in assisting democracy; and the editor adds an uninspiring conclusion—a shame, because, given the importance of the topics and the quality of many of the contributions, this could have been an excellent and major study.

These three books and so many others lead one to worry about the field of Latin American studies. Because of the intense conflicts of the 1970s and 1980s, has the field now become so politicized as to all-but-rule-out serious, genuinely *objective* scholarship? Not quite, but we have come perilously close to that point.

University of Massachusetts HOWARD J. WIARDA

Inventing Accuracy: A Historical Sociology Of Nuclear Missile Guidance. By Donald MacKenzie. Cambridge: Massachusetts Institute of Technology Press. 1990. 464p. $29.95.

Inventing Accuracy recounts the development of nuclear missile guidance in the United States and the Soviet Union. It is based on available documents plus 140 interviews with guidance and navigation technologists, navy and air force officers, and defense officials.

The thick description of missile guidance technology provides information about an important element in the nuclear command-and-control process. It adds new detail to public knowledge of the area known as C^3I (Command, Control, Communications, and Intelligence) and makes it accessible to a wider audience. This knowledge is relevant background to understanding smart nonnuclear missiles in the post–Cold War era (e.g., the accuracy of the Patriot missile in the Persian Gulf).

At the same time, the book brings new concerns to weapons development case studies. Where previous work has focused on the high politics of weapons, this study shows that low politics is critically important as well. Since it provides technical details about the black box of inertial guidance, it also shows that this black box includes politics. The technical is not naturally isolated from the political—it is joined with it in a "seamless web" (p. 411). This web goes well beyond and below our received understandings of bureaucratic politics, into

the sheltered laboratories of science itself, into the politics of the ordinary. The state and political decision are disaggregated to an infinite level of detail: "The black box of technology is actually a Russian doll. . . . Only the analyst's energy and the reader's patience provide a limit" (p. 393).

The micro details of technology also provide a view onto the macro workings of the international political system they support: "Missile guidance forms a technological 'window' into crucial divides in nuclear thinking," including both the technical and political aspects of the "projection of military power over long distances" (pp. 2, 385). The study shows that important aspects of the international system are not given but made, and it shows how they are made. It is thus an addition to general theory and research in international relations.

In addition to substance, *Inventing Accuracy* unobtrusively provides interesting methodological contrasts and insights. It takes the approach of historical sociology, using qualitative oral interviews and written documents as data. It is an ethnomethodological anthropology, "a journey among a tribe, which, to most of us is unknown and alien" (p. 25). Yet this tribe is quite mathematical, and one of its central concerns is "the theoretical understanding of errors" and the mathematical modeling of error processes (p. 349). Consequently, Mackenzie's study of missile accuracy treats the reader to a detailed discussion of the error term in the guidance equation, which can easily be extended by analogy into political science. The discussion of missile error budgets includes a standard discussion of differentiation between systematic bias and random error. Beyond this, however, there is an explanation of sophisticated techniques for the estimation of error component parameters, empirical extrapolation of error in different contexts, and error compensation algorithms.

Finally, *Inventing Accuracy* is a contribution to political epistemology. Mackenzie challenges both technological and political determinism. The central theme of the book is that science and technology are constructed, not inevitable. Science is "a historical product and social creation" (p. 2). There is no "hard fact," no "atomic fact" with "absolute warrant," only flexible "knowledge claims" (pp. 10, 381). Parts of the "network of knowledge" may be more or less "well-entrenched" (p. 364). Nevertheless, claims can never be conclusively and finally tested, proven true and valid once and for all. They are always open to challenge, even though such challenge may not actually be made (p. 10). Certainty tends to vary with the distance from knowledge production. Naive knowledge consumers tend to be true believers to a greater extent than sophisticated producers (p. 371). Indeed, the producers have an important interest in maintaining the hardness of fact and the autonomy of science precisely so that external confidence and support will continue. They are the basis of the expert's power and authority.

Science and technology take the form they do because of the choices, decisions, and behavior of actors living in a particular historical, social world. The past included different possible alternative political paths: "Different technical pathways are seen by those involved as having different military and political meanings" (p. 19). Science and technology could thus have been discovered and invented differently had the actors lived in different times or place, seen the world differently, or chosen or acted differently in the past.

More importantly, different alternative paths stretch equipossibly into the future. Aspects of contemporary science and technology can thus be remolded if new actors perceive and choose differently in the future. Scientists can imagine and construct different possible sciences. The political sphere is much wider than we have previously believed. Scientists, including both natural and social scientists, are political actors not only when they vote but also in their everyday scientific lives.

Modern weapons are not inevitable, either their creation or continued existence; and neither is the larger international political system that they support and in which they are embedded. Both the technological and political systems can be deconstructed, "uninvented" through continuation of the same process that created them (p. 424). The Pandora's Box of a missile-based international system cannot be closed at once. But perhaps it may gradually be taken apart, enclosed in another container, or simply left behind.

University of Colorado FRANCIS BEER

The Myth of America's Decline: Leading the World Economy into the 1990s. By Harry R. Nau. New York: Oxford University Press, 1990. 424p. $29.95.

Such is the plethora of books on U.S. hegemony (or decline thereof) that one's reaction to the title of Nau's book is an unenthusiastic groan. Yet the book does reward a good reading. It is not merely a rehash of tired academic arguments about hegemonic stability but a sensitive account of international economic diplomacy, emphasizing the Reagan years. A former National Security Council staffer, Nau's theme is the importance of ideas and policy choices, rather than exogenous structural factors.

The book covers the Bretton Woods conference up to the end of the 1980s. The descriptive theme is that although the coercive power of the United States has declined in relative terms, the United States has retained its ability to achieve desired outcomes by seeking "common political purposes and common economic policy standards" with other countries (p. 369). The world is, he believes, converging on a commonly held set of norms and values that guide economic policy; and this consensus has served to offset the decline of U.S. power. The descriptive theme underpins the explanatory argument. Structural explanations of outcomes (those relying on exogenous variables, such as power concentration, systemic polarity, etc.) fail to provide an adequate explanation because they neglect ideas and the policy choices that follow from the competition of ideas. The vicissitudes of U.S. foreign economic policy have been due as much to policy purpose and choices as to structural determinants.

Explanation leads to prescription; and Nau has some strong rules that he feels ought to guide international policymakers: "America has a choice to reaffirm Western values of human freedom and individual dignity and to reapply the efficient economic policy standards of price stability, flexible domestic markets, and internationally freer trade of the Bretton Woods era" (p. 370). Unfortunately, Nau does not seem sure of how his desired policies are to be achieved—what practical mechanisms will lead to the consensus he prescribes. His disparaging comments on bureaucratic politics and organizational

design as policy remedies and his moderate praise of Reagan's economic vision suggest that Nau may hope that his prescription would appeal to a suitable political entrepreneur.

The organization of the book is simple. Part 1 sets the framework by contrasting the structural approach with Nau's "choice-oriented perspective."Purpose and policy, not power and politics, drive outcomes. The distinction may not be as clear as Nau believes (he confesses the possibility, p. 51) and is disadvantaged by the fact that his argument for the primacy of policy depends on his own interpretation of historically unique events. Some of his comments on alternative models are a little puzzling (e.g., the assertion that market-based models assume harmony of interests and an egalitarian distribution of power, pp. 66–67).

Parts 2 and 3 examine the rise and fall of the Bretton Woods system. The United States was slow to take advantage of its postwar opportunity to lead a consensus around Bretton Woods values, until the Marshall Plan provided the impetus. Nau follows the familiar story that the postwar economic system was built around the Western security system as a "shared political community." His account of the fall of Bretton Woods is more original. Contrary to those who have suggested that Bretton Woods fell because the lack of an adjustment mechanism led to the need for dollar-based liquidity, which in turn undermined confidence in the convertibility of dollars, Nau argues that there was no inherent structural instability in the system. Claiming that there was no liquidity problem, Nau states that the problem was policy failure in the United States in the form of an unwillingness to contemplate deflationary macroeconomic policies in the 1960s. The United States was, he says, "unwilling to recognize the roots of instability in domestic politics [and] blamed the international system" (p. 148). Hence, the deliberate smashing of the system in 1971 and a decade of policy drift. The Reagan administration faced, he believes, a unique opportunity to return to good policy—the "Bretton Woods triad"—but failed to do so for reasons that I, at least, never understood. Nau's explanation is that "the Reagan Administration lacked a full, integrated, intellectual understanding of its programs and failed to generate the political will and support to follow through on its programs" (p. 223). This might describe any government. The book returns again and again to this curious lacuna: we need good policy leadership, but Nau is reluctant to tell us how to get it.

University of Iowa JOHN A. C. CONYBEARE

The Soviet Union in the Horn of Africa. By Robert G. Patman. Cambridge: Cambridge University Press, 1990. 411p. $49.50.

Soviet Strategy in Southern Africa: Gorbachev's Pragmatic Approach. By Peter Vanneman. Stanford: Hoover Institution Press, 1990. 142p. $15.95.

The Soviet Union has a surprisingly long history of relations with the countries, conflicts, and political movements of Africa. Indeed, since the successful conclusion of the Bolshevik Revolution, the Soviets have—with varying degrees of commitment—promoted the cause of national liberation in their foreign policy. First under the aegis of the Communist International, later with the provision of military equipment and training to insurgent guerrilla groups in the 1950s and 1960s, and finally with the direct assistance of Soviet and Cuban military personnel in a variety of brush war conflicts in Africa during the 1970s and 1980s, the Soviet Union has championed the plight of revolutionary groups and fledgling socialist governments in black Africa.

There was a period of increased Soviet activism in Africa during the 1970s when the Soviet Union was involved in several regional disputes and revolutionary changes, principally in Southern Africa and around the Horn of Africa. During this period, Soviet optimism about the success of the Socialist model, the "leading role" of the vanguard party, and the power of the Soviet state was at its peak. The Soviet Union became primarily engaged in the endgame after the collapse of the Portuguese empire in Southern Africa and sponsored local Marxists to power in both Angola and Mozambique. Subsequently, the Soviet Union sided with Ethiopia against its erstwhile ally, Somalia, and again sent massive shipments of armaments to the region.

However, the optimism of the 1970s faded by the middle of the 1980s. These outposts of empire and the fledgling Soviet foothold in Africa were undermined by local and international forces that conspired to showcase the limits of Soviet influence. The Reagan Doctrine of championing anti-Soviet insurgencies in the Third World did much to tax Soviet energies and extend them in the peripheries; and the Soviet Union did little to alleviate the dire economic predicaments of its local allies. Even before the era of Gorbachev and perestroika, the Soviet Union had learned the traditional lessons of imperial powers with far-flung holdings: it is easier to undermine an empire than to hold it. Or as one Southern African politician translated it to an African context, "it is easier to be a poacher than to be a gameskeeper."

The Soviet involvement and intervention in African politics and areas far from its territory spawned a tremendous literature in the West concerning the sources, methods, and achievements of Soviet foreign policy. Much of this Western literature hoped to describe a complex set of motivations that led the Soviet Union to engage its weapons and prestige in far-flung corners of the globe. However, the current circumstances of the Soviet Union have changed so dramatically since the African activism of the 1970s that the study of Soviet–African relations has been rendered virtually obsolete. With the collapse of pro-Soviet regimes in Eastern Europe and the increasing signs of Soviet disorder and decline, Soviet policy toward Africa has received much less academic attention in the last several years. Yet the Soviet Union, perhaps contrary to conventional wisdom, has persevered in its political, military, and diplomatic initiatives in Africa long after there were any identifiable geopolitical benefits or regional rewards to be gained.

Trying to explain this persistence is the subject of these two very able, but very different, efforts on Soviet policy in Africa. The first, Robert G. Patman's *Soviet Union in the Horn of Africa*, traces the diplomacy of intervention and disengagement of the Soviet Union in the Horn of Africa from its earliest periods. Patman draws on extensive Soviet and Russian source material to examine the earliest Russian interest from the czarist period to the initial era of Soviet rule and subsequently through the Cold War in Africa to the current period of Gorbachev and retrenchment.

Patman makes a persuasive case that Soviet activities in the Horn cannot be neatly compartmentalized into Africanist or globalist schools of thought. The Africanist approach, he explains, "emphasizes that Soviet policy in the region lacked coherence and was essentially reactive and opportunistic in the face of a local conflict that was not of Moscow's making." In contrast, he explains, the globalist school of thought argues that "Soviet behavior in the Horn was consistent with the grand design to undermine Western and Chinese influence wherever possible through the cynical manipulation of local grievances" (p. xiii). Patman's book suggests that there is a better explanation for Soviet involvements, which he describes as an approach of "calculated opportunism" that is based more on short-term openings and initiatives than on long-term strategic blueprints.

Patman provides us with a thorough accounting of a period of Soviet adventurism in the Horn that appears to be coming to a close. He has managed to sift through a tremendous range of Soviet sources to trace the Soviet initiatives during its most important period, 1973–79. Patman also offers lucid analysis of the expansion of Soviet–Somali ties in the late 1960s and early 1970s and the disputes in Soviet–Somali relations before the Ethiopian revolution in 1974. On several occasions he points to Western misreading of Soviet intentions in Ethiopia and makes a powerful case for the reasons and rationale that led to the Soviet intervention. Patman then takes us through the sometimes contentious relations between Mengistu and a procession of Soviet leaders and the subsequent decisions to reassess Soviet policies towards Ethiopia and the Horn at large. The book also provides a fascinating and detailed description of the Ogaden campaign and the international diplomacy and intervention that shaped the war.

The only major shortcoming in this generally informative book is that while there is a tremendous record of the intervention, there is not nearly enough discussion about the issues that have led to the Soviet disengagement and diplomatic efforts to promote national reconciliation and retreat. The shifts in Soviet foreign policy from confrontation to cooperation and even condominium vis-à-vis the United States and the international community needs much greater amplification. In this sense, there is little context for current Soviet policies in the Horn among the larger dilemmas of Soviet policy in the fall of Eastern Europe and the dire domestic problems facing the Soviet leadership at home. While Patman rightly states at the outset that Western analysts ignore Soviet writings at their peril, a minor shortcoming of his own book is that he has not consulted a number of important Western studies about change and continuity in Soviet foreign policy. It is, however, an important historical rendering of the complex history of conflict, discord, and disengagement that marks Soviet policy toward the Horn.

A second book explores very different territory, Peter Vanneman's *Soviet Strategy in Southern Africa*. Vanneman was one of the first Western commentators to explore Soviet policy in Southern Africa; and his work in the early 1980s (particularly on the Soviet involvement in the wars of Southern Africa), played an important role in helping to understand the scale and significance of foreign intervention in the region. His book is subtitled *Gorbachev's Pragmatic Approach* and makes a rather dubious case that Southern Africa still plays an important role in the formulation and execution of Soviet foreign policy. Vanneman cites the meetings between Soviet and African dignitaries, various international discussions about the new directions of Soviet policy in Southern Africa, and the growth of personnel in Soviet embassies in the region as evidence of increasing Soviet interest toward the region. He uses the same prism and measurements of Sovietology that were more relevant in the past—ideological imperatives, geostrategic motivations, and the extension of empire. Yet, this so-called Soviet strategy in Southern Africa has been ambushed by the historic changes in the Soviet polity, rendering this traditional approach of Western Sovietology virtually irrelevant.

Vanneman does describe a Soviet Union that has played a prominent military role in the past but is determined to share in a larger political and diplomatic role in the emerging new political realities in Southern Africa. Here, his overall analysis is essentially sound, but some of his claims have been overtaken by events. For instance, the perceived Soviet interest in basing rights or air facilities in Southern Africa can hardly be seen as important, given the magnitude of the Soviet military retreats in arms control negotiation and in its Eastern European presence in the last two years.

Both Vanneman and Patman make a powerful case that Soviet policy in Africa is out of phase with the larger challenges and directions of the Soviet Union's foreign and domestic policy. Although the Soviet Union has seen drastic setbacks in such central strategic areas of East–West contention as Eastern Europe, it persists in competing for influence on the peripheries. Indeed, it is almost as if the global game for influence has been declared over by political elites in Washington and Moscow but someone has forgotten to inform the local agents and embassies of this decision, so that the proxy wars, desperate diplomacy, and clandestine meetings continue in Soviet policy toward Africa.

Harvard University KURT M. CAMPBELL

Nuclear Deterrence Theory: The Search for Credibility. By Robert Powell. Cambridge: Cambridge University Press, 1990. 230p. $47.50.

History has not been kind to the concerns of Robert Powell's new book, which appear exceptionally untimely in the current climate of superpower relations. Its technical style, too, will be uninviting to most political scientists.

Yet Powell's game-theoretic study deserves close attention. The book offers an imaginative and insightful examination of a critical issue in the nuclear era—whether it is possible for a state to protect its national interest by means of a threat to use nuclear weapons against an opponent with a capability to retaliate in kind.

Two well-known theoretical attempts to deal with this problem are subjected to game-theoretic analysis. The first is Schelling's idea of the threat-that-leaves-something-to-chance, and the second is the strategy of limited retaliation. In both cases, the effects on crisis dynamics and crisis stability are assessed.

Powell models these approaches to the credibility problem by means of games of incomplete information, which are solved for sequential equilibria. The process that underlies these models is the dynamic (Bayesian)

updating of prior probabilities by players who observe each other's moves as the crisis unfolds. This feature allows Powell to trace the net effect of complex interactions between players' beliefs and strategies. It also explains most of the results that the author presents as counterintuitive or unconventional.

For example, whereas brinkmanship crises are often said to be competitions in risk taking that favor the actor with the greater resolve, the game-theoretic model of brinkmanship indicates that such a description can be misleading. First of all, "sometimes bluffing works, and the state with the least resolve prevails" (p. 60). In addition, a state may be more, rather than less, likely to escalate when the opponent's resolve is increased.

The first result, however, does not undercut the "competition in risk taking" image. It merely reminds us that resolve has a perceptual dimension and that what an irresolute state must do in order to deceive may turn out to be quite resolute. The second result (that a greater stake in the status quo may not make crisis initiation less likely) obtains if the actors engage in a long and complicated chain of reasoning—which will be greeted with skepticism by empirically oriented readers.

The brinkmanship model also shows that contrary to intuition, the misperception of an opponent's resolve (or type) may increase crisis stability. Powell might be reminded that Stein ("When Misperception Matters," *World Politics* [1982]) demonstrated by means of a simple game model the sometimes benign effects of misperceiving a conditional strategy as a dominant one.

The findings are more surprising when the brinkmanship model is analyzed under the assumption that striking first is advantageous. The conventional logic predicts a destabilizing increase in the temptation to preempt. But the game turns out to be entirely stable: in equilibrium, neither the challenger nor the defender attacks. The reason, as Powell aptly demonstrates, is that the conventional logic treats beliefs as exogenous to crisis bargaining. Yet when beliefs and strategies are interdependent, "neither state can become sufficiently confident that war is inevitable to justify its attacking" (p. 122).

As to the debate between mutually assured destruction and counterforce, the model of limited retaliation suggests that states will in fact prefer to have more discriminatory options, as well as counterforce capabilities. The reason, however, is not damage limitation but coercive bargaining: the reduction of the opponent's options makes the opponent less likely to initiate or escalate further.

These conclusions and others that support current strategic theory rest on simplifying assumptions that detract from their persuasiveness. For example, the play of the game does not affect the players' resolve, which is assumed to be fixed; the assumption of common knowledge removes a critical element of the uncertainty that defines crises; and the exclusion of nonescalation or deescalation from the set of alternatives precludes compromise outcomes.

Unfortunately, these and other simplifications yield models that only a few would call simple. It is impossible not to wonder whether similar results could not be achieved by more tractable models.

The *descriptive*, rather than prescriptive, terms in which the results of the book are couched also invite reflection on the empirical accuracy of predictions about actors capable of the most complex chains of reasoning. Indeed, this book is symptomatic of the gulf that separates game-theoretic studies of crisis from their psychological counterparts. The cognitive effects of crisis-induced stress (even the noncrisis heuristics and biases described by Kahneman and Tversky) have yet to inform the game-theoretic literature.

Having expressed these reservations, there is much to admire in Powell's book when it is taken on its own terms. It is a study that will set a new standard for modelers, who will appreciate the ingenious formalization of theoretical ideas, the technical mastery, and the careful interpretation of the game solutions.

University of Haifa BEN D. MOR

Power in Contemporary International Politics. By Michael P. Sullivan. Columbia: University of South Carolina Press, 1990. 259p. $32.95.

This book presents a valuable summary, review, and interpretation of recent studies of international relations that impinge directly or indirectly on power issues. Michael Sullivan begins by juxtaposing the realist school of international politics with the transnational—the former operating principally from the perspective of power politics, with the latter originating largely from the principles of political economy. Along the way, he finds (not unexpectedly) that the two perspectives have a number of things in common—not the least of which is an underlying presence of power issues. And that even when the language is couched in other terminology such as interdependence or micropolitics, there is an abiding, if thinly concealed, concern with power politics.

In addition to conclusions such as these (that in themselves constitute a valuable contribution to the ongoing debates in the field of international relations), there are other positive features of the book. These are the individual discussions of major issues of concern to the discipline. Notable among them are the treatments of international polarity, parity versus preponderance, and the measurement of national power.

As we enter the post–Cold War period with its incipient multipolarity, the polarity issue has understandably emerged as a hot topic. The discussion of polarity here highlights the central question, namely, that of the superior stability of bipolarity or multipolarity and the evolution of the international system away from the confrontational bipolarity of the Cold War era. The discussion emphasizes this area as one in which cumulation of knowledge indeed has occurred, in both the clarity with which research questions have been framed and the various answers given.

The problem of polarity versus preponderance also benefits from the treatment here. As the ongoing debate in the literature reveals, the problem admits of no easy answer. Nevertheless, Sullivan's treatment summarizes not only the findings but some of the trends in East–West conflict behavior that bear on the parity-versus-preponderance issue, namely, the increase in tension when parity between East and West was more closely approximated. Indeed, much of the evidence in this book is presented in time series form either as given by the primary researchers or in Sullivan's adaptations. And this is one of the virtues of this volume, that data are presented in ways that clearly reveal trends, where they exist.

Power (that ever elusive concept) is shown to be substantially, although not entirely, susceptible to accurate measurement. Although the measures are crude, they nevertheless have a rough face validity that enhances their usefulness. It is also one of the positive features of this volume that measures of power are compared and are frequently found to arrive at the same answer in rankings of national power.

If all this be true and trends in power, polarity, and conflict behavior were clearly discernible during the Cold War period, why was not the demise of the Cold War predicted by practitioners using these measures? After all, was it not the dismal economic situation in the Soviet Union and Eastern Europe that hastened the demise of communism? The transnationalists then would appear to have the last word. Yet if we examine the measures carefully (as Sullivan does), we see that measures of bipolarity were constantly declining, especially during the 1970s. Even the 1980s did not witness any precipitous increase. This trend is supported by indicators of détente (treaties and agreements between the United States and the Soviet Union) that spiraled upward after the late 1960s and did not appreciably diminish during the 1980s. In others words, these time series clearly indicated the ending of the Cold War that then *allowed* the demise of communism as a form of garrison state economics that simply was no longer required. With the diminution of external threat, the state-run economies of heavy industry in Eastern Europe required to build the heavy weapons of war were no longer necessary. It was against this backdrop that East European countries could be allowed to go their own way and new forms of economic systems could be attempted in these countries and now in the Soviet Union itself. It is a major benefit of Sullivan's work that this collection of time series can yield conclusions such as this. If proponents of this mode of inquiry had taken their own work more seriously, then the ending of the Cold War—if not the actual death of communism—might have been predicted with a fair degree of accuracy.

This book is an ideal choice for graduate classes and even advanced undergraduate courses in international relations research. The literature review is fairly complete and the necessity, as well as the benefits, of precision in such research are made abundantly clear.

Rutgers University, New Brunswick MANUS I. MIDLARKSY

Immigration Reconsidered: History, Sociology, and Politics. Edited by Virginia Yans-McLaughlin. New York: Oxford University Press, 1991. 342p. $39.95 cloth, $14.95 paper.

Refugees without Refuge: Formation and Failed Implementation of U.S. Political Asylum Policy in the 1980s. By Barbara M. Yarnold. Lanham, MD: University Press of America, 1990. 270p. $34.50 cloth, $18.75 paper.

Undocumented Migration to the United States: IRCA and the Experience of the 1980s. Edited by Frank D. Bean, Barry Edmonston, and Jeffrey S. Passel. Washington: Urban Institute, 1990. 292p. $38.75 cloth, $21.75 paper.

After profoundly influencing the character of U.S. society through the early twentieth century, only to be greatly restricted during its middle decades, immigration emerged again in the 1980s as a prominent and emotionally charged public policy issue. Not only was illegal immigration increasingly viewed as a critical problem for the weakened U.S. economy and sovereign integrity, but its link to hotly debated foreign policy goals in Central America broadened the interests drawn into the debate over immigration reform legislation. The 1980s also witnessed the centennial celebration of the Statue of Liberty with all the powerful meaning that symbol holds for our national identity and heritage. These three books examine in a complementary fashion important aspects of the contemporary United States' complicated immigration picture.

The volume edited by Yans-McLaughlin reassesses the historical impact of immigration on U.S. society, questioning the traditional assimilationist interpretation of the United States as a melting pot where immigrants as individuals adopt mainstream cultural and economic traits. Instead, a prominent theme running through the chapters is that structural economic and social mechanisms shape the migratory and adaptation processes. As a result, different groups of immigrants meld into U.S. society and succeed economically to a greater or lesser degree depending on the features and strength of the group networks available to aid their resettlement and employment search.

The factors behind the disparate experiences of various immigrant groups assume greater relevance today as the United States grapples with increased immigration pressures and persistent racial–ethnic tensions. Unfortunately, the volume fails to include any study on recent immigrant groups where the theory regarding the importance of structural constraints and networks is applied to the situation of Indochinese or Central American immigrants. Alejandro Portes's chapter on Hispanics discusses the different economic experiences of Mexicans and Cubans; but none attempts to apply the knowledge gained through historical analysis of immigration to increase understanding of the newest U.S. immigrants.

Other features of the book deserve mention as useful additions to the traditional study of immigration. Apart from the emphasis on structural constraints and networks, the book strives to place the U.S. immigration experience in a broader world context, where it appears less unique than is commonly touted in the traditional immigration literature. For example, Philip D. Curtin's contribution illustrates that the immigration to the United States of African slaves (forced in this case), Europeans, and Asians represented only a fraction of the overall movement of people in the tropical world during the fifteenth to nineteenth centuries. Other contributors examine the economic and political situations in sending countries, thus demonstrating the complex motivations for migration beyond the lure of the United States.

The volume further broadens the study of immigration to include African-Americans. Suzanne W. Model's piece presents an intriguing, if tentative, assessment of the less effective adaptation process of black migrants to northern urban centers as compared to South and East European immigrants. The resultant loss of employment and collapse of the nuclear family continue as two major problems facing African-Americans today.

Overall, *Immigration Reconsidered* represents a valuable contribution to immigration studies reflective of both

contemporary scholarly concerns and U.S. immigration heritage. The other two books, however, delve more directly into contemporary immigration policy issues—the application of the 1980 Refugee Act and the effectiveness and impact of the Immigration Reform and Control Act.

Yarnold's study of the formulation of the Refugee Act of 1980 and its subsequent implementation certainly lacks the engaging narrative of the Yans-McLaughlin volume and, in fact, is almost devoid of the scene setting essential for readers with limited knowledge of the Central American migration experience. The book, instead, concentrates on investigating the variables influencing the passage of the act and its implementation—particularly as it affects asylum applicants, most of whom are from Central America. The array of data compiled and analyzed in the study strongly supports the assertion that U.S. asylum policy remains politically biased despite the intentions of the framers of the act.

Using government data on approval rates for asylum applicants, Yarnold illustrates the persistent bias in favor of persons from countries with governments "hostile" to the United States. She shows that this is the case throughout the application process so long as it remains in the hands of executive branch administrators—whether in the Immigration and Naturalization Service or the Board of Immigration Appeals. Only when cases reach the federal courts on appeal does this bias disappear; and then, Yarnold finds, other "political biases" influence the decisions. For example, judges appointed by Democratic presidents tended to rule more often in favor of asylum applicants regardless of their country of origin.

Yarnold also investigates the role of public interest organizations in the passage and implementation of the act. She examines various aspects of these types of organizations, including their ability to overcome the free-rider problem, their role in bolstering social movements, and their importance in facilitating successful asylum applications. Generally, this part of the study has less to offer than the data analysis concerning the implementation of the Refugee Act. Those results provide valuable insights into the workings of the asylum bureaucracy and the federal courts. They illustrate the ability of administrators to shape policy independently of other actors. The results also arm those critical of the political bias in the U.S. asylum policy with additional evidence.

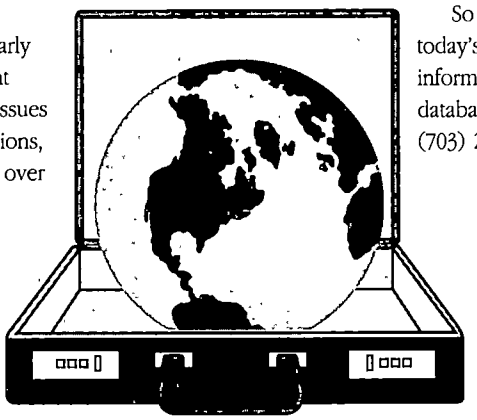
In contrast to Yarnold's book, the volume edited by Bean, Edmonston, and Passel includes an introductory background discussion of different types of illegal immigrants and the difficulties in determining their numbers. This discussion is useful to anyone seeking a better understanding of the illegal immigration issue.

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The introduction identifies the various types of illegal immigrants, differentiating those who intend to stay (settlers) from those who come to work with the intention of returning (sojourners) and those who cross on a daily basis to work (commuters). These distinctions become crucial when trying to calculate the number of illegal immigrants entering the country (the flow) and those residing in the country at any one time (the stock). The editors point out that the failure to think of the flow, as distinct from the stock, of illegal immigrants wreaked havoc on the policy debates of the 1970s and early 1980s.

After clarifying these conceptions of illegal immigrants, the remainder of the book attempts to gauge the effectiveness of the Immigration Reform and Control Act of 1986. The act had the two goals of decreasing the stock of illegal immigrants through an amnesty provision and curbing the entry of additional illegal immigrants through employer sanctions and increased enforcement. Five of the studies seek to measure either the change in the stock of illegal immigrants or the flow of additional entrants after the passage of the act. Two studies try to ascertain changes in the perceptions and intentions of potential migrants still in Mexico. The collection represents both a timely public policy study and a challenging endeavor into a problematic area of research and data analysis. The book is a must for those involved in demographic studies calculating the size of illegal immigrant populations.

Virginia Commonwealth University SUZANNE L. FIEDERLEIN

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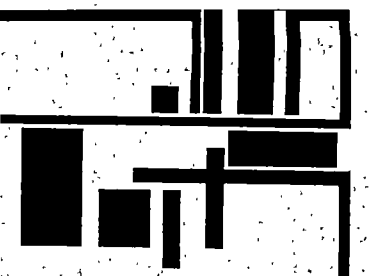
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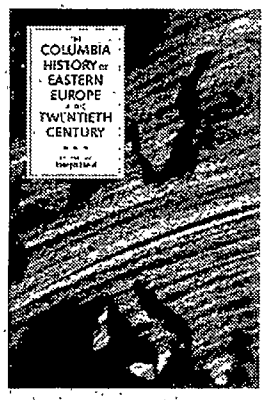
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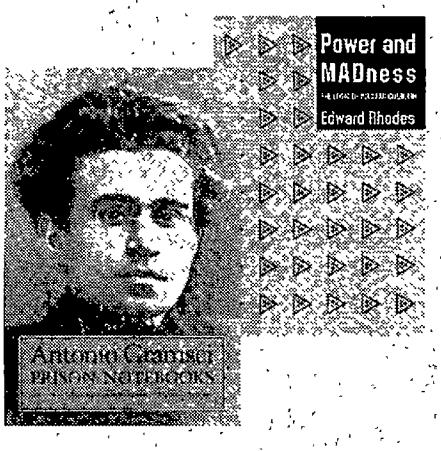
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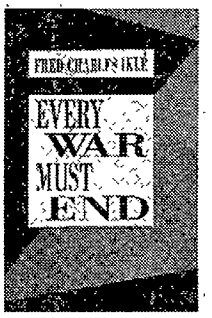
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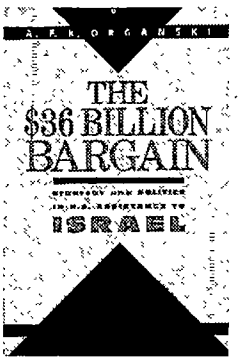
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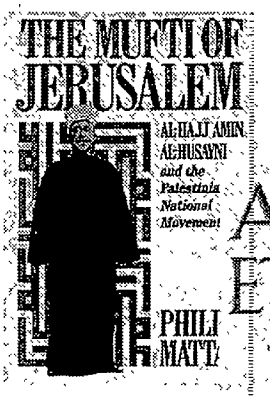
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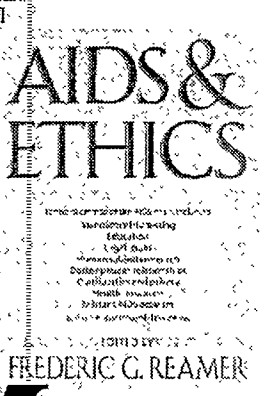
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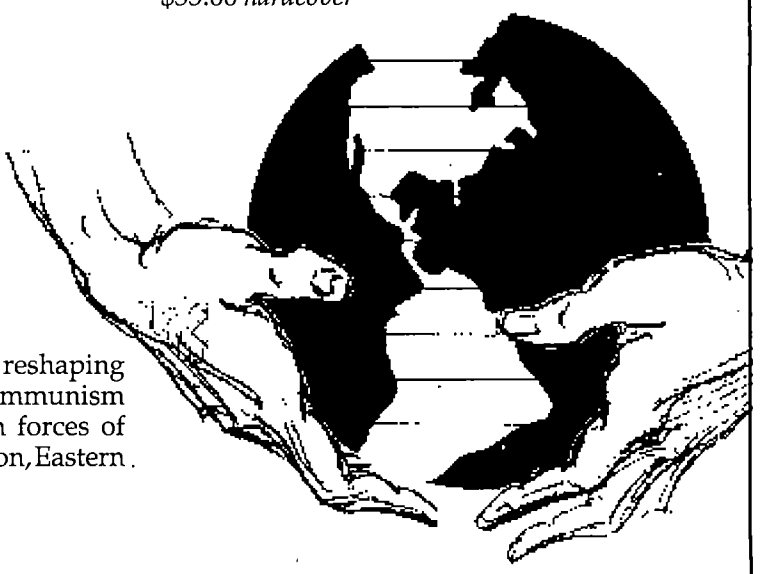
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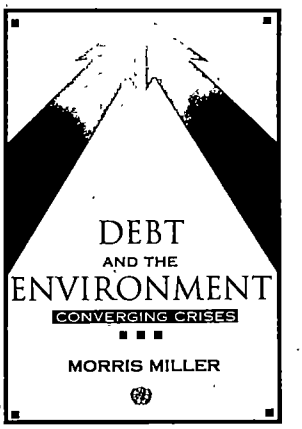
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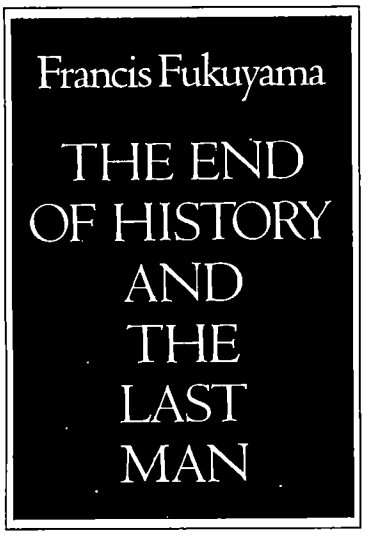
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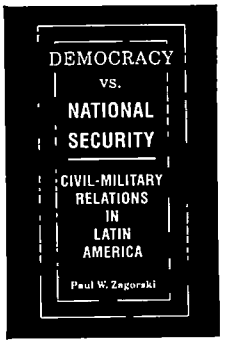
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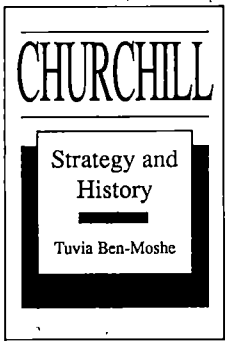
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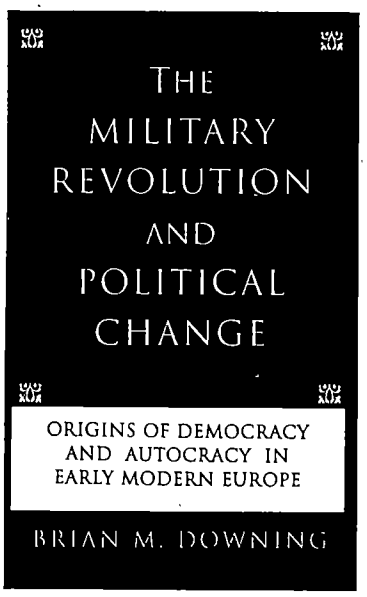
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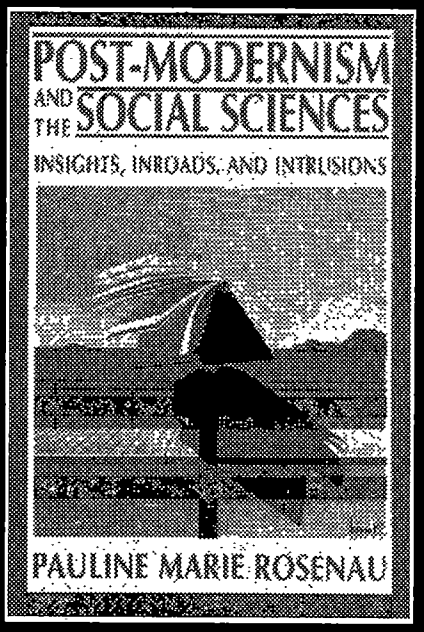
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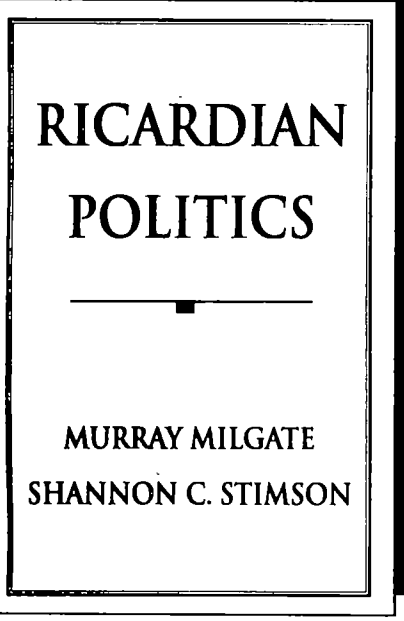
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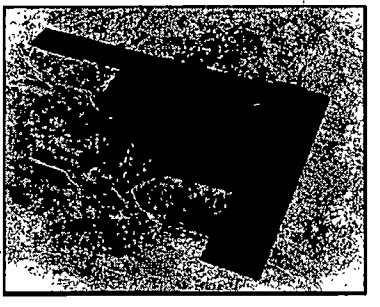
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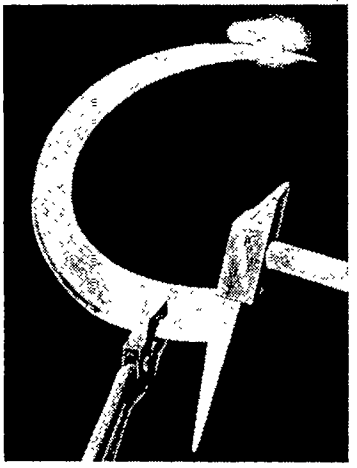
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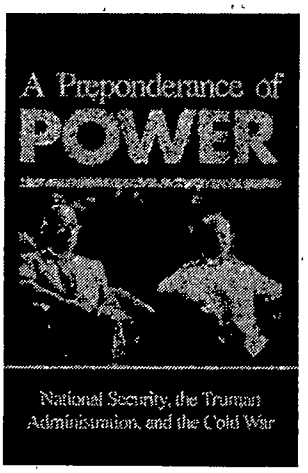
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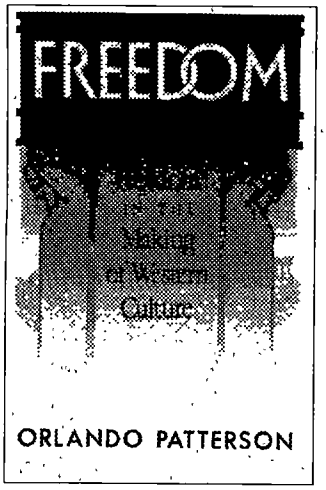
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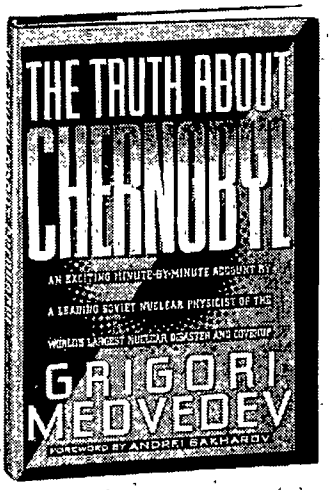


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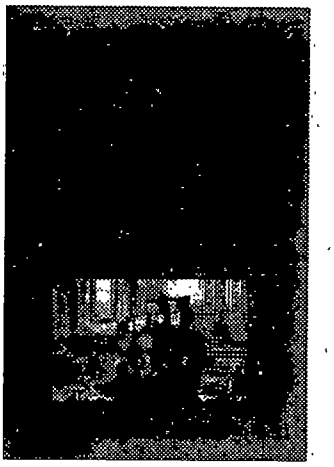
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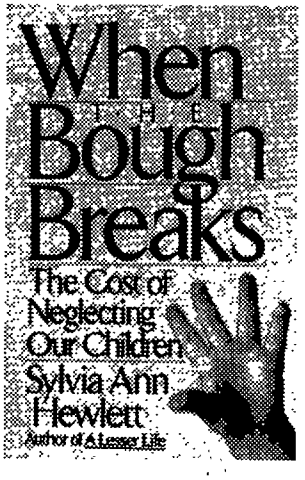
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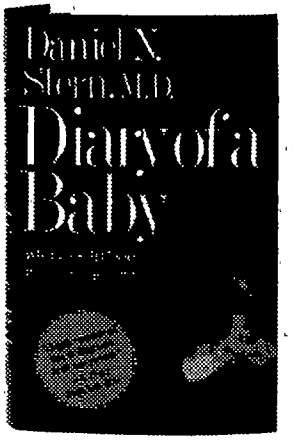
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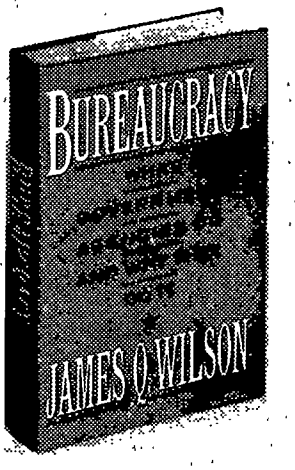
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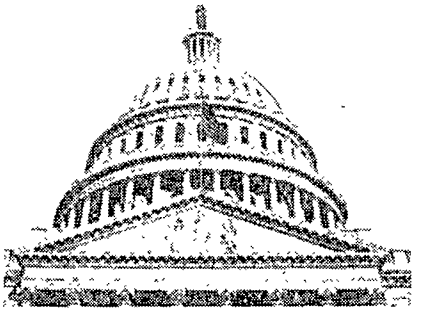
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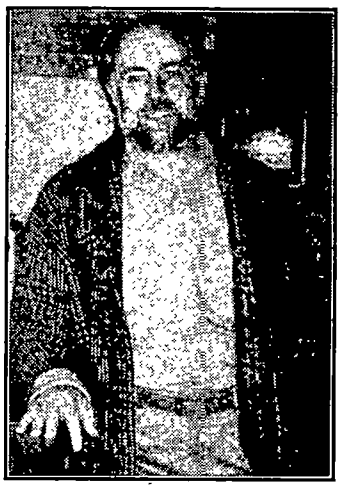
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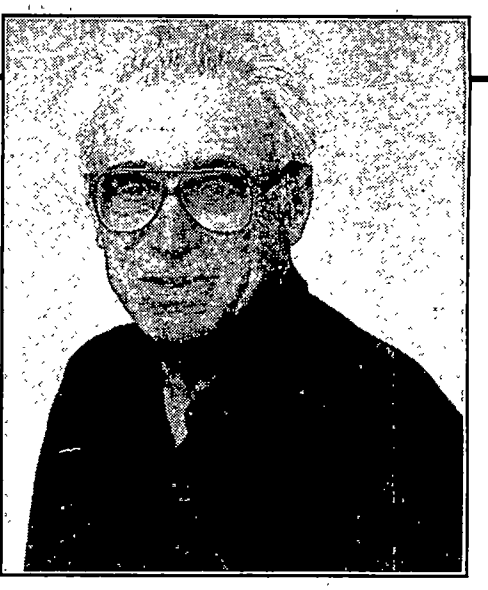
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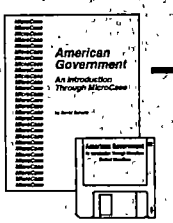
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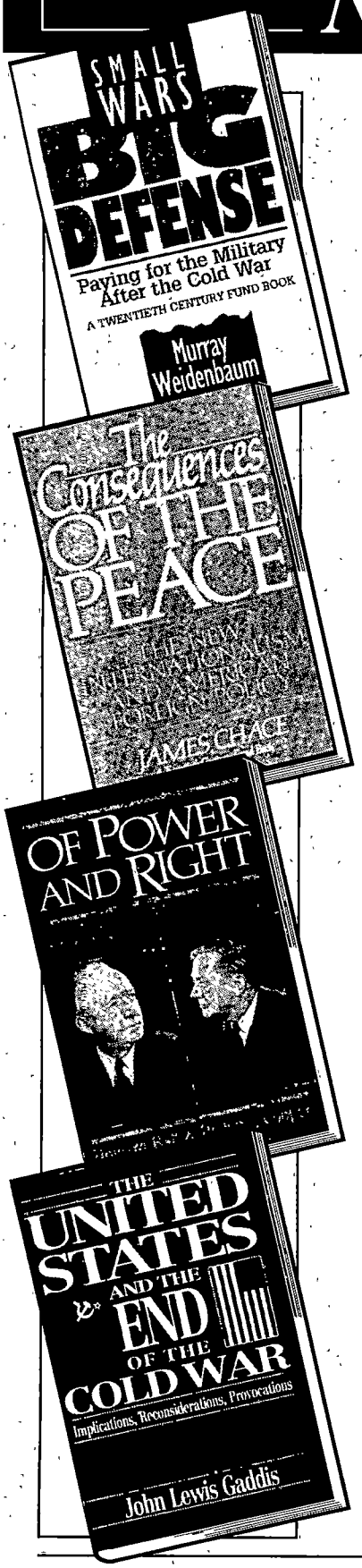
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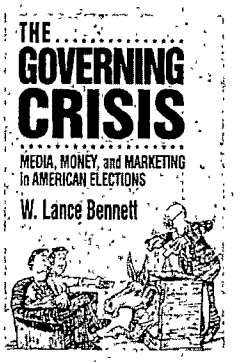
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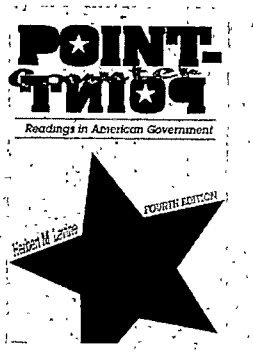


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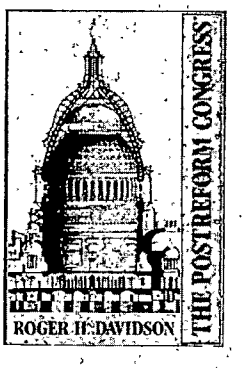


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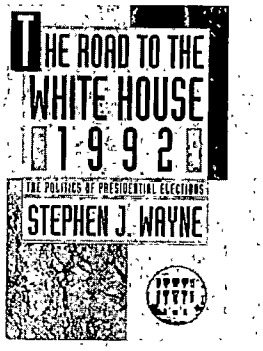


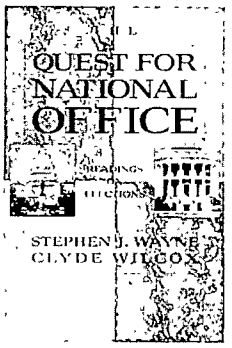
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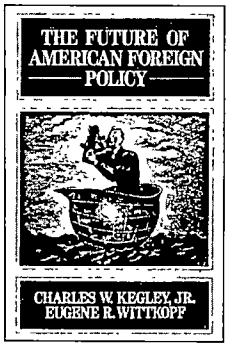
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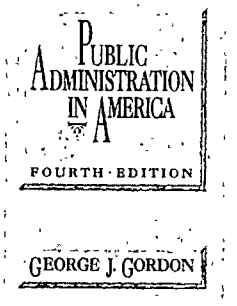
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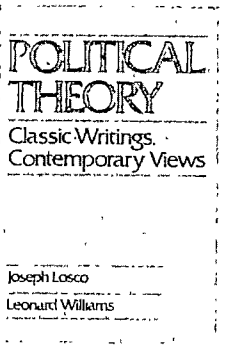
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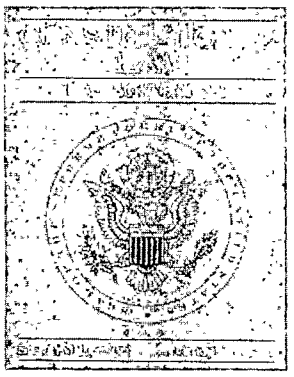
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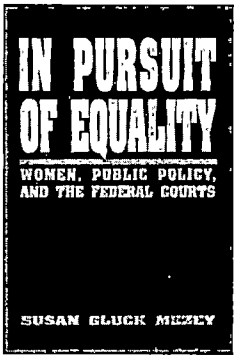
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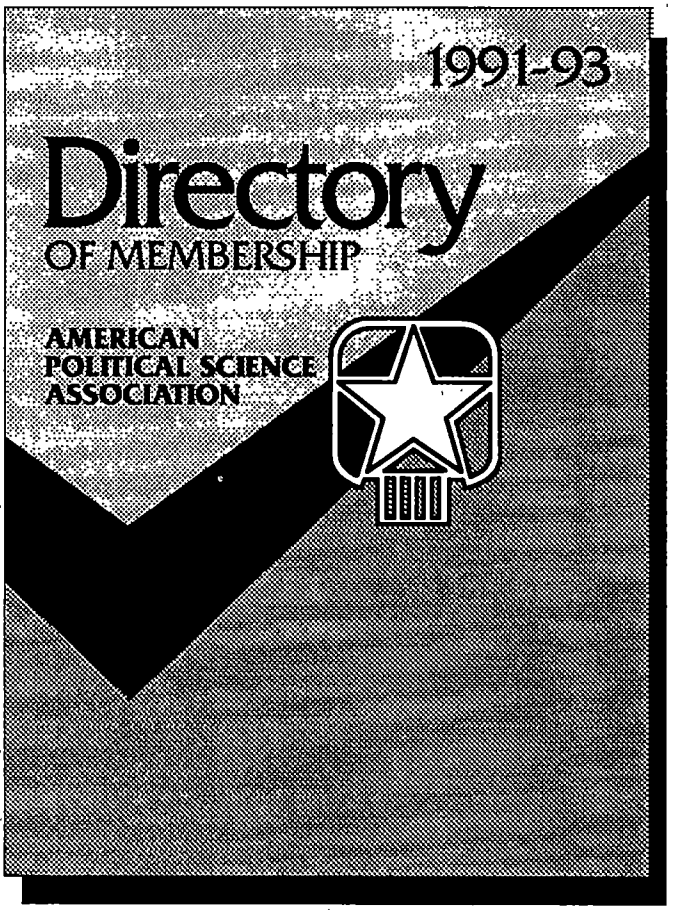
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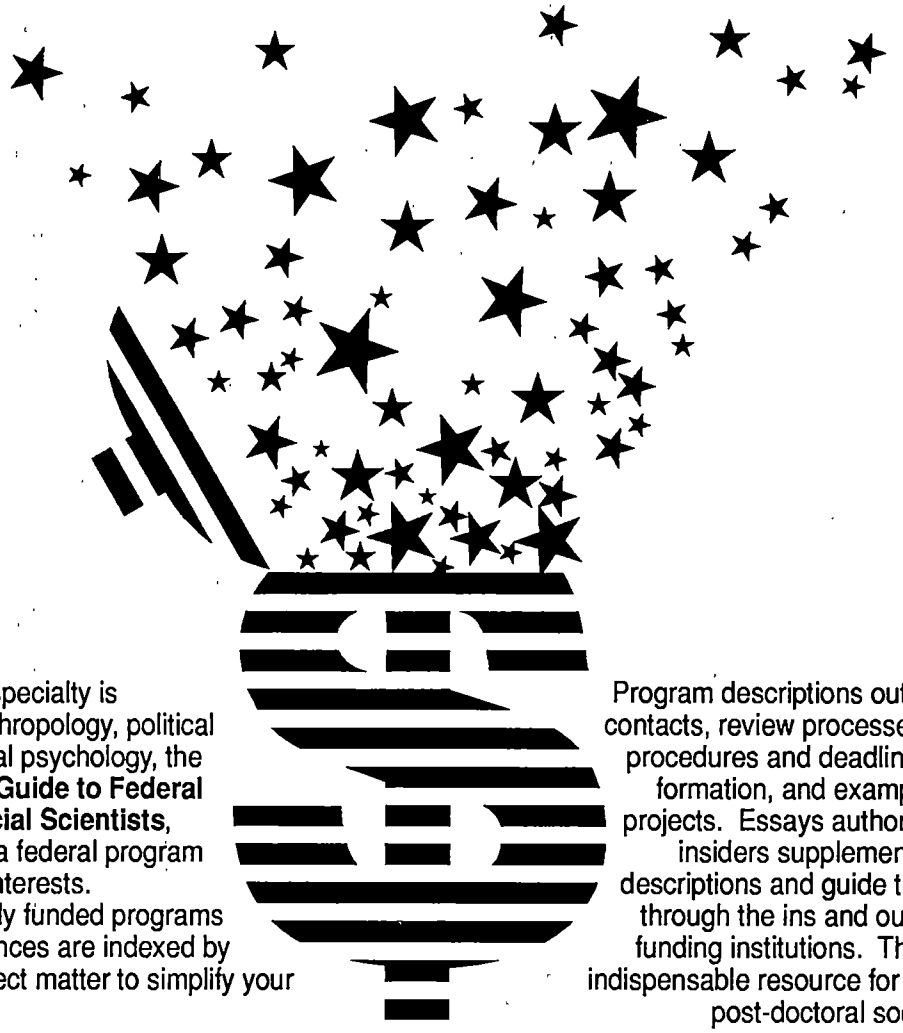
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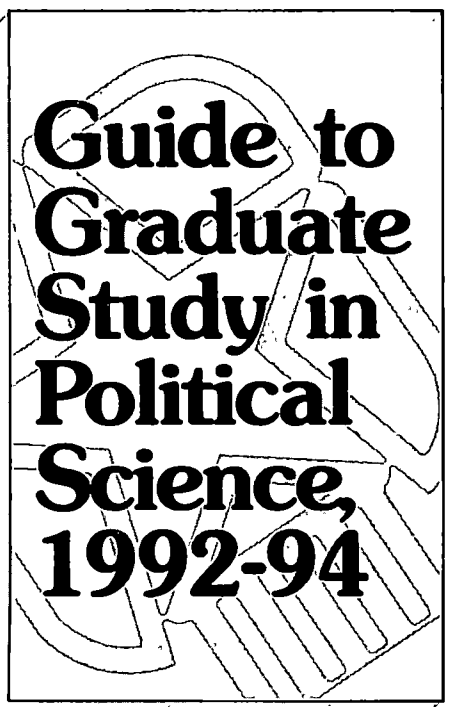
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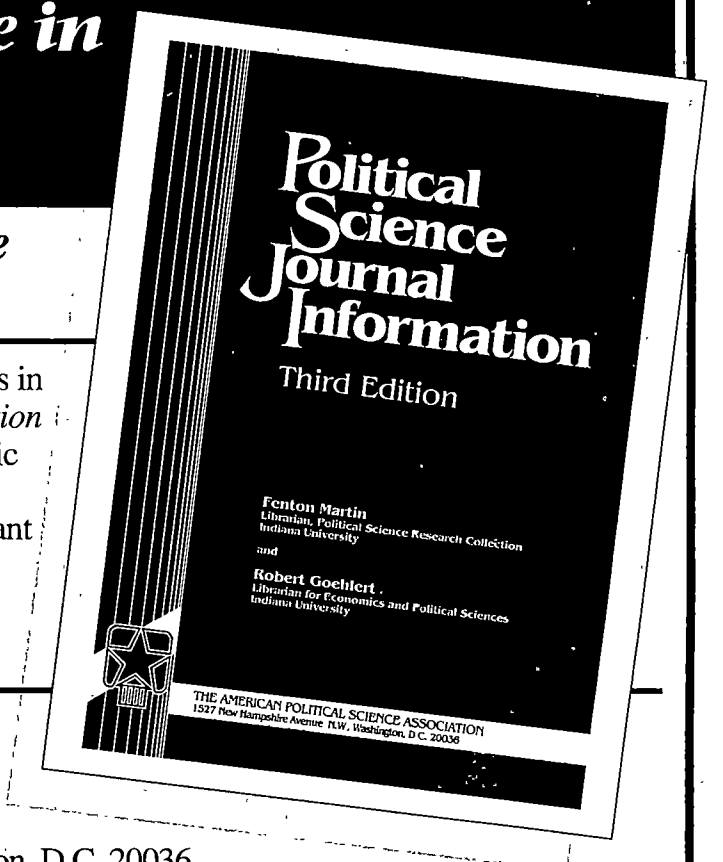
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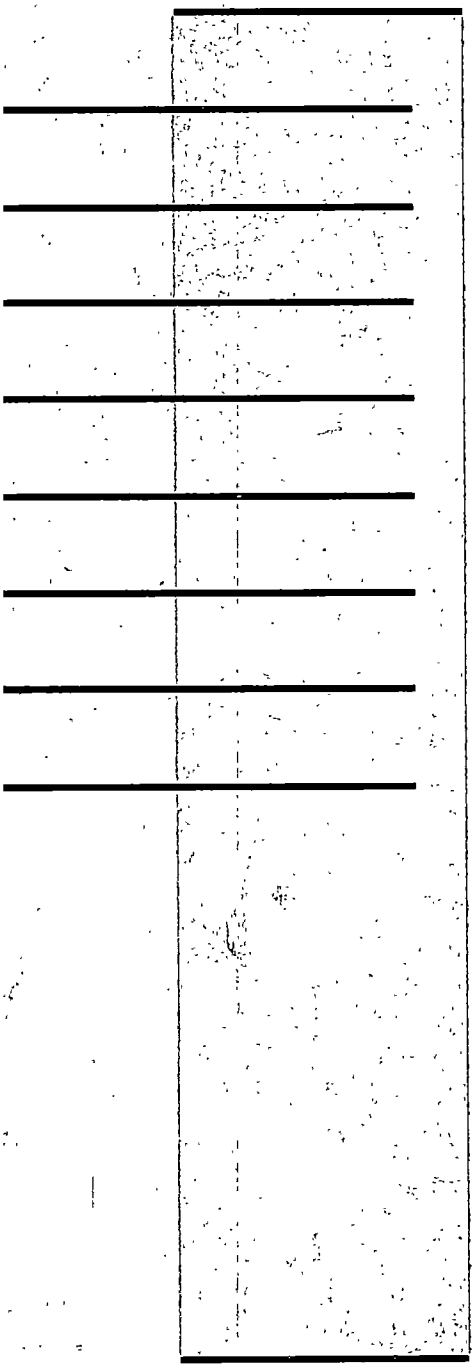
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ARTICLES

RETHINKING ALLISON'S MODELS

JONATHAN BENDOR *Stanford University*
THOMAS H. HAMMOND *Michigan State University*

The ideas in Graham Allison's Essence of Decision *(1971) have had an enormous impact on the study and teaching of bureaucracy and foreign policy making. While Allison's work has received considerable critical attention, there has been surprisingly little examination of the content and internal logic of his models. We subject each of Allison's three models to a systematic critical analysis. Our conclusion is that the models require substantial reformulation.*

Graham Allison's study of the 1962 Cuban missile crisis, published as "Conceptual Models and the Cuban Missile Crisis" (1969) and as *Essence of Decision: Explaining the Cuban Missile Crisis* (1971), is a landmark in our discipline's analysis of bureaucracy's role in foreign policy making. Prior to Allison's work, most studies of bureaucracy and foreign policy, though rich and informative, were largely descriptive and rather unfocused theoretically. They seldom presented an explicit analytical perspective, and readers were often left to infer what the authors believed were the fundamental properties of bureaucratic policymaking.

Allison's work demonstrated that a more self-consciously theoretical approach to the study of bureaucracy and foreign policy was both feasible and desirable. The earlier literature had rarely developed alternative explanations for events and decisions. But in formulating his three models—the *rational actor, organizational process,* and *governmental politics* models, labeled I, II, and III, respectively—Allison showed how to make explicit the explanations of events that had only been implicit in many of these earlier studies. In so doing, Allison helped to place the study of bureaucracy's influence on foreign policy on a more scientific foundation.

Allison's work received high praise in reviews by a wide range of scholars such as Ole Holsti (1972), Francis Rourke (1972), and Harrison Wagner (1974). For example, as Wagner remarked in his review of *Essence of Decision*:

> It may, in fact, be the most generally persuasive attempt ever made to show the relevance of theorizing to the treatment of what are ordinarily thought to be the main problems of the study of international politics. Its general appeal is primarily due to the effectiveness of Allison's theoretical arguments in his analysis of the Cuban missile crisis, which contains a number of striking insights and some information that no one else had thought to look for. The results are a much more complex analysis of that event than anyone else has offered and a radically different understanding of what happened from the one most people had previously adhered to. To the extent that these considerable virtues are the direct result of Allison's reflections on the theoretical literature mentioned, they seem to be the product of his efforts to make these three paradigms as explicit as possible, enabling him to derive as much insight from the assumptions of each as he could, and to develop alternative explanations for this event, which had the heuristic effect of leading him to look for possibilities that had not occurred to analysts more nearly the prisoners of a single, and poorly articulated, set of assumptions. These are, I think, among the important benefits to be derived from explicit theory construction, and Allison's example is therefore one well worth taking seriously. (pp. 446–47)

As predicted in the reviews, Allison's approach has indeed had a substantial impact on both the study and the teaching of bureaucracy. His three models have been widely applied, and other scholars have followed his lead by developing additional models to account for aspects of policymaking left unexplained by Allison's analyses.[1] Moreover, Allison's book is still widely used in courses on bureaucracy, public administration, and organization theory. We have frequently used it in our own classes; one of us (Hammond) is alone responsible for the sale of several hundred copies to students in the past 15 years. As Holsti's review suggested, the book is so useful because it shows students how to formulate and evaluate alternative explanations for political events (1972, 137). Indeed, for courses dealing with bureaucracy and foreign policy there are few alternatives to *Essence of Decision*. Even now, over 20 years later, the book remains heavily cited by scholars in a remarkably wide range of disciplines.[2]

Allison's work did attract considerable critical attention from the outset (e.g., Art 1973; Ball 1974; Bobrow 1972; Caldwell 1977; Cornford 1974; Freedman 1976; Krasner 1972; Perlmutter 1974; and Yanarella 1975). Even those praising the book (e.g., Holsti 1972; Rourke 1972; and Wagner 1974) also expressed reservations. However, none of these early criticisms comprehensively examined the internal logic of all three of Allison's models, and it is precisely the logical structure of his work that is particularly beginning to show its age.

With the advantage of two decades of hindsight, much more is known about the properties of models of bureaucracy and policymaking. It is now clear that alternative versions of Allison's models could and should be developed to highlight somewhat more fundamental factors in policymaking. Since Allison's work still has wide currency in our discipline, the

time is ripe for a critical analysis of his models and arguments.

We make five arguments about Allison's work. First, while a key purpose of modeling is to force the analyst to clarify the assumptions on which the analysis will be based, it is often difficult to determine for Allison's models just what the assumptions are. If one cannot determine what is driving a model, its explanatory power cannot be accurately assessed. Second, the hypotheses drawn from a model should, ideally, be logically derived from the initial assumptions. But many of Allison's hypotheses seem, at best, only loosely related to the models' initial assumptions. Third, whatever the logical status of Allison's propositions (are they rigorously derived from his assumptions, or are they freestanding conjectures?), it is possible to demonstrate, on strictly logical grounds, that several key propositions in Model II are simply incorrect.

Fourth, a well-crafted model must strike a balance between simplicity and complexity. Too simple a model misses key aspects of the problem one is trying to understand; too complex a model is analytically intractable and yields few testable hypotheses. Of course, what is too simple and what is too complex is partly a matter of what analytical technology is available. Nonetheless, even given what was available 20 years ago, we think that it is fair to say that Allison's Model I is (and was) much too simple. On the other hand, we will also argue that Model III is, in some ways, too complex. Finally, Allison's Models I, II, and III are based on three bodies of literature (rational choice theory, organization theory, and bureaucratic politics, respectively) that only specialists in these fields know well. Specialists in other fields, such as foreign policy making, have often used *Essence of Decision* as an introduction and guide to these literatures. Unfortunately, Allison misinterpreted parts of each of these three fields. In some cases, the misinterpretations were, in our judgment, quite substantial. Thus, unsuspecting readers may be led into a double error—believing that the models are better constructed than they are and that they accurately reflect the literatures on which they are based.

We examine the various sets of assumptions on which the models are based. These sets of assumptions will be used to develop a typology of policymaking models, thus providing a context for analyzing the models in *Essence of Decision*. We then probe the logic of the three models and conclude by discussing some further implications of our arguments.

We should emphasize that our general purpose here is to examine the *theoretical* aspects of Allison's work. There is, to be sure, a large and growing empirical literature on the Cuban missile crisis. Some of this literature is quite new, drawing on evidence only recently made available to the public (see Allyn, Blight, and Welch 1989/90; Blight, Nye, and Welch 1987; Bouchard 1991, ch. 4; Brugioni 1991; Garthoff 1988, 1989; Hampson 1984/85; Lukas 1987; Trachtenberg 1985; and Welch and Blight 1987/88). But for the most part, we will not address empirical issues raised by Allison's analysis.

A TYPOLOGY OF MODELS

Before plunging into a detailed analysis of Allison's three models, it is advisable to distinguish one from another by defining their key properties. As several early critics of Allison's work noted, however, this is more difficult than one might think (see, e.g., Cornford 1974, 241; Wagner 1974). It seemed particularly difficult to disentangle Models II and III, Wagner arguing that "it is not entirely clear whether Model III is independent of Model II or an extension of it; certainly bureaucratic bargaining seems to be constrained by many of the factors discussed under Model II, and many of Allison's readers seem to mingle the two together in speaking of the lessons of his book" (1974, 448). Hence, we will begin by asking a very general and deceptively simple question about Allison's three models: On what assumptions is each one based?

Four different kinds of assumptions appear to be important, and they lead to a typology of models of policymaking useful for characterizing Allison's three models. The first class of assumptions concerns the number of actors. The classical approach to the study of international relations postulates that the government of a sovereign state behaves as if it were a unitary actor. Allison's contrasting approach, exemplified by his Models II and III, is to decompose the government into multiple actors. Note, however, that a shift to multiple actors immediately requires specification of whether the multiple actors have the same or conflicting goals. Very different models—and very different understandings of policymaking—follow from different assumptions here. Though Allison is less explicit here than he might be, he appears to assume in both Models II and II that the actors' goals are in conflict. But even if actors have the same objectives, this does not mean that policymaking is a trivial problem. As any student of bureaucracy knows, even when there is no serious conflict over goals, coordinating the actions of a large number of executive branch actors is no easy task. Thus, a model could logically—and quite meaningfully—stipulate multiple actors whose objectives are the same. Hence, when we consider the number of actors posited by a model, a second type of assumption, namely, whether or not the actors' goals are the same, is necessarily involved. If a central goal of *Essence of Decision* is to understand the relative virtues of government-as-single-actor versus government-as-multiple-actors models, the results of the comparison will be misleading if this second kind of variable is neglected.

The third class of assumptions concerns the degree of rationality attributed to decision makers. As Allison stresses, the classical approach assumed that decision makers are rational. In contrast, students of behavioral organization theory like Herbert Simon

Figure 1

A Typology of Policymaking

	DECISIONMAKERS ARE: Perfectly rational		Imperfectly rational	
Single Decisionmaker	1a complete information	1b incomplete information	2a complete information	2b incomplete information
Many Decisionmakers, same goals	3a complete information	3b incomplete information	4a complete information	4b incomplete information
Many Decisionmakers, conflicting goals	5a complete information	5b incomplete information	6a complete information	6b incomplete information

and James March, as well as political psychologists like Robert Jervis and John Steinbruner, maintain that all agents have limits to their cognitive capacities. Though there is a continuum of possibilities here, we can simplify by dichotomizing. A model could stipulate either that agents are perfectly or imperfectly rational.

Finally, a model may assume that decision makers are perfectly rational yet also postulate that they are imperfectly informed, either about which outcomes will occur (prospective uncertainty) or about what has already transpired (retrospective uncertainty).[3] It seems intuitively obvious—and here explicit theorizing supports intuition—that either informational problem can strongly affect the predictions of models. Thus, the fourth kind of assumption concerns the amount of information attributed to decision makers. We simplify by assuming that with "complete information" there is neither retrospective nor prospective uncertainty. Thus, all players know the structure of the game (including payoffs), who has done what in the past, and the outcome that will result from any set of moves. With "incomplete information" there can be either retrospective or prospective uncertainty, or both.

These four classes of assumptions can be combined to yield the typology of models in Figure 1. The typology will help us identify various possible models of the policy process. To serve this purpose, a typology's categories should be mutually exclusive (overlapping categories make identification uncertain) and collectively exhaustive, so that any candidate can be "typed" as an instance of some class. A quick inspection of this typology reveals that the cells are indeed mutually exclusive and collectively exhaustive.

With this typology we can now attempt to characterize Allison's three models. Model I, the rational actor model, clearly belongs in cell 1. Indeed, the overwhelming bulk of Allison's discussion in *Essence of Decision* suggests placement in cell 1a, with its assumption of a single, perfectly rational actor with complete information. We will soon argue, however, that the complete information assumption neglects what international relations theorists have identified as a central aspect of their field, namely, actors' pervasive uncertainty about the capabilities, motives, and actions of others. Considerable insight, in other words, is to be gained by extending Model I into cell 1b, the realm of incomplete information. Further insight into the workings of single-actor models could be obtained by relaxing the assumption of perfect rationality, thus moving into cell 2. As examples we might cite psychological analyses of individual bounded rationality in foreign policy decision making (e.g., George 1980, chaps. 2–3; Jervis 1976).

At first glance, it might seem that Model II, the organizational process model, belongs in cell 4b. After all, boundedly rational actors, facing uncertain environments and using simple adaptive strategies, are the heart of this model. However, at several places in his discussion of Model II Allison mentions conflicting goals. For example, he refers to "parochial priorities," the "quasi-resolution of conflict," and organizational "imperialism" (1971, 81–82, 93). He even refers to agenda manipulation: "The short list of alternatives [generated by an organization] reflects not only the cost of alternative generation but, more important, each organization's interest in controlling, rather than presenting, choices—for example, by serving up one real alternative framed by two extremes" (p. 90). One can also find, in his discussion

of Cyert and March (1963), references to organizations as a "coalition of participants" and "bargaining among potential coalition members" (p. 76). Thus, Model II might appear more properly to belong somewhere else, perhaps in cell 6.

As an empirical observation, an assumption of conflicting goals in an organizational process model is unassailable. For most important issues (virtually by definition of *important*), organizations and their leaders disagree about objectives. However, we do think that some interesting hypotheses—particularly about the kinds of problems involving bureaucratic coordination that Allison discusses in regard to Model II—could be generated by "withdrawing" into cell 3. This class of models is ideal for examining pure coordination problems among perfectly rational actors. Anyone who thinks that pure coordination problems are trivial should consider Schelling's examples, which only scratch the surface of coordination problems (1960, 54–58). If different members of a team observe different aspects of the environment and these environmental signals are noisy, formidable communication and division-of-labor problems typically arise.[4] Even in the context of goal consensus and perfect rationality (i.e., each agent, like a perfect Bayesian statistician, extracts the maximal amount of information from each noisy observation), there remain structural issues of who should communicate what to whom.[5] If we make the problem dynamic (e.g., involving optimal adaptation to a changing environment), the complexities multiply—even for actors with the same goals. Understanding of these particular issues is probably hindered, not helped, by construction of models involving conflicting goals.

The heart of Model III is multiple decision makers with conflicting goals—hence the third line of the typology. However, as Harrison Wagner pointed out, it is unclear whether this model assumes perfect or bounded rationality: "Model III . . . contains no discussion of behavioral assumptions at all. Yet one could presumably construct a theory of bureaucratic bargaining based on the decision theorists' assumptions, and another based on Herbert Simon's" (1974, 448).

Allison's own summary of the core of Model III (1971, 162) emphasizes bargaining and other obviously political processes as central to policymaking and deemphasizes issues involving information processing. Moreover, his discussion of power involves actors who are apparently able to make complex strategic calculations about how to maximize their influence (pp. 168–69). These actors thus seem to have the cognitive capacity usually assumed by rational choice models of policymaking, so that readers might conclude that classical rationality is presumed for Model III, leading to its placement in cell 5.[6] Moreover, since Allison explicitly states that bureaucratic politics games "are not played under conditions of perfect information" (p. 178), Model III seems to belong in cell 5b.

However, other of Allison's statements about Model III suggest a different classification. For example, Allison refers to foul-ups ("e.g., choices that are not made because they are not recognized or are raised too late, misunderstandings, etc." [pp. 145–46]); the limited attentional capacities of decision makers (pp. 176–78); and problems of misperception, misexpectation, and miscommunication (pp. 178–79). Of incrementalism in decision making, he writes "Analysis is drastically limited. Important policy outcomes are neglected. . . . By proceeding incrementally and comparing the results of each new policy with the old, actors reduce or eliminate reliance on theory" (p. 154). All these characteristics bear a strong family resemblance to the decision processes of ordinary, imperfectly rational humans.

In terms of our typology, then, Model II (which initially might seem to belong in cell 4, probably 4b) can also be classified in cell 6, perhaps even 6b. Model III, which might seem to belong somewhere in cell 5 (probably 5b) can also be classified in cell 6, perhaps even 6b. Thus, we find that Models II and III, which have different intellectual pedigrees and which Allison presumably intended to be distinct, apparently share much of the same analytical turf.

We can see at the outset, then, that our simple typology raises some serious questions about just what is driving each of Allison's models. We do believe that Allison's initial instincts were on the right track. As our typology suggests, there is room for several different classes of models, and it is an essential enterprise to assess the relative explanatory and interpretative power of models in each of these different classes. It is necessary, however, to specify much more precisely what is assumed for each of these models. For this reason, Allison's subsequent enterprise of *combining* Models II and III into one overarching *bureaucratic politics* model (see Allison and Halperin 1972) is probably a step in the wrong direction. Combining two theories before clarifying their defining properties can easily produce inconsistent claims (Landau 1972, 227). Hence, we are inclined to follow Cornford's (1974) lead in arguing that there are virtues in separating Models II and III and in clarifying their internal logic prior to synthesizing.

We close by noting that identifying a particular model as belonging, say, to cell 1a does not necessarily yield a detailed prediction about what policy outcomes to expect from the model.[7] Such an identification does have certain implications—for example, that the relevant government will not encounter surprises (by virtue of the complete information assumption) and will exhibit transitive preferences (the unitary actor and rationality assumptions). But such predictions fall well short of the kind of policy-relevant claims naturally sought by students of foreign policy. The burden of generating falsifiable predictions lies on the models, not on the typology.

ANALYSIS OF MODEL I

Allison's analysis of rational decision making is the most coherent and most easily understood of his

models. A rational actor model in international relations should have two essential components: a decision-theoretic one and a game-theoretic one. The former covers attributes of the decision maker(s), the latter, strategic interactions. We have two major points to make about Allison's rational actor model. First, the decision-theoretic component is too simple: one can easily construct a far more complex rational actor that better reflects the complexities of real decision makers and the choice situations they confront. Second, the model neglects strategic interactions.

Before examining the details of Model I, we must discuss one difficulty with analyzing Allison's treatment of the model: his tendency to distance himself from his own presentation. As he notes "Since the purpose of the Model I account is to present a strong, typical explanation, it seems fair to let the account reflect only conventional evidence plus additional facts that the Model itself would naturally uncover" (1971, 248). This raises the problem whether one should critique (1) the classical version of single-actor rational choice that scholars have traditionally used, (2) Allison's interpretation of this standard model, or (3) what Allison himself thinks this model should look like if (given its premises) one developed and applied it "properly." We are inclined to think that approach 1 or 2 best characterizes Allison's treatment; Allison never tells the reader what he thinks Model I would be like if properly developed. This matters because we think a more fully developed version of Model I would have substantially more explanatory power than Allison's Model I. For simplicity, we will treat the model as Allison's own and criticize it as such, understanding that it has a different status in Allison's thought than his Models II and III.[8]

Decision-theoretic Aspects

Model I employs two key decision-theoretic assumptions about nation–states, postulates found in most canonical formulations in international relations. First, the nation can be treated as a single actor endowed with a set of goals and the capacity for pursuing these goals. Second, this single actor pursues these goals rationally. Though it is easy to criticize the assumption that the nation is a single actor, many abstract analyses require treating an aggregation of human beings as a single unit. For some purposes such aggregation is useful and appropriate. We will, instead, focus our attention on Allison's treatment of rationality.

In common parlance and in Allison's usage, rational decision making consists of four key steps: define one's goals, list all options for achieving these goals, evaluate each option in terms of the extent to which it achieves the goals, and choose the option that best achieves the goals. As Allison points out, students of international politics and foreign policy have long used this model (if only implicitly). He demonstrates how the model might be used to make sense of three major decisions in the missile crisis: why the Soviet Union placed the missiles in Cuba in the first place, why the United States responded with a naval quarantine, and why the Soviet Union withdrew its missiles.

Allison's rational actor model is as simple a version of rational choice as one could imagine. His purpose in formulating this model was to show how something much like it underlies many different bodies of thought in international relations. But in this particular enterprise lies what we see as the model's major fault. It is *too* simple, for it omits several significant properties of rational actors and important problems they face. The model can be enriched in several different ways.

Goals. An ambiguous aspect of Model I is whether rational actors are presumed to have just one goal. Before explicating the model, Allison notes that the Model I theorist "presents an argument for *one objective* that permits interpretation of the details of Soviet behavior as a value-maximizing choice" (1971, 11; emphasis added). Later he discusses how this analyst "produced an argument for *one goal* (rectifying the nuclear balance) that made the Soviet emplacement plausible" and "proceeded as if his assignment had been: make a powerful argument *for one objective* that permits the reader to see how, given the strategic problem, if he had been playing the Soviet hand, he would have chosen that action" (pp. 246, 253; emphasis added). And in applying Model I to the Soviets' decision to install the missiles, Allison's discussion appears largely oriented toward inferring which *one* of the five possible goals best explained the Soviets' decision. For example, in ending his discussion of Cuban defense as a possible motive, he concludes, "Cuban defense might have been a subsidiary effect of the Soviet gamble, but not its overriding objective" (p. 50).

Yet in discussing the international relations theorists Hans Morgenthau and Raymond Aron, Allison notes in particular, "Criticizing the attempts of theorists such as Morgenthau to explain national action by reference to a single goal, Aron argues that governments pursue a spectrum of goals, tempered by 'the risk of war [that] obliges [them] to calculate forces or means' " (1971, 27). And in his presentation of Model I, Allison seemingly allows the possibility that a rational actor might have multiple goals: "National security and national interests are the principal categories in which strategic goals are conceived. Nations seek security and a range of other objectives" (p. 33).

Although Allison does not explicitly discuss this matter of multiple goals, it raises important issues in understanding and applying Model I. Nothing whatsoever in the theory of rational action requires an actor to have just one goal. This is a significant feature of the theory, for empirically, an actor with only one goal is odd indeed. In fact, we normally use the term *monomaniacal* to refer to the *irrationality* of people motivated by only one objective. Typically, we expect rational actors to have multiple goals. Indeed, nothing in the axioms of rational action states

that it is nonsensical to harbor multiple goals. The goals may conflict. But in a rational choice model a utility function is postulated to resolve all *intra*personal conflict.[9] Thus, rationality lies in the considered and consistent pursuit of whatever goals one happens to have.

We can support our arguments by reference to the Soviet Union's possible motives for placing nuclear missiles in Cuba. Presumably there was, as Allison concluded, a strong concern for the strategic balance. The recent U.S.–Soviet conferences tend to bear this out (Blight and Welch 1989). However, it is not unreasonable to think that the Soviets were also concerned about defending Cuba. The Bay of Pigs invasion had given the Soviets (and the Cubans) cause for concern. Moreover, the Kremlin would have been pleased if placing missiles in Cuba would have diminished the prestige of the United States in the eyes of West Europeans and nonaligned countries.

The Soviets' concern for the strategic balance may well have been of overriding importance; other concerns may have been of negligible significance in their minds. Nonetheless, it is reasonable to think that the Soviets would have been less inclined to place the missiles in Cuba if this action would have clearly *impaired* these other goals. Thus, these other goals cannot be considered irrelevant to an understanding of the Soviets' motives and choices.

For each of these other goals, Allison may be correct in arguing that there was some other action better than placing nuclear missiles in Cuba. However, the bare fact that the chosen action is not *optimal* for the other goals (such as defending Cuba) does not imply that these other goals were completely irrelevant to the decision.

Time. Allison's rational actor model is characterized by a single-time-period orientation. Thus, the rational actor considers a problem occurring at a point in time; and once an alternative is chosen and implemented, the matter is over. Allison himself mentions this feature as a limitation of the model, labeling it "static selection."

One might argue that a single-time-period model is appropriate for crises. However, even in a crisis, the effects of options may be spread over multiple time periods. Consider, for example, President Kennedy's concern for the United States' international reputation for firmness and resolve: such reputational consequences may endure for years. Thus, in estimating the effects of any option, a rational agent must aggregate, into a single measure, costs and benefits that may be spread over a long time. Aggregating streams of costs and benefits is a rather different kind of choice problem than aggregating costs and benefits that all occur at the same time.

Incorporating multiple time periods in a model of rational choice leads directly to the issue of time preferences. Is the rational actor patient, valuing distant benefits nearly as much as current ones, or impatient, sharply discounting benefits that are enjoyed long after the moment of choice? Note that actors with identical goals but different time preferences would value the same option differently, due to their different degrees of impatience.

Uncertainty. Allison's rational actor model also ignores the problem of uncertainty. While Allison obliquely considers uncertainty in his discussion of Model II, the problem is completely neglected in Model I. This is a striking omission, since the traditional literature in international relations, which Allison cites as having implicitly used rational actor models for centuries, emphasizes how a state's uncertainty about other states' goals and capabilities shapes its own choices.

There do exist formal models of rational choice that show how an actor should make optimal choices under uncertainty. Some of these models were formulated well before Allison conducted his study. (Indeed, he cites relevant texts on statistical decision theory and game theory [1971, 285, n. 87].) Thus, several different types of uncertainty were analyzed and well understood by rational choice theorists before Allison's article or book was written. One kind of uncertainty that was well understood was a nonstrategic, prospective type, involving exogenous and unpredictable acts of nature. Though probably less important than the strategic aspects considered below, they were not irrelevant in the Cuban missile crisis. Bad weather, for example, would have made a surgical air strike more difficult. Unexpected equipment problems, such as those that plagued U.S. helicopters in the aborted Iranian hostage operation, also fall under the domain of statistical decision theory. No rational military planner would assume that all his equipment will work with certainty. And these problems can be sensibly addressed within the confines of the single-actor assumption. Indeed, statistical decision theory classically developed under this assumption, as did the related probabilistic methods in operations research.[10]

Observe that just as introducing multiple time periods raises the issue of decision makers' time preference, introducing uncertainty raises the issue of decision makers' attitudes toward risk. Whether they are risk-averse, risk-neutral, or risk-seeking must be specified in order to complete a rational actor analysis. However, it must be recognized that these issues, though analyzed in more recent international relations theories (e.g., Bueno de Mesquita 1981, 1985), were not salient in rational choice theorizing in political science prior to *Essence of Decision*.

In addition to uncertainty about states of nature, it is empirically clear that U.S. decision makers were very uncertain about the Soviets' motives for placing missiles in Cuba. Allison himself pointed up this problem, describing the markedly different U.S. interpretations of Soviet behavior (1971, 40–56).[11] These interpretations placed the Cuban adventure in the context of larger Soviet strategies, and the magnitudes of these strategies' payoffs were hotly debated by U.S. policymakers.

However, game theory was poorly equipped to address this kind of uncertainty when Allison wrote *Essence of Decision*, and one's evaluation of the book must take this history into account. But over the last dozen years, much of the frontier work in game theory *has* addressed the issues of incomplete information about another player's preferences and imperfect information about a player's past actions. Moreover, this kind of analysis has diffused into formal models of foreign policy crises (Morrow 1989; Powell 1990; Wagner 1989). In this respect, therefore, Allison's rational actor model is seriously out of date.

Game-theoretic Aspects

In international relations, the prime motivation for the unitary rational actor assumption is to conserve the intellectual resources of scholars and to focus these resources on analyzing the strategic relations among nations. It is therefore a surprising and serious omission that Model I does not explicitly examine how other nations might react to a state's moves.

To be sure, Allison's description of the U.S. decision to institute a naval quarantine is replete with references to possible reactions by the Soviets. This, however, is in chapter 2, where he *applies* Model I to the crisis. In chapter 1, where the model itself is presented, there is relatively little in his informal analysis—and nothing in his rational actor model—describing how a rational actor should think through the possible countermoves that an adversary might take in response to the first actor's moves.

Indeed, in chapter 1 Allison persistently blurs the distinction between decision theory and game theory. Under the head "A Rigorous Model of Action," presenting the analytical core of Model I, he writes: "In modern statistical decision theory and game theory, the rational decision problem is reduced to a simple matter of selecting among a set of given alternatives, each of which has a given set of consequences: the agent selects the alternative whose consequences are preferred in terms of the agent's utility function which ranks each set of consequences in order of preference" (1971, 29). And in several other places he refers to the basic "value-maximizing" postulate of Model I (pp. 30–31, 34, 36).

It is true, of course, that a basic premise of decision theory is that actors maximize utility or (in risky circumstances) expected utility. The idea of choosing the alternative that the decision maker prefers above all others makes sense when that decision maker is isolated from all other people. These are the so-called Robinson Crusoe problems (Tsebelis 1989). But clearly, most of the important choices in the Cuban missile crisis were made in a strategic setting—outcomes depended on the behavior of both governments, thus intertwining the fates of the two nations. The relevant branch of rational choice theory for such situations is game theory, not decision theory. And in the branch of game theory that is most relevant to the anarchic context of international politics (noncooperative game theory, which assumes that players cannot make binding agreements), actors are not presumed to maximize utility in any simple, decision-theoretic sense.

Figure 2

The Prisoner's Dilemma

		Player 2: Cooperate	Player 2: Defect
Player 1	Cooperate	R, R	S, T
Player 1	Defect	T, S	P, P

Prisoner's Dilemma Assumption: T>R>P>S

The fundamental prediction of noncooperative game theory is that the outcome of a strategic interaction will be a Nash equilibrium, defined as follows: Given the choice of player 2, player 1 is doing as well as possible; and given the choice of player 1, player 2 is doing as well as possible. Yet the idea of a Nash equilibrium is never mentioned in *Essence of Decision*. The analysis is purely in terms of the value-maximizing choice of an isolated decision maker (1971, 29–31, 34, 36), and there are several important reasons why the outcome of a Nash equilibrium may not be transparently linked to value maximization. We consider three of these.

Games with Undesirable Equilibria. First, *all* the players in a game may dislike the outcome of a particular Nash equilibrium. This can happen not only in a subjective sense (both parties prefer the whole loaf to the half each agreed to take) but in a stronger, objective sense. A Nash equilibrium can be Pareto inferior, that is, there may exist another (feasible) outcome that *both* players prefer to the Nash equilibrium.

This point is particularly important in games where the Nash equilibria are deficient by the Pareto criterion. The outstanding example of this type of game is, of course, the prisoner's dilemma (Figure 2). If this game is played only once, the only Nash equilibrium is the Pareto inferior outcome of (P, P). In such a situation there is no transparent connection between each player's preference ordering $(T > R > P > S)$ and the outcome. Not only is the outcome of mutual defection ranked third by each player, but *both* players prefer the payoff deriving from mutual cooperation.

Games with Multiple Equilibria. Second, in many situations it is impossible for both actors to get their most preferred outcomes, even if the Nash equilibria are Pareto optimal. A well-known example of this in international relations is the game of chicken (Figure

Figure 3

A Game of Chicken

Player 1 \ Player 2	concede	aggress
concede	0, 0	-3, 8
aggress	8, -3	-10, -10

Figure 4

A Game with a Mixed Strategy Equilibrium

U.S. \ USSR	Put missiles in Cuba	Do Not Put Missiles in Cuba
Intensive Inspection	1, -1	-1, 1
Casual Inspection	-1, 2	2, 0

3). There are two Nash equilibria in chicken: (*aggress, concede*) and (*concede, aggress*). In the first outcome, player 1 gets its most preferred outcome, and player 2 is stuck with its third best. In the second equilibrium, matters are reversed. The presence of these two equilibria, with their markedly different payoffs, points up another important difference between decision- and game-theoretic analyses. If there is only a single decision maker, then there is either a unique alternative that is value-maximizing or several alternatives. Neither case presents a problem; for even if there are several such alternatives, they must by definition yield the same payoff and are therefore equivalent. But in a multiperson situation, the equilibria need not be equivalent; they can differ sharply in both relative and absolute senses. Accordingly, the players in chicken are not indifferent between the two equilibria.

Hence, once again, there is no simple relation between the game-theoretic notion of equilibrium outcomes and the decision-theoretic idea of value maximization. It is true that *if* player 1 aggresses, player 2's value-maximizing choice is to concede. But player 2 would itself be better off aggressing—so long as player 1 concedes. Thus, contrary to Allison's claim that "in modern statistical decision theory and game theory, the rational decision problem is reduced to a *simple matter* of selecting among a set of given alternatives" (1971, 29; emphasis added), chicken exhibits an intricate strategic structure. Indeed, due to its complexity—the potential for mutual disaster if each side tries to get its maximal payoff or mutual accomodation if both sides try to avert disaster—predicting the outcome of chicken is by no means trivial. *Any* of the four outcomes could plausibly occur. Whether this is a defect of game theory in not making a falsifiable claim for this game or an indeterminacy inherent in the situation, we cannot say. What we can say is that just assuming rationality does not, in itself, reduce the decision problem to a "simple matter."

In our experience teaching models of politics, most students begin a course believing that preferences and outcomes are tightly linked in rational choice models and that the paradigmatic rational choice answer to the question, "Why did this outcome occur?" has the form, "Because actor so-and-so wanted it."[12] Such explanations do not come close to tapping the potential of rational choice analyses. They completely overlook the complexities arising both from strategic interaction and from the institutional context of the game. Indeed, by slighting such complexities, these uses of rational choice theories *degrade* their value; much of the intellectual benefit of rational choice analyses is that by black-boxing inner mental processes, we can concentrate our attention on relations among decision makers.

Endogenous Uncertainty in Games of Complete Information. The preceding remarks about the game of chicken informally suggest that game theory might view some interactions as being fundamentally indeterminate. We will now sharpen this point. It has been well known for decades that uncertainty can arise *endogenously* in strategic situations. To illustrate the point squarely in the matter at hand, consider the representation of one aspect of the Cuban missile crisis (Figure 4).

In the situation shown in Figure 4, if the United States carries out intense inspections, the USSR would prefer not to put the missiles in Cuba ($1 > -1$). However, if the USSR holds back, then the United States would rather not incur the cost of intense inspections ($2 > -1$). But if the United States conducts only cursory inspections, then the USSR does want to install the missiles ($1 > 0$). This, however, takes us full circle, since if the USSR is going to install the missiles, the United States wants to inspect closely ($1 > -1$). Hence, in this kind of situation, there is no equilibrium in pure strategies; but there is an equilibrium if each player uses mixed strategies, that is, plays probabilistically.[13] Thus, uncertainty will arise endogenously from the strategic logic of the situation. The only equilibrium in this circumstance is where each side keeps the other guessing about its course of action, just as in the child's game of stone–scissors–paper.[14]

It is evident, therefore, that strategic uncertainty was relevant in the crisis and that some theoretical tools were available to aid analysis. (For a recent

analysis of the Cuban missile crisis using a model of strategic uncertainty, see Wagner 1989.)

Many readers of *Essence of Decision*, we suspect, conclude that models of states as unitary rational actors provide an inadequate account of this crisis. Such a conclusion is not well founded. Allison's version of a rational action explanation is so oversimplified that it should not have been *expected* to perform well.

ANALYSIS OF MODEL II

Allison's discussion of Model II is one of his strongest sections. He extracts several insights from organization theory and applies them to the missile crisis in interesting ways. For example, though the Soviets may have had sound engineering reasons for building the missile sites in their characteristic pattern, Model II suggests an interesting alternative explanation—it was simply a bureaucratic routine being enacted. In the study of foreign policy making and implementation, Allison's chapters on bureaucratic routines were pioneering efforts.

Nonetheless, we have some reservations about his analysis. The core of Model II is the idea, developed by Herbert Simon and James March, that individual decision makers are boundedly rational (see Allison 1971, 71). We have two main concerns with Allison's interpretation of this theoretical core. First, Model II assumes that imperfectly rational agents would usually use simple decision rules (p. 83). In itself, this assumption is consistent with much of the writing of the Carnegie School.[15] But Model II goes beyond this premise by suggesting that these simple rules, or *standard operating procedures*, sharply limit and constrain *behavior*, that is, that simple rules generate simple, predictable behavior (pp. 78–79, 83, 87–91). We believe that this conclusion greatly underestimates how complex behavior—of even a single decision maker—can arise out of the use of simple rules. Second, suppose we grant that *individual* decision makers are sharply constrained in their cognitive abilities. What does this premise imply about how constrained *organizations* are? Consistent with the viewpoint of Simon's *Administrative Behavior*, we argue that organizations can significantly ease the constraints on information processing that confront individuals (1947, 79–80). Taking these two points together, our position is that the use of simple decision rules by individual decision makers does not imply that the behavior of an organization will be simple, unsophisticated, or predictable. Thus, we agree with March and Simon, who argue that "complex processes can be aggregated from simple elements" (1958, 178).

Decision Rules Versus Behavior

Allison's basic image of organizations in Model II is that of simple, predictable behavior generated by simple rules:

> Major lines of organizational action are straight—i.e., behavior at one time, t, is marginally different from behavior at $t - 1$. Simple-minded predictions work best: behavior at $t + 1$ will be marginally different from behavior at the present time.
>
> The preeminent feature of organizational activity is its programmed character: the extent to which behavior in any particular case is an enactment of preestablished routines.
>
> The fact that the fixed programs (equipment, men, and routines that exist at the particular time) exhaust the range of buttons that leaders can push is not always perceived by these leaders. (1971, pp. 91, 81, and 79, respectively)

This image resonates strongly with our intuitive conceptions. We are all familiar with the stereotype of an inertial bureaucracy mindlessly following its procedures regardless of consequences. As with many popular images, this one is not completely wrong—far from it. The picture of simple, rule-governed action provides important insights into organizational behavior. However, precisely because the image is so intuitive, we must be careful. Familiarity in model building breeds laziness, if not contempt. The notion that simple rules generate simple behavior is so commonsensical that it is easy not to question it. Yet the link between rules and behavior is more subtle than is captured by either Model II or its associated popular image. Complex behavior *can* emerge from simple rules. We now describe six ways this can happen.

Varying, and Possibly Stochastic, Inputs. Many choice rules work on inputs—information, people, objects—that vary significantly. This variability, in tandem with even a simple rule, can produce complex behavior. A good example of this is the behavior of one of the simplest strategies in the prisoner's dilemma, Tit for Tat (TFT). This strategy—cooperate on the first round and thereafter play what one's partner did on the previous round—is only a tad more complicated than the *simplest possible* rules (always cooperate or always defect (all-D)). However, whereas those unconditional strategies generate behavior as simple as themselves (regardless of their strategic environment), in variegated environments TFT will behave in correspondingly complex ways. Against all-D, TFT cooperates once and never again; against a nice partner TFT cooperates forever; against a sneaky nasty strategy, TFT will cooperate as long as its partner does, retaliating against a defection but then returning to cooperation if its sneaky partner apologizes. Moreover, if the environment is noisy (unobserved random disturbances affect the players' payoffs), TFT produces variable behavior even when playing an equally simple strategy—itself (Bendor 1987).

Because simple rules can be activated by random shocks in an organization's environment, its behavior can be, in a certain sense, surprising, as well as complex. For example, disaster relief could be highly routinized and preprogrammed, but the organiza-

tion's behavior would not be *unconditionally* predictable. For these organizations, it need not be true that the best predictor of the organization's behavior at time t is its behavior at $t - 1$.[16]

Recursive Rules. Many public agencies seem to follow a simple history-dependent rule of the form, decision $t + 1 = f$(decision t). Such rules are called *recursive*. To figure out what the decision will be in period $t + k$, in terms of the decision made in period t, one just reapplies the rule $f(\cdot)$ over and over again. Thus, decision $t + 2 = f$(decision $t + 1$) $= f[f$(decision t)], and so on. Do such recursive choice rules always generate simple behavior? They do not. Interesting developments over the last 10 years in a field of mathematics known as *chaos theory* have shown that if a recursive rule is nonlinear, it can create a pattern that is so complex as to appear random, even though the rule itself is completely deterministic. For example, consider the simple recursive rule $x_{i+1} = k \cdot x_i(1 - x_i)$, where k is a constant between 0 and 4. Let the initial value x_1 be between 0 and 1. How will x behave over time? It turns out that if k is close to 4, the behavior of x is "chaotic," wandering all over the [0, 1] interval and never settling down. For example, let $k = 3.7$ and $x_1 = .4$. Then the next 10 values of x_i (rounded to four places) are .888, .368, .8605, .4441, .9134, .2926, .7658, .6636, .8259, and .5319.

Merely inspecting these values of x_i, one would probably attribute them to a stochastic process. Yet the underlying equation is not only deterministic but also very simple, being only one degree higher than linear. Moreover, it is not a historically rich rule, for it depends only on the decision in the previous period. Real precedent-governed agencies have a much more complex memory structure.

Simple Linear (Though Probabilistic) Rules. Since Model II repeatedly refers to the linear nature of organizational behavior, one may object to the previous example because it is nonlinear. We respond in two ways. First, linearity is an empirical hypothesis; we have no a priori guarantee that organizations typically behave linearly. Second, we now demonstrate, via another example, that behavior can be surprisingly complex even if linearity is retained.

Consider the following model of experiential learning. Suppose an administrator has two options, x and y, and knows that neither one is sure to work or doomed to fail. Objectively, the probability that x succeeds is p, the chance that y works is q, where $0 < q < p < 1$. (They need not sum to one.) These probabilities are constant and independent over time. In the first period, the decision maker has a propensity to try x denoted $p_1(x)$ and a propensity to try y denoted $p_1(y) = 1 - p_1(x)$. Over time, the administrator adapts via a simple learning rule. If in period i x was tried successfully, the administrator's propensity to try x again is strengthened:

$$p_{i+1}(x) = p_i(x) + a[1 - p_i(x)].$$

The parameter a represents the rate of learning and is between 0 and 1. If x failed, the administrator's propensity to try it weakens: $p_{i+1}(x) = bp_i(x)$, with b also between 0 and 1. The equations for alternative y have the same form. Note that these equations are linear.

However, whereas an expected utility maximizer will behave simply in the long run—choosing x always—a decision maker using this linear adaptive rule will never settle down on either alternative. Instead, the adaptive decision maker will use alternative x in the long run $(1 - q)/[(1 - p) + (1 - q)]$ proportion of the time and y the remainder. Thus, the simple linear adaptive rule produces more complex behavior than does the optimal strategy.

Combinatorial Effects. Several times in *Essence of Decision,* Allison uses the metaphor of chess when discussing how leaders are constrained in their choice of options. This is odd, because chess is the paradigmatic example of a choice situation that involves only a handful of basic rules yet exhibits truly Byzantine strategic complexity. There are only six distinct pieces in chess; each moves in only a few ways. (We may consider each piece, with its legal moves, as equivalent to a standard operating procedure.) Therefore, each player is "limited" to shuffling and reshuffling this combination of standard operating procedures over the 64 possible locations.

But the quantitative impression thereby conveyed, namely, that the limited number of procedures creates a roughly equal (even in respect to order-of-magnitude) number of moves is grotesquely inaccurate. As is well known and as Allison himself noted, the number of possible sequences of play (i.e., behavior) in chess is staggeringly large (1971, 286, n. 93). A common estimate is 10^{120}, a number so large as to be equivalent, for all practical purposes, to infinity. Human beings can never exhaust the richness of chess. This is pure combinatorial explosion: the rules are deterministic. Nor does the complexity depend upon stochastic inputs. It is simply a matter of stringing together the handful of rules to generate new positions, whence one reapplies the basic rules, and so on. The example of chess suggests that our intuitions grossly underestimate combinatorial complexity.

Moreover, when we compare chess to the strategic maneuverings of two real military forces (to return to *Essence of Decision*'s empirical domain), the odds are good that chess is *simpler*. No matter how one counts distinct "pieces" in, say, a navy, there are surely more than seven! No matter how crudely one wants to count the basic standard operating procedures of, say, a destroyer, it has more than the handful available to even the most complex chess piece. And the "board" of any moderately large battle is more variegated than the (nontopographical) eight-by-eight chessboard. Therefore, chess, the paradigmatic choice environment of behavioral decision theory, provides a *lower bound* for the complexity of behavior one would expect to see in a clash between two governments. Thus, in this instance *Essence of Deci-*

sion seriously misapplied the lessons of the Carnegie School.

Hierarchies of Procedures. In a sense the linking of procedures into a larger unit (creating a "program," in Allison's terminology) is just a type of combinatorial complexity. In this sense the remarks just made apply. However, hierarchies of rules create another way for complex behavior to arise from simple rules. Suppose the Department of Defense used the simple budgeting rule, "Every year we allocate 10% of our budget to research and development." Naturally, this rule would be accompanied by a host of procedures concerning for example, how the designs of weapon systems should be specified. For our purpose, we focus on the relation between this one rule and consequent behavior.[17]

Again, the rule's simplicity is deceptive. The allocation of a portion of the Defense Department's resources to research and development has created a historically unprecedented number of new weapons, introduced at an equally unprecedented rate. Each new weapon is accompanied by its own standard operating procedures, as well as the procedures "frozen into" the hardware itself. Thus simple high-order rules can generate new lower-order ones. (For a discussion of this point, see March and Simon 1958, 150, 170.)

Large Sets of Rules. Our first three points showed how a single rule could produce complex behavior. The next two points showed how a few simple elemental rules could aggregate into complex behavior. Here we wish to underscore an obvious empirical point: even if we set aside combinatorial complexity, some agencies have a great many rules. Consider the Internal Revenue Service code. The gradual, barnaclelike accumulation of exemption piled upon exemption, modification upon modification, has created a code of rules that is awesomely complex.

Very large sets of rules create problems for scholars, as well as citizens. An observer may find it difficult to predict organizational behavior because of not knowing *which* rule will be evoked out of the set of all possible rules. There are two related reasons for this. First, large sets of rules increase the possibility that two or more rules may conflict: more than one rule will apply to any one situation, and it will be unclear which has priority. Second, though many organizational rules have an *if/then* structure like computer programs ("If condition x obtains, apply procedure y"), the conditional part of the rule—the description of condition x—is often much less precise than in computer programs. This also makes it more difficult to predict which rule will be evoked.

The classic illustration of both problems—conflicting rules and imprecise conditionals—is the legal system. If one regards the judiciary as an organizational system, the model of rule-governed behavior seems apt. A judge's behavior is, after all, supposed to be informed by legal rules. But even knowing the whole set of rules that could guide the judge's decision may not enable one to predict behavior, because one does not know *which* of the many legal precedents the judge will select. Again, behavior is unpredictable despite the stable set of rules. The test is not, Did the decision follow from an organizational rule? The test is, Could one have *predicted* which organizational rule would be evoked?

It should be understood that we are not here advancing empirical hypotheses. We are not asserting that any particular set of officials or agencies use any of the rules described in our first five points. Our argument is theoretical. The relation, implicitly assumed by Model II, between simple rules and simple behavior is weaker than one might think. The examples illustrate that rules can generate relatively complex behavior even when the rule is deterministic (points 1 and 2) or linear (point 3) or the game is isolated from stochastic shocks (point 4). Accordingly, the suspicion grows that simple rules will produce simple organizational behavior only if (1) the rules are deterministic *and* (2) the rules are linear *and* (3) the game is isolated from stochastic shocks *and* (4) individual choice rules are not aggregated in a combinatorially rich way *and* (5) individual choice rules are not grouped hierarchically so that higher-level rules generate new lower-order ones (and new ensuing behavior). These conjunctions indicate a shrinking of the parametric space in which the postulated simple behavior will occur. In terms of our prior beliefs about bureaucratic behavior, one may feel confident in asserting, say, that individual choice rules are linear, or that they are deterministic, or the like. But would many scholars in this field confidently assert all five conditions? We doubt it.

Bounded Rationality: Individual and Organizational

A second problem of Model II concerns the relation between individual and organizational constraints on rationality. Organization theorists have often moved "rather cavalierly from theories of individual cognition and choice to theories of organizational cognition and choice" (March and Shapira 1982, 11). This tradition has sometimes been carried to the point of anthropomorphizing organizations, positing that organizational constraints on information processing simply mirror individual constraints.

This quick passage from individual to organizational limits on rationality is evident in *Essence of Decision*: "Simon and the Carnegie School focus on the bounded character of human capabilities. Firms are physically unable to possess full information, generate all alternatives" (Allison 1971, 174). Again, "The physical and psychological limits of man's capacity as alternative generator, information processor, and problem solver constrain the decision-making processes of individuals and organizations" (p. 71).[18]

Undoubtedly the bounded rationality of single de-

cision makers constrains the information-processing capacities of agencies in some fashion. However, we maintain that the relation between the two is more complex than is portrayed by either Model II, in particular, or much of organization theory, more generally. For normative reasons—and because the literature has focused heavily on the tendency of institutions to be dumber than their members (via, e.g., conformity pressures)—we are especially interested in how organizations can be *smarter* than the individuals who compose them. We therefore consider three reasons why organizations can process information more effectively than individuals can.

From Serial to Parallel Information Processing. In a neglected passage of *Organizations*, March and Simon note an important difference between individual and collective cognition: "The individual possesses only a single focus of attention, hence can only deal with one aspect of the problem at a time. In organizations, however, there is no limit to the number of attention centers that can be allocated to the parts of a problem" (1958, 193). They go on to note the advantages of parallel processing. Clearly, if one large problem can be decomposed into many subproblems, an agency can greatly speed up its information processing by assigning a different official to each subproblem.[19] Of course, there is no guarantee that any given division of labor will be an effective decomposition. The trick, as many students of organizational design have noted, is to figure out a scheme of specialization that "carves nature at the joints" (see Gulick 1937; March and Simon 1958; Simon 1947; Simon, Smithburg, and Thompson 1950). When this can be done, the serial constraint can be substantially eased.

This organizational advantage is obvious. Just mentioning the phrase, *division of labor*, helps us recognize an organization's advantage over an individual. In the simplest model of problem decomposition (an additive one) information processing is essentially the same as performing a physical task, like Frederick Taylor's ([1911] 1947) pig iron loading. Obviously, the more people, the more pig iron is loaded or the faster information is processed. Otherwise, why bother to hire the additional people? Yet in the organizational decision theory literature, this obvious point is sometimes overlooked.

A second, more subtle advantage of specialization is that it enables decision makers to become experts in their domains. Becoming an expert means relying less on ordinary folk heuristics, with their attendant biases and more on scientifically based inferences, with their lower rates of error (Hogarth 1987; Nisbett and Ross 1980). As Nisbett and Ross replied to a colleague who, having read a draft of *Human Inference*, asked, "If we're so dumb, how come we made it to the moon?"—

> Humans did not "make it to the moon" by trusting the availability and representativeness heuristics or by relying on the vagaries of informal data collection and interpretation. On the contrary, these triumphs were achieved by the use of formal research methodology and normative principles of scientific inference. Furthermore. . ., no single person could have solved all the problems involved in such necessarily collective efforts as space exploration. Getting to the moon was a joint project, if not of idiot savants, at least of savants whose individual areas of expertise were extremely limited- . . . . Finally, those savants included people who believed that redheads are hot-tempered, and who at the advice of an acquaintance's brother-in-law bought their last car at the cocktail-party. (1980, 250)

Reliable Systems, Unreliable Components. In his book on the Polaris missile system, Sapolsky (1972) observed that several teams worked independently to develop a key component. The Navy's Special Projects Office deliberately established this redundant structure to increase the odds that the essential component would be ready by a specified date. By doing so, the Special Projects Office loosened the connection between the reliability of an organizational subunit (one of the problem solving teams) and the larger organization—in contrast to the message of Model II.

More generally, a properly designed agency can display large disparities between the reliability of different organizational levels (Bendor 1985; Grofman, Owen, and Feld 1983; Landau 1969). Consider a representation of the Polaris project. There are n teams trying to develop the same component. The probability that any one of the teams will succeed in the specified time is p, where $0 < p < 1$. If the performance of the teams is independent, the probability that the organization will succeed equals p(at least one of the teams succeeds), which equals $1 - [(1 - p)^n]$. This probability increases steadily toward one as n increases, so long as p exceeds zero. Thus, in this simple setting, the performance of subunits and units need not be closely connected.

From Tunnel Vision to Innovation. Model II's image of organizational change is of sluggish entities dominated by a single way of thinking. Yet modern bureaucracies are often populated by different kinds of professionals with distinctive mindsets. What is commonplace to one professional culture may be dramatically new—even bizarre—when introduced to an agency dominated by another profession. Observe, for example, the clash between civil engineers and environmental analysts in the Corps of Engineers (S. Taylor 1984). The questions raised by new environmental analysts about water projects, though alien to the engineers and the Corps, were standard in the community of environmental analysts. Or consider the idea of vouchers for schools. To a professional with a background in educational administration, vouchers are a dramatic departure from the status quo. But to an economist specializing in education policy, they are an obvious extension of the basic principle of market competition. In both examples, innovation was based on straightforward applications of professional expertise; heroic mental efforts were not required.

Scholars typically understand the main lesson of Model II to be that routines and standardized scenarios constrain and rigidify organizational behavior (e.g., Art 1973, 476–79; Gallucci 1975, 145; Jefferies 1977, 231–32; Krasner 1972, 169–75; Levy 1986; Peterson 1976, 113; Scott 1981, 6; Thompson 1980, 27; Williamson 1979, 140). (For a partial exception to this pattern, see Posen 1984, 46–47.) True, Allison occasionally mentions the possibility of organizational innovation and at times portrays standard operating procedures as efficient ways of handling standard problems.[20] But most of chapter 3's theoretical exposition emphasizes the negative, constraining effects of organizational routines. And this pattern is strikingly reinforced in chapter 4's empirical application of Model II to the missile crisis. Virtually without exception, the episodes covered there describe how the permanent bureaucracies of both countries fouled things up. Toward the end of chapter 4, Allison pointedly asks, "Were the organizations on top of which the President was trying to sit going to drag the country over the nuclear cliff in spite of all his efforts?" (1971, 141). The heroes of this chapter are clearly the unbureaucratic president and his advisors. Therefore, the common interpretation that Model II's main lesson concerns the rigidifying, maladaptive effects of bureaucracy is well founded.

This is an irony of intellectual history. As Allison noted, the central ideas of Model II are rooted in the Carnegie School. Yet a founding volume of that tradition, Simon's *Administrative Behavior*, views routines far more positively:

> Habit performs an extremely important task in purposive behavior, for it permits similar stimuli or situations to be met with similar responses or reactions, without the need for a conscious rethinking of the decision to bring about the proper action. Habit permits attention to be devoted to the novel aspects of a situation requiring decision. A large part of the training that goes to make a championship football team, crew, army battalion, or fire company is devoted to developing habitual responses that will permit immediate reactions to rapidly changing situations.
>
> Habit, like memory, has an artificial organization counterpart, which has been termed by Stene "organization routine." (1947, 88)[21]

Indeed, Simon goes much further than this in praising the general role of organizations in amplifying individual rationality:

> It is impossible for the behavior of a single, isolated individual to reach any high degree of rationality. The number of alternatives he must explore is so great, the information he would need to evaluate them so vast that even an approximation to objective rationality is hard to conceive. . . . One function that organization performs is to place the organization members in a psychological environment that will adapt their decisions to the organization objectives, and will provide them with the information needed to make these decision correctly. . . . In the course of this discussion it will begin to appear that organization permits the individual to approach reasonably near to objective rationality. (pp. 79–80).

Hence, chapter 4 of *Essence of Decision* inverts Simon's view. Instead of organizations boosting individual rationality, the bureaucracy's rigid conduct nearly nullified Kennedy's best efforts.[22]

Why do Simon's and Allison's books assess standard operating procedures in particular and organizations in general so differently? We believe that different baselines of evaluation are at work here. In *Administrative Behavior*, Simon began with the premise that *individuals* are boundedly rational. With this as a starting point, it is not surprising that he emphasized the enabling aspects of standard operating procedures and specialization. Given the real limits on individual cognition, technically demanding projects such as going to the moon can be handled only by collective endeavor. In contrast, *Essence of Decision* begins with Model I, which presumes perfectly rational individuals. Whether Allison intended unboundedly rational decision makers to be the standard for evaluating Model II actors is not completely clear; but the book's structure and the frequent use of Model I as an analytical benchmark suggest that this was the implicit standard.[23] With that baseline, it is not surprising that Model II organizations, staffed by boundedly rational individuals, appear more the problem than the solution. But we would ask, How well could the United States have gathered information about the Soviet missiles in Cuba, conducted the blockade, and carried President Kennedy's message to the world had there been *no* routines to rely on? How effective would the U.S. response been if it had had to invent all procedures on the spot? Had Allison disentangled the different effects of bounded rationality and organizational action by inserting a model of a unitary-actor-with-imperfect-rationality in between Models I and II, his evaluation of the *net* effects of organization—holding constant the degree of individual rationality—might have changed considerably.

ANALYSIS OF MODEL III

Allison's central argument in Model III is easily stated: "The name of the game is politics: bargaining along regularized circuits among players positioned hierarchically within the government. Government behavior can thus be understood according to a third conceptual model, not as organizational outputs but as results of these bargaining games" (1971, 144). It proved difficult, however, to build a clear and coherent model around this claim. Some of the fault lies with the literature on which Allison had to rely. For the rational actor model, he could draw on well-specified, axiomatically constructed theories. The organizational process model is based on works that advance propositions that are thematically (though not deductively) connected. In contrast, the literature on bureaucratic politics is more discursive and far less explicitly theoretical. Allison himself remarked on this difficulty: "Unfortunately, the interests of these analysts have not led them to invest much labor in

squeezing their insights into propositions. Consequently, the paradigm . . . can be no more than a tentative formalization" (p. 147). Even his tentative formalization, however, attracted far more critical attention from reviewers of *Essence of Decision* than either of his other models.

Criticisms of Model III can be grouped into four categories. First, many critics argued that Model III misconstrues the nature of executive branch policymaking. In particular, we will argue, the central claim (that policymaking necessarily involves bargaining among executive branch decision makers) must be questioned. Second, we will point out that Model III said surprisingly little about the fact that executive branch policymaking takes place within a *hierarchy*. Hierarchy's impact on policymaking is something that both Allison and many of his critics overlooked. Third, we will argue (as did several critics) that Model III is the least precisely formulated of Allison's approaches. Not only are its fundamental assumptions much less clear than those of Models I and II, but many of the propositions seem to be ad hoc observations not derived in any rigorous sense from the assumptions. Finally, while many critics questioned Model III's premise of policymaking-as-bargaining and some pointed out that Model III's ambiguities raise questions about its status as a model, few critics made an argument about Model III that we think is equally important, namely, that the model is simply too complex. There are so many different assumptions, variables, and relationships in Model III that it is almost impossible to determine the role and impact of any one of them. For Model III to be useful for systematic analysis, it must be simplified considerably.

Does Policymaking in the Executive Branch Proceed Via Bargaining?

It is not hard to distill from the bureaucratic politics literature the claim that policymaking in the executive branch proceeds via bargaining. Yet this claim is not a completely accurate reading of the literature. And whatever the literature says, the claim turns out to be vulnerable to several different kinds of criticisms. We will examine these difficulties by posing a series of questions about the properties of Model III.

Do Executive Branch Decision Makers Always Have Different Goals? Given our typology of models of policymaking in Figure 1, the assumptions of Model III would clearly seem to place it on row 3: there are many decision makers and they have conflicting goals. However, row 2 of the typology raises the possibility that these multiple decision makers may sometimes have the *same* goals. Indeed, Krasner suggests that considerable similarity in policymakers' views should be expected: "The President chooses most of the important players and sets the rules. He selects the men who head the large bureaucracies. These individuals must share his values. Certainly they identify with his beliefs to a greater extent than would a randomly chosen group of candidates" (1972, 166).

Moreover, Art (1973) argues that shared "mindsets" often influence foreign policymaking. He notes, for example, that in the decisions to intervene in Korea and the Dominican Republic, "there was unanimity on the need to use military force, once the principals involved realized that nothing short of that would achieve American objectives" and that for Cuba and Vietnam, "the need to use force was questioned by one or two principal advisors; but they were clearly out of the mainstream of the President's thinking and his advisors' outlooks" (p. 481). The implication of Art's point is clear: "If shared images dominate senior players' outlooks and if they are truly *shared*, then what is the merit in asserting that governmental actions are the resultants of pulling, hauling, and bargaining?" (p. 476). To the extent that Model III assumes that actors have conflicting goals, these examples suggest some clear limits to the model's empirical domain.

Does Conflict Imply That Policymakers Have Different Goals? When executive branch conflict does occur, understanding its causes requires that we distinguish between differences over goals and differences over beliefs about how to achieve the goals. The relative absence of conflict in Art's four cases might suggest that the key decision makers did share objectives. However, even if actors' goals are identical, they may disagree about how to achieve those ends, and these conflicting beliefs might foment arguments. While Allison does mention the importance of differing beliefs, he does not adequately distinguish between conflicts caused by differing beliefs and conflicts caused by differing goals.

What kind of policymaking should we expect among actors with conflicting beliefs? Thompson and Tuden (1959) suggested that when there is agreement on beliefs about causation but disagreement on goals, then bargaining will be observed, as in Model III. But where there is agreement on goals but disagreement on beliefs, they suggest that "collegial" judgment will prevail. Collegial problem solving involves efforts to ascertain the justification for differing beliefs and to change the beliefs of other actors. If agreement on beliefs remains elusive, more overtly political activities might occur, though it seems likely that actors would try these only as a last resort. Why yield something by bargaining when one might persuade others of the empirical correctness of one's position? When stakes are high, outcomes uncertain, and beliefs deeply held, debates over how to reach a common end may become rancorous; passionate disagreements need not indicate goal conflict. But such debates differ significantly from those caused by conflicting objectives.

As an empirical question, Did President Kennedy and the various members of the ExCom have conflicting goals? Decisions were certainly made in a partisan context. Recall the criticisms by congressional Repub-

licans and the administration's nervousness about the upcoming congressional elections. But by Allison's own account this was more a matter between the president and his critics *outside* the administration than among ExCom members (1971, 187–200). Certainly, administration officials argued over which action to take; but these disagreements could have stemmed mainly from honest differences in beliefs about the effects of various options. While Allison criticizes Sorensen (1963) and Schlesinger (1965) for presenting "the efforts of the ExCom in the Cuban missile crisis as essentially rational deliberation among a unified group of equals" (p. 146), it is not apparent from Allison's own account whether differences in personal and institutional goals (hence, "politics") or differences in beliefs (hence, "collegial," and possibly "rational," decision making) were more important in the ExCom debate.[24]

Why Would a President Bargain with Other Executive Branch Officials? One of Model III's great ambiguities concerns the nature of the president's relations with other officials in his administration. At one point, for example, Allison approvingly comments on Neustadt's picture of the president's role in policymaking: "Sometimes Neustadt's 'President-in-sneakers' is the central political gamesman. Sometimes the President merely observes the pulling and hauling among various groups within the government. Sometimes the President is the target of the tactics of a bureaucratic group" (1971, 158). Later, Allison also states, "Where an outcome was for the most part the triumph of an individual (e.g., the President) or group (e.g., the President's men or a cabal) this model attempts to specify the details of the game that made the victory possible" (p. 173). It would seem, then, that the extent of the president's involvement in policymaking—and his ultimate success—can vary from case to case.

Yet the central thrust of Model III is not this bland point that the president's influence varies across issues. Instead, it is the much bolder hypothesis that in order to make policy the president must bargain with members of his own administration. As Allison himself put it, "The primary source of the paradigm is the model implicit in Neustadt's work, though his concentration on Presidential action has been generalized to a concern with action as a resultant of political bargaining among a number of independent players, the President being only a 'superpower' among many lesser but considerable powers" (1971, 162). This raises an obvious question: Since the president has personally appointed the top officials in his administration and can dismiss them at any time and since he has substantial formal authority (especially on foreign policy issues) to order them to do what he wants, why must the president bargain with them?

The literature on which Allison based *Essence of Decision* certainly does not lend unqualified support to the idea that superiors will bargain with their subordinates. For example, March and Simon (cited by Allison for Model II) note that such bargaining has some "potentially disruptive consequences":

> Bargaining almost necessarily places strains on the status and power systems in the organization. If those who are formally more powerful prevail, this results in a more forceful perception of status and power differences in the organization (generally dysfunctional in our culture). If they do not prevail, their position is weakened. Furthermore, bargaining acknowledges and legitimizes heterogeneity of goals in the organization. Such a legitimation removes a possible technique of control available to the organizational hierarchy. (1958, 131).

Thus, bargaining is something the president would seek to avoid, if at all possible. The question is whether he can avoid it.

Model III, of course, is based on the bureaucratic politics literature, and this literature reveals ample support for an argument that the president often *can* avoid having to bargain with subordinates. For example, Huntington's *Common Defense* has a substantial section analyzing when policymaking is *legislative* in character and when it is *executive* (1961, 146–59). It is interesting to note that Allison quotes a passage from this section (*Essence of Decision*, 156). Indeed, he even cautions that Huntington's analysis "overemphasizes participant equality as opposed to the hierarchy that structures the game" (p. 162). Hence, it is all the more puzzling that Allison nowhere actually *uses* Huntington's arguments. Instead, he just assumes that executive branch policymaking will always be legislative in nature; the possibility of executive policymaking is simply ignored.[25]

Thus, if presidential power is a *variable*, as Huntington suggests, when are presidents powerful and when not? The literatures on bureaucratic politics and organization theory reveal two basic reasons why a president may sometimes be in a disadvantageous position vis-à-vis members of his own administration. The first is that subordinates may have sources of political support outside the executive branch. The second turns on Weber's classic argument about informational asymmetries between a superior and his subordinates. Allison's presentation of Model III does mention these two sources of power (1971, 168–69); but his model does little to explore the role of either factor in policymaking.

Do Subordinates Have Political Support outside the Executive Branch? While the president has much more formal authority than his appointees, his authority in the larger political system is far from absolute. Because Congress has an important constitutional role in policymaking, agencies in the executive branch find it useful to cultivate support in Congress. While the president can often order his political appointees and their respective bureaucracies to do his bidding, they may be able to hurt him politically if they disagree with his choices and make their disagreements known to outside supporters. Hence, the president may end up bargaining with subordinates not because they are intrinsically powerful but because their outside supporters can make life difficult

for him. Thus even though the most obvious manifestations of bureaucratic politics may take place inside the executive branch, what is driving the politics may be happening outside.[26]

Of course, when the most important actors in the environment support the president, he need not bargain with any subordinates who are allied with them. Moreover, even if influential decision makers, whether inside or outside the executive branch, have diverse views and even if some of them are hostile to the president, the president may still be left relatively free to select who will be his allies. As Art suggests, "When senior executive players are split on their policy stances, the President, by virtue of the division, has considerable leeway to choose that which he wishes to do, or that which he thinks he ought to do, or that which he reasons he must do" (1973, 475). It is primarily when most of the environment is hostile and the president lacks countervailing support that he must bargain with his nominal subordinates. In sum, support outside the executive branch does not always mean influence inside it.

Do the President's Subordinates Have Informational Advantages? Even a subordinate who lacks support outside the executive branch may still enjoy an informational advantage over the president. Nonetheless, it is not obvious that bargaining will be the empirical manifestation of informationally based influence. Three categories of informational asymmetries can be distinguished. For a president to exercise his authority over the executive branch, he must become aware of what problem needs to be addressed, decide what to do about it, and have his choice implemented. For each of these three tasks, subordinates may have more information or expertise than the president.[27] This puts them in a position where they can manipulate what problems he is aware of, what advice he receives, and how his final choice is implemented.

However, the standard works on bureaucratic politics in the era of *Essence of Decision* (e.g., Downs 1967; Halperin 1974; Rourke 1969; Tullock 1965), as well as the more recent literature on incentives (e.g., Bendor, Taylor, and Van Gaalen 1987), suggest that there are ways the president can alleviate these informational problems. A wide-ranging communication network can alert him to a problem; and if he then attends closely to the problem, he can learn a good deal about it, thereby reducing his subordinates' informationally based influence. And once he knows what he is doing, he is not without weapons, as Krasner observed: "The Chief Executive involves himself in those areas which he determines to be important. When the President does devote time and attention to an issue, he can compel the bureaucracy to present him with alternatives. . . . Even when Presidential attention is totally absent, bureaus are sensitive to his values. Policies which violate Presidential objectives may bring Presidential wrath" (1972, 168–69). Kohl (1975) provides systematic support for this argument: in an empirical test of several models of president–subordinate relationships, the attention of the president and his top aides and the coherence of their ideas emerged as key variables. Bureaucratic politics flourished largely when the president and aides paid little attention to an issue or lacked clear policy preferences about it.

Finally, even if informational asymmetries persist, the subordinates' influence depends on the president not realizing that he is ignorant. The subordinates' influence is maximized when the president is unaware of what constitutes good advice or that a problem exists or that implementation should be monitored. Even here, however, we think their influence would not make itself felt through *bargaining*. If the president does not realize that he is ignorant, the very fact that his subordinates try to bargain with him should alert him that something odd is going on. To influence the president, the subordinates would have to use their informational advantage in more subtle ways.

Was U.S. Policy in the Cuban Missile Crisis Determined by Bargaining? While our purpose is largely theoretical, it is of interest to ask whether Model III accurately describes U.S. policymaking during the missile crisis. It seems to us that the most obvious manifestations of politics—coalition formation, bargaining, logrolling, splitting the difference, leaks to the press—were largely absent from the ExCom deliberations.[28] Instead, our reading of *Essence of Decision* is that policymaking revolved around President Kennedy and that decision making lay largely in his hands. Allison's own evidence reveals that the participants themselves believed that one person was in charge. Recall, for example, his observation that Dean Acheson, emerging from an unsuccessful effort to convince the president to approve an air strike, "left with no question in his mind about where the buck stopped" (1971, 207). Allison even quotes Robert Kennedy's observation, as the crisis reached its peak, "It was now up to one single man. No committee was going to make this decision" (p. 208).

Moreover, a key tenet of Model III is that policy outcomes are *resultants*—unintended by any one actor—that emerge out of the dynamics of bargaining. But if policy outcomes are unintended resultants and if Model III accurately describes the ExCom deliberations, one must then conclude that the choice of the blockade was unintended by President Kennedy. But Allison provides no evidence that the president preferred some other option. On the contrary, his story suggests that the blockade was the option most favored by the president and his closest associates; it was not chosen by the president as part of a bargain or political compromise with other ExCom members.[29]

A Model of Bureaucratic Politics Needs a Model of Hierarchy

Model III was intended to synthesize the bureaucratic politics studies of Hilsman, Huntington, Neustadt,

Schilling, and others. What is odd about the model—a model of politics in a hierarchy—is that it says almost nothing about how the hierarchy affects the politics. References are made to "players in positions," "action-channels," and "the rules of the game" (1971, 164–66, 169–71); but it is almost impossible to infer from Allison's brief mention of these variables how they are expected to affect outcomes. This neglect of the impact of hierarchy is surprising, since Schilling (1962)—whom Allison discusses at length (pp. 154–56)—offered some useful insights about hierarchy's impact on policymaking. In his study of the 1950 defense budget, Schilling argued that the shape of the military hierarchy affected who handled which issues. Schilling described several post–World War II plans to reorganize the defense establishment, noting claims that particular plans would reduce the interservice rivalry over the military doctrines justifying particular allocations of missions and budgets. One proposal was to reorganize the land, sea, and air forces into unified functional commands: one was for conducting limited war, one was for all-out war, and one was for continental defense. Such reshuffling, Schilling argued, would not end the key conflicts:

> Some of the issues which had formerly been subject to inter-service argument and determination would become matters for intracommand argument and determination (for example, the Air Force–Navy dispute over the relative merits of sea as compared to land-launched vehicles for the delivery of a nuclear strike would now take place within the confines of the All-Out War Command). Conversely, other issues formerly subjects of intraservice conflict would now become matters of intercommand dispute (for example, the argument within the Air Force over the allocation of resources between tactical and strategic air forces would now be joined between the All-Out War Command and the Limited War Command). (p. 228)

This rearrangement of the issues could affect how the conflicts were settled. As Schilling put it, "What reorganization would change, and it would be a consequential change, would be the political conditions under which these issues would be argued and resolved" (1962, 228). For example, since an organization's design thrusts some issues to the top while pushing others down and since top decision makers care more about some issues than others, different choices might be made in different structures.

Only recently have Schilling's ideas been used to develop a more explicit theory of hierarchy's impact on policymaking. The essential ideas in one set of studies, for example, are that policymaking involves making comparisons (of pieces of information, of policy options, or of proposals for implementation), and that an organization's structure affects who compares what with what, so that different structures can produce different policy outcomes (Hammond 1986, n.d.; Hammond and Thomas 1989). It is well known that a legislature's agenda, which specifies the sequence of voting on amendments, can greatly influence the outcome. An organization's formal structure can, via its impact on the sequence of decisions, have a similar impact on which policies are ultimately adopted.[30]

Allison apparently did judge that hierarchy was relatively unimportant in the missile crisis. He observed at one point, for example, that the ExCom "functioned with minimal reliance on the standard channels down into the second or third levels of the government, causing no little pain to the players left out of the action" (1971, 215). Hence, it may have seemed unnecessary to explore the impact of hierarchy. But what is at issue, we would argue, is whether the hierarchy's *particular configuration* influenced, for example, the CIA's information processing (which Model II treats as an apolitical concern) or the particular policy proposals advanced by the bureaucratic chiefs represented on the ExCom. Without considerably more attention to hierarchy as a variable, Allison could not have determined whether the bureaucratic hierarchy actually had a negligible impact on policymaking.

Logical Problems with Model III's Propositions

Thus far, our argument has been that Model III's conceptual development is inadequate. Some of the model's premises are ambiguous, and it is difficult to determine from Allison's discussion when bargaining will characterize policymaking and who will have to bargain with whom about what. These issues—central to a theory of bureaucratic politics—are left unresolved by Model III. Model III's propositions, which define how it should be applied to data, are no more satisfactory.

A *proposition*, in Allison's usage, is an empirically testable hypothesis that can shed light on the validity of the underlying theory. But it is difficult to know what to conclude from a careful examination of some of his propositions. For example, consider the proposition, "Where you stand depends on where you sit" (1971, 176). Several critiques noted that this proposition contradicts other aspects of Model III (Art 1973, 472–73; Ball 1974, 77; Caldwell 1977, 94; Krasner 1972, 165); Allison himself mentions many other influences, besides bureaucratic position, on the stances of participants. Moreover, empirical evaluation of the proposition is difficult, since, as these critics observed, some key participants do not "sit" anywhere. And even where the proposition can be empirically scrutinized and is found to lack support (as when, in the missile crisis, the secretary of defense initially took a relatively dovish stand despite the hawkish views of many of his uniformed subordinates), it is not clear what conclusions we should draw about Model III more generally.

In fact, some of the propositions seem to be only ad hoc generalizations lacking clear derivation from the underlying model.[31] For example, it is not at all apparent how the claim "In a nuclear crisis, the central decisions will be hammered out *not* in the formal forums, e.g., the National Security Council, but rather by an *ad hoc* group that includes the

President, the heads of the major organizations involved, plus individuals in whom the President has special confidence" (1971, 180) stems from the central argument of Model III, namely, that policymaking proceeds via bargaining. Hence, even if the proposition were found to have empirical support, one's judgments about the model from which it was (supposedly) derived would be largely unchanged. Such a proposition would seem to have little scientific value.

Model III Is Too Complex

Finally, some of Model III's problems stem from its extraordinary complexity. Whereas Allison's formulation of the unitary rational actor model is too "thin," Model III is simply too "thick." It incorporates so many variables that it is an analytical kitchen sink. Nothing of any possible relevance appears to be excluded, as the following passage illustrates:

> A Secretary of State's resolution of these conflicts depends not only upon the position, but also upon the player who occupies it. For players are also people; men's metabolisms differ. The hard core of the bureaucratic politics mix is personality. How each man manages to stand the heat in *his* kitchen, each player's basic operating style, and the complementarity or contradiction among personalities and styles in the inner circles are irreducible pieces of the policy blend. Then too, each person comes to his position with baggage in tow. His bags include sensitivities to certain issues, commitments to various projects, and personal standing with and debts to groups in the society. (1971, 166; emphasis original)

Indeed, after reading chapters 5 and 6 carefully, one is hard pressed to determine what, if anything, escapes the purview of Model III.

An analyst must make some hard choices about what variables a theory should include and what it should exclude. It is often argued that there is a trade-off between explanatory richness (ability to explain one case well) and theoretical generalizability (ability to explain many cases well). Thus, one may conclude that a complex theory, like Model III, can have considerable value. But the nature of this trade-off has been oversimplified: it is possible to include so many variables that the theory does not explain even one case very well. A model that is as complicated as the phenomena it represents is of little use.

This is the trouble with Model III: the relations of so many variables in Model III are left so obscure that it is difficult to use the model even to explain the Cuban missile crisis. In our view, it would have been advisable to pay closer theoretical attention to a smaller number of variables—perhaps just the impact of the domestic political environment and the nature of hierarchical relationships inside the executive branch (with a focus on authority, expertise, and external political support). In general, a model that includes everything explains nothing. If it does not simplify, it cannot explain.[32]

CONCLUSION

Allison's classic book showed an entire generation of scholars and students how to study bureaucracy's role in foreign policy making, and its role in making public policy more generally. *Essence of Decision* made a persuasive case for the use of formal reasoning, for the development of alternative models to explain an important event, for the derivation of testable propositions from the models, and for the testing of the propositions. This approach to bureaucracy was relatively unfamiliar to students of bureaucracy in the 1960s, and Allison's message remains important to this day. It is the rare scholar whose research on organizations and bureaucratic politics has been uninfluenced by the approach advanced by *Essence of Decision*. For these reasons, it legitimately remains one of the most frequently cited studies in the literatures on bureaucracy, foreign policy, and governmental policymaking. If its readers come away with just these methodological lessons, it will remain an important book.

But *Essence of Decision* is more than an exercise in methodology. A very ambitious work, it is simultaneously a treatise on models, a historical study of the Cuban missile crisis, and a heuristic guide to explaining other major foreign policy crises. We admire its reach. But in trying to do so much, it had to accept certain trade-offs.[33] Most importantly, there is an inevitable tension between attempting to explain a particular event (a task characteristic of historians) and attempting to construct models (a job more characteristic of social scientists).[34] The demands of specialization, of allocating one's limited time either to investigating particular historical facts or to developing the mathematical expertise needed in model building, imply that this trade-off is a difficult one. Moreover, these opportunity costs apply to readers as well as authors. Scholars who read *Essence of Decision* primarily to learn more about the missile crisis in particular or cold war diplomacy in general have typically invested much time studying important past foreign policy events. Consequently, they are not trained to read formal models. Thus, Allison would not only have incurred substantial opportunity costs had he tried to transform his analyses into genuine models, but he might also have lost much of his audience. It is, in short, exceedingly difficult for a single volume to be both a work on social science theories and a historical study. A fair assessment of the book must recognize these trade-offs and its ensuing compromises. Nevertheless, we are forced to conclude that the price paid by *Essence of Decision* on the theoretical dimension was that due to its attention to historical detail, it paid insufficient attention to the internal logic of the models.

Thus, a close examination of these models shows that they are much less rigorously formulated than is generally recognized, that the derivations are in some cases wrong and in others do not follow from the models, and that the meaning of the empirical tests is often quite ambiguous. Hence, the continued refer-

ence to, and use of, *Essence of Decision* has a rather troubling character. Aside from its important methodological points, what scholars and students have been learning from the models may lack foundation and, in some cases, may simply be incorrect. We conclude by emphasizing five of these problems.

First, modeling generally begins with some hunches of the theorist—or with the claims of the academic literature—about how the world works. The model should then reflect these judgments. But none of Allison's models are, in our view, sufficiently accurate renditions of the literatures that he himself cites in justification for the models. The traditional literature on war, crisis, and international politics has emphasized uncertainty and state-versus-state gaming. Model I almost ignores these properties. The organizational literature of Simon and March portrayed standard operating procedures and routines as *enabling* choice and action. Model II depicts them as *constraining* choice and action. The understanding of politics-in-hierarchies, available in the bureaucratic politics literature by the late 1960s, is not accurately reflected by Model III's analysis of bureaucratic bargaining. The role of hierarchy, which presumably structures the entire game of bureaucratic politics, is neglected—and the critical interplay between authority and expertise in a bureaucracy, a major topic of discussion in organization theory since Weber, receives almost no attention at all.

Second, in laying out his three models of policymaking, Allison has generally been credited with advancing the important point that intellectual progress comes from formulating, testing, and evaluating alternative models. Yet his presentation of Model I suggests that he saw it, at least in part, as something to be set up in order to be knocked down. This may explain Model I's inadequate development, even given the relatively crude state of the art of game theory and rational choice models when Allison was writing. Nonetheless, one cannot seriously evaluate the relative performance of two new models (II and III) if the baseline model is almost destined to fail. For this reason, what is often taken to be a major lesson of *Essence of Decision*—that rational choice explanations of policymaking are weaker than those of Models II or III—lacks foundation. Whatever their ultimate worth, rational choice models were not given a fair test.

Third, Allison's overall approach—the development of explicit models of policymaking—was based on the view that clarifying one's key assumptions is analytically helpful. Yet the assumptions of Models II and III were sufficiently ambiguous that it is difficult to discern the models' defining properties. Hence, it is unclear whether Models II and III belong to separate classes of theories or not.

Fourth, one of the purposes of developing an explicit model is rigorously to derive the logical implications of one's fundamental assumptions. Yet for neither of the two models (II and III) for which *Essence of Decision* is best known are the propositions rigorously derived. The general proposition to be drawn from Model II—that simple rules lead to simple, predictable behavior—is almost surely wrong; and Model III is so complicated that virtually no propositions can be rigorously derived from it at all.

Fifth, if propositions are not rigorously derived from a model yet receive some evidential support, it is difficult to know what one should learn from the empirical corroboration. We may learn something about the propositions; but lacking any logical relationship between them and the model, the empirical test teaches us little about the model or the hunch that originally generated it.

Some of the inadequacies of Allison's models stem merely from the fact that there has been considerable progress in the understanding of game theory and rational choice models, in the appreciation of the role of uncertainty and incomplete information, and in development of insights about organizations, hierarchy, and bureaucratic politics. Indeed, we have been at pains to point out what literature was available to Allison—and what was not—when he wrote his book. Yet many of the flaws we have pointed out in *Essence of Decision* do not stem from advances in these fields: they were there from the beginning, and a reasonable assessment of the book must recognize this fact. Since the book still has considerable currency in political science and in a wide range of other disciplines, continued use is thus likely to lead to the widespread perpetuation of major misunderstandings about the nature of bureaucracy and governmental policymaking. If the academic community is not made aware of these flaws, reliance on Allison's models is as likely to lead to error as it is to inform. Nonetheless, *Essence of Decision* richly deserved its two decades of intellectual prominence. The fact that the study of bureaucracy, organization, and foreign policy making has gone beyond it detracts in no way from its remarkable accomplishments.

Notes

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1. Perlmutter (1974) distinguished three different models, as did Snyder and Diesing (1977) and Steinbruner (1974). Kurth (1971) analyzed five models, Kohl (1975) six, while Caldwell (1977) distinguished 12 models! Allison himself cited Steinbruner's contribution of additional models and noted that "a number of others are clearly possible" (1971, 255; see also pp. 276–77).

2. E.g., in the 1990 *Social Science Citation Index*, *Essence of Decision* and Allison's 1969 article were cited 84 times in 58 different journals.

3. Though classical decision theory emphasized prospective uncertainty, retrospective uncertainty is also prevalent in crises: Did the enemy just launch a first strike, or are we picking up a flock of geese on our radar screen? This example also points up the connection between the two types of uncertainty: if one is unsure what one's opponent has done,

one will also probably be unsure how he will react to one's own move.

4. For just one example (which could have initiated a nuclear exchange), recall the U-2 that blundered into Soviet air space during the height of the Cuban missile crisis. As President Kennedy was reported to have remarked about this incident, "There is always some son-of-a-bitch who doesn't get the word" (quoted in Allison 1971, 141).

5. Even if individual attention were unconstrained, costly communication would mandate this issue. As Simon (1973) suggests, however, attention is often a major bottleneck.

6. Given this assignment, Model I should not be seen as the only rational choice model in *Essence of Decision*.

7. There is a tendency to jump to such claims, particularly regarding unitary rational actor models; but we shall argue that these claims oversimplify the models' implicational richness.

8. Ball argues that Model III "is the model closest to Allison's own heart," citing a seminar discussion at Harvard in October 1972, in which "Allison himself made this quite clear" (1974, 76).

9. Cognitive analyses (e.g., Steinbruner 1974) recognize that decision makers may sometimes fail to resolve intrapersonal conflict in a consistent manner.

10. In redundancy theory, one often analyzes the optimal amount of duplicate men and materiel to achieve a task when implementation is uncertain. These analyses are facilitated by assuming a single decision maker whose objectives are to be maximized.

11. Indeed, Blight and Welch (1989) present ample evidence that even latter-day *Soviet* analysts disagree about Khrushchev's motives.

12. Allison does challenge this notion, but only in discussing Model III in respect to bargaining *inside* governments.

13. Mixed strategies are covered in game theory texts going back to Luce and Raiffa 1957, chap. 4; see also Schelling 1960. Note that in the inspection game and similar two-player games, the mixed strategy equilibrium is replaced by a pure strategy equilibrium if the players move sequentially. The indeterminacy of a mixed strategy equilibrium does require some exogenously assumed uncertainty (i.e., the imperfect information occasioned by simultaneous play).

14. Clearly, the optimal outcome for the United States is to deceive the USSR, convincing the Soviets that we will inspect closely, but not to do so. A similar point about deception applies to the Soviets. Neither optimal outcome involves a mixed strategy. However, this does not invalidate the text's point, since neither outcome is an equilibrium.

15. For example, Cyert and March describe the general procedure—"Use simple rules"—as one of the "three basic principles" of choice (1963, 102). And certainly, the best-known rule in this literature, *satisficing* (searching for alternatives until finding one that exceeds a specified threshold) is a very simple procedure (Simon 1957, 204–5, 252–53).

16. March and Simon, who pioneered the metaphor of organizational behavior as governed by computer programs, recognized that behavioral flexibility could arise from a program's sensitivity to inputs: "The term 'program' is not intended to connote complete rigidity. The content of the program may be adaptive to a large number of characteristics of the stimulus that initiates it. Even in the simple case of the fire gong, the responses depend on the location of the alarm" (1958, 142). Again, "At the [programmed] limit, an environmental stimulus may evoke immediately from the organization *a highly complex* and organized set of responses" (p. 141, emphasis added).

17. What follows is therefore a conservative analysis that underestimates the complexity of the armed services' *rules* for research-and-development and procurement.

18. This view of Allison's appears to have influenced other international relations scholars. For example, Keohane wrote that "If individuals typically satisfice rather than maximize, all the more so do governments and other large organizations (Allison, 1971; Steinbruner, 1974; Snyder and Diesing, 1977)" (1984, 114).

19. Allison mentions this advantage once (1971, 80). He does not, however, emphasize it.

20. "If the SOPs are appropriate, average performance—i.e., performance averaged over the range of cases—is better than it would be if each instance were approached individually (given fixed talent, timing, and resource constraints)" (Allison 1971, 89). He immediately adds, however, "But specific instances, particularly critical instances that typically do not have 'standard' characteristics, are often handled sluggishly or inappropriately" (ibid).

21. See also: "An important objective of standardization is to widen as far as possible the range of situations that can be handled by combination and recombination of a relatively small number of elementary programs" (March and Simon 1958, 150).

22. In a striking essay, Norton Long also differed sharply with the viewpoint of *Essence of Decision*, chapter 4, arguing that it is the *bureaucracy* that via its "fact-finding [and] policy proposal . . . procedures," must discipline the tendency of *politicians* to manipulate "a free-wheeling world of rhetoric and emotion, . . . a world in which solid facts evaporate" (1954, 28). It is interesting to note that though Allison cited this essay in respect to Model III (1971, 315, n. 65), neither Model II nor Model III was influenced by its main theme.

23. Surprisingly, neither chapter 3 nor chapter 4 specifies an explicit baseline of evaluation: using routines and standard operating procedures constrains organizational behavior *compared to what*? To an organization that though still staffed by boundedly rational actors, did not use standard operating procedures? Or to how an organization with perfectly rational members would behave? Or to how a perfectly rational *individual* would behave? Since the answer is unclear, the reader is left to infer the baseline of comparison.

24. We do not underestimate the difficulty of determining whether actors' disagreements are due primarily to conflicting goals or to conflicting beliefs. In a debate involving a president, insiders whose goals differ from the president's may find it wise to pretend their disagreements are about means. As March and Simon observe about organizations, "Bargaining (when it occurs) will frequently be concealed within an analytic framework" (1958, 131).

25. After criticizing what he considers Allison's overemphasis on bargaining in foreign policy formation, Rourke does remark that "in domestic policy, on the other hand, models structured around bargaining are much more useful, since executive organizations here compete with the President on a relatively equal footing" (1972, 432).

26. While Allison cites Dahl and Lindblom in support of his point that Huntington overemphasized participant equality (1971, 315, n. 65), it is interesting to note that Dahl and Lindblom consistently maintain that outside political support enables subordinates to bargain with the president (1953, 341–44).

27. In the missile crisis, for example, President Kennedy was apparently uncertain whether an air strike would destroy *all* the Soviet missiles in Cuba. When he sought advice from the Tactical Air Command, its commander told him that no such guarantee was possible. This judgment apparently played a major role in Kennedy's ruling out the air strike option.

28. To be sure, there were leaks to the press from inside the executive branch *before* the president and his top advisors became convinced that Soviet missiles were indeed in Cuba (Allison 1971, 192). Once the ExCom was convened, however, Allison mentions nothing further about leaks intended to influence policymaking.

29. Our conclusions here are in line with those of most of Allison's critics; see, esp., Art 1973; Ball 1974; and Krasner 1972.

30. We are not suggesting that politics occurs only within formal channels; evidence on the importance of informal organization is too strong to ignore. We do think, however, that the informal relationships that do emerge will be efforts to overcome the impact of the formal structure. Thus, formal structure shapes informal structure.

31. Allison himself bemoans "the difficulty of formulating Model III propositions about outcomes" (1971, 173).

32. We are reminded of Lewis Carroll's (1893) parable about map-making: "That's another thing we've learned from your Nation," said Mein Herr, "map-making. But we've carried it much further than you. What do you consider the largest map that would be really useful?" "About six inches to the mile." "Only six inches!" exclaimed Mein Herr. "We very soon got to six yards to the mile. Then we tried a hundred yards to the mile. And then came the grandest idea of all! We actually made a map of the country, on the scale of a mile to the mile!" "Have you used it much?" I enquired. "It has never been spread out, yet," said Mein Herr: "The farmers objected: they said it would cover the whole country, and shut out the sunlight! So we now use the country itself, as its own map, and I assure you it does nearly as well."

33. For a more detailed discussion of these trade-offs and of the great heuristic value of conceptual frameworks (partially formalized models), see Bendor and Hammond 1989.

34. For evidence that Allison took the historian's job of explaining the missile crisis seriously, see 1971, 1–2. Here he asks four central questions. The first three—Why did the Soviet Union place strategic offensive missiles in Cuba? Why did the United States respond with a naval quarantine? Why were the missiles withdrawn?—concern particular aspects of this particular event. As such, they are indistinguishable from those posed by historians.

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ON THE NATURE OF SUPREME COURT DECISION MAKING

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How does the U.S. Supreme Court reach decisions? Since the 1940s, scholars have focused on two distinct explanations. The legal *model suggests that the rule of law (stare decisis) is the key determinant. The* extralegal *model posits that an array of sociological, psychological, and political factors produce judicial outcomes. To determine which model better accounted for judicial decisions, we used Supreme Court cases involving the imposition of the death penalty since 1972 and estimated and evaluated the models' success in accounting for decisional outcomes. Although both models performed quite satisfactorily, they possessed disturbing weaknesses. The legal perspective overpredicted liberal outcomes, the extralegal model conservative ones. Given these results, we tested another proposition, namely that extralegal and legal frameworks present codependent, not mutually exclusive, explanations of decision making. Based on these results, we offer an integrated model of Supreme Court decision making that contemplates a range of political and environmental forces and doctrinal constraints.*

> The common law's . . . reverence for . . . *stare decisis* [means] that a policy made in a piecemeal manner would be better able to win support. . . . When the Court reverses itself or makes new law out of whole cloth . . . the holy rite of judges consulting a higher law loses some of its mysterious power.
>
> —Walter F. Murphy

> Judges are inevitably participants in the process of public policy formation; . . . they do in fact "make law"; [and] in making law they are necessarily guided in part by their personal conceptions of justice and public policy.
>
> —C. Herman Pritchett

How does the U.S. Supreme Court reach decisions? The above statements answer that question quite differently. The first, grounded in positivist jurisprudence, suggests that the rule of law stare decisis is the key determinant. The second, reflecting political jurisprudence, posits the converse—that an array of sociological, psychological, and political factors having little to do with legal doctrine produce judicial outcomes. These perspectives represent the mainstream—albeit highly divergent—responses to the question we pose. That both have persisted, despite the obvious tension between them, is somewhat puzzling; that the subfield of law, courts, and judicial processes may be more polarized than ever is extraordinary. While Hensley and Rhoads (1988) reported that the political (extralegal) model dominates disciplinary research, Slotnick (1989) noted the opposite trend—that the studies conducted by entering professionals reflect a return to doctrinal (legal) analysis.

Can two highly divergent conceptualizations of judicial decision making coexist? Empirical evidence suggests that at minimum, each can account for variations in behavior. Pritchett and many other adherents of extra legalism have more than ably demonstrated the explanatory power of their approach (e.g., Pritchett 1948; Schubert 1954; Spaeth 1961, 1979; Ulmer 1960). Segal and others present equally compelling results supporting the legal model (e.g., Hagle 1989; Kort 1957; Segal 1984; Wolpert 1991).

Yet for all the research conducted and all the years this debate has raged, it is almost startling to find that scholars have yet to compare these basic models systematically.[1] Pritchett and the extralegalists virtually ignore doctrine; Segal and others consider some attitudinal variables, but only after controlling for doctrine.

How legal and extralegal models of judicial decision making perform in head-to-head competition forms our central concern here. Using Court cases involving the imposition of the death penalty since 1972, we estimate and evaluate the ability of each to account for decisional outcomes.

CONCEPTUALIZATIONS

In his classic study of the various schools of jurisprudential thought, Pound (1959) identified over 20. Today, we probably could add 4 or 5 more (see Carter 1985; Stumpf 1988). In fact, debates—some more heated than ever—over how judges and justices *should* contemplate cases continue to occupy a preeminent position in legal and political circles (see "Critical Legal Studies Symposium" 1984; Posner 1981). We, on the other hand, are concerned with how justices *do* reach decisions. On some level the answers to this would probably rival in number those to the more normative query. To be sure, many—if not most—of the various jurisprudential schools find some representation within the nation's vast judicial apparatus. Within the political science literature, however, only two have received serious theoretical

and analytical attention: the legal and the extralegal models.

The Legal Model

Often termed *positivist,* or *analytical, jurisprudence* (see Austin 1904), the *legal model* was the primary modus operandi for political scientists through the 1940s and continues to dominate legal education in the United States. At its core, legalism centers around a rather simple assumption about judicial decision making, namely, that legal doctrine, generated by past cases, is the primary determinant of extant case outcomes. It views judges as constrained decision makers who "will base their opinions on precedent and will adhere to the doctrine of *stare decisis*" (Wasby 1988, 210). Some scholars label this *mechanical jurisprudence,* because the process by which judges reach decisions is highly structured. Levi (1949) describes the basic pattern of legal reasoning, reasoning by example, as a process characterized by the doctrine of precedent and articulated in three steps: (1) observation of a similarity between cases, (2) announcement of the rule of law inherent in the first case, and (3) application of that rule to the second case.

Legal education and scholarship adopted this as a paradigm—the process by which judges and lawyers should proceed. Eschewing normative approaches, political scientists (from the 1920s through the 1950s) instead viewed "reasoning by example" as the way judges *do* proceed. Cushman (1929), Corwin (1924), and many others centered their work on the notion that previously announced legal doctrine provided the single best predictor and explainer of Court decisions.

How this model became so entrenched in the political science literature is readily discernible. Many scholars reasoned that judges (all schooled in legalism) would naturally gravitate to it upon ascension to the bench. After all, how else would they approach decision making? In addition, strong expectations exist within the legal community that judges will reason by example. As Murphy (1964) suggests, following legal precedents is a well-entrenched norm of the deliberative process. Finally, the case studies of the day reinforced the model's value. Articles published in political science journals summarized the reasoning used and the precedents set by the justices, disregarding any other factors contributing to decisional outcomes. Cushman's analysis of constitutional law for 1936–37 (one of the most volatile terms in the Court's history), provides a wonderful example. After acknowledging "The 1936 term of the Supreme Court will probably be rated as notable," he enumerated some of the "facts . . . one should bear in mind"—that Roosevelt had won a landslide reelection and had submitted a plan to reorganize the Court. Rather than demonstrate how those "facts" might have affected Court decisions, however, Cushman noted simply that "no suggestion is made as to what inferences, if any, might be drawn from them" (1938, 278) and proceeded to analyze the New Deal cases vis-à-vis existing precedent (a difficult task, indeed).

The Extralegal Model

While the legal model was predominating political science thinking about the Court, new perspectives emerged from the ranks of the nation's judiciary and law schools. In general, these thinkers denounced legalism as mechanical jurisprudence—sometimes subtly (e.g., Holmes 1881), sometimes loudly (e.g., Pound 1931). Instead, they beckoned judges to consider more dynamic factors as bases for decisions. Many credit Holmes's (1881) *The Common Law* with initiating this plea. Students of this school often cite as exemplary his remark that "the life of the law has not been logic; it has been experience. . . . It cannot be dealt with as if it contained only the axioms and corollaries of a book of mathematics" (1–2). Illustrative, too, is Louis Brandeis's famous brief in *Muller* v. *Oregon* 1908, containing 113 pages of sociological data but only 2 of legal argument. It was Pound (1931), however, who catalyzed the first strain of extrajudicialism, sometimes called *sociological jurisprudence.* In his seminal *Harvard Law Review* article, Pound drew his now-famous distinction between "law in books" and "law in action," behooving judges to adopt the latter, without necessarily abandoning the former. Cardozo and many others followed suit.

Later adapters of sociological jurisprudence—the realists of the 1930s—though were far more radical in orientation, maintaining that rules based on precedents were nothing more than smokescreens (see Frank 1930, 1950; Llewellyn 1951) or "myths, clung to by man out of a childish need for sureness and security. A mature jurisprudence recognizes that there is no certainty in law" (Stumpf 1988, 16).

So began a long line of thinkers who harshly critiqued legal reasoning for its inadequacy as a basis for judicial decision making, an inadequacy stemming from various considerations. From a normative standpoint, many followed Brandeis's lead, arguing that extralegal factors should enter judicial deliberations. After all, if judges were constrained by precedent, law would remain static when it should reflect changing morals and values. Additionally, critics argued that justices, "like other human beings, are influenced by the values and attitudes learned in childhood" (Frank 1950). It would be extraordinary, they claimed, to think that judges, just because they don black robes, were any less susceptible to such influences. Indeed, justices may be even more vulnerable than other decision makers because the rules of law are "typically available to support either side" (Pritchett 1969, 31). In making choices between competing precedents, then, other factors are bound to come into play.

Although legal realism gained a strong following within the nation's leading law schools during the 1930s, political scientists were reluctant adherents. It was not until the publication of Pritchett's (1948) *Roosevelt Court* that students began to abandon a

positivist approach and view Court decisions more critically and analytically. In essence, Pritchett brought legal realism to political science. He equipped scholars with the arsenal necessary to estimate and evaluate its propositions. And, as an important by-product, he provided the fodder for the development of political jurisprudence and behavioralism—a development more fully stylized and realized by Schubert (1954), Spaeth (1961), and Ulmer (1960), who incorporated the assumptions of realism into their models of judicial decision making. Like Frank, they viewed the Court's environment as one that provided the justices with "great freedom to base their decisions solely upon personal policy preferences" (Rohde and Spaeth 1976, 72). But unlike the realists, they proceeded operationally to define and systematically to test attitudinal models of judicial behavior.

MODELS: CURRENT STATUS

The Extralegal Model

The underlying premise of *The Roosevelt Court*—that a host of internal, attitudinal factors other than "the law" contribute to case outcomes—continues to constitute a mainstay of research on Supreme Court decision making. Over the past decades, countless efforts have viewed court decisions as a function of personal attributes (e.g., Tate 1981), attitudes (Rohde and Spaeth 1976), and role conceptions (Gibson 1978). In general, however, most of these depict a Court driven by single-minded seekers of legal policy, justices who wish to etch into law their personal views.

Though the basic tenets of behavioralism continue to shape our vision of the judicial process, we have made some attempts at refining that conceptualization. In general, we now know that Court decisions are a product of factors not only internal but also external to the Court. Research by myriad scholars reveals that litigants, attorneys, and parties can affect case outcomes (e.g., Galanter 1974; Vose 1959). Although this body of literature is, conceptually speaking, quite murky, we acknowledge the validity of several of its basic premises. First, we recognize that attainment of "repeat player status" can translate into litigation success (Galanter 1974). How one becomes a "repeat player" is difficult to specify; but at least two factors contribute: association with a national litigating organization and regular appearance before the Court. Attorneys from the ranks of public interest law firms are ensured some level of financial and other support that many private counsel lack (Vose 1959). Repeated and continuous use of legal recourse breeds familiarity, confidence, and expertise that one-shotters cannot possibly acquire (O'Connor 1980). Second, and concomitantly, we acknowledge the extraordinary role played by one repeat player, the solicitor general of the United States. Whether participating as a direct party or as an amicus curiae, solicitors general seem to carry great weight with the justices. Their presumed legal expertise, their utility in preconference screening, and their embodiment of the interests of the nation are just some of the factors explaining the solicitors' general excellent record before the Court (Segal 1991). Third, we accept the proposition that appellants are more likely to win their cases than appellees. This expectation is grounded both in fact (Wasby 1988) and in theory (Palmer 1982). Thus, no extralegal model can afford to ignore it.

Political scientists also recognize that pressures emanating from outside the legal system can affect case outcomes. In particular, we believe that the Court is responsive to the larger political environment. Some suggest that its rulings reflect the wishes of the citizenry (e.g., Barnum 1985). But owing to difficult conceptual and analytical obstacles, scholars have yet to provide truly compelling support for this assertion (see Caldeira 1991, 313–16). They have been more successful at establishing a link between the Court and the other branches of government. That is, even though the Justices lack an electoral connection or mandate of responsiveness, we have several reasons to suspect that Congress and the president affect Supreme Court decision making (Casper 1972; Murphy 1964) and that the direction of their influence reflects their partisan composition (Nagel 1969). First, scholars assert that both Congress and the president possess a vast array of powers over matters important to the Court, which the Justices can hardly afford to ignore (Baum 1989a, 133). Some of these powers, such as presidential and senatorial roles in the appointment process, are obvious; some are not. As Wasby (1988) notes, Congress can restrict the Court's jurisdiction to hear cases; enact legislation, or even propose constitutional amendments, to recast Court decisions; and hold judicial salaries constant. In turn, "the Court's policies may be affected by a desire to deter Congress" from taking these steps (Baum 1989a, 133). By the same token, Murphy (1964) and Scigliano (1971) point to the multifaceted relationship between the Court and the president, one that goes well beyond his power to nominate justices or even to send his solicitor general into the Marble Palace. It is no secret, for example, that many presidents have enjoyed special relationships with sitting justices and that presidential preferences have found their way into particular opinions. Nor can we ignore the role the president plays in shaping congressional responses to the Court.

Another set of literature focuses on the role of the Court within a democratic society, many scholars arguing that Court decisions do not significantly deviate from the preferences of the existing majoritarian regime. Though some argue that this is largely a result of the regularity with which presidents appoint justices (e.g., Dahl 1957), others suggest that it stems, at least in part, from the institutional nature of the Court (Marshall 1989; Murphy 1964). In short, because the justices lack any real mechanism to enforce their decisions, they depend on other politi-

cal actors (and the public) for support of their positions (Johnson and Canon 1984). Without such support, the Court could lose its institutional legitimacy, a danger of which its members seem well aware (Caldeira 1991). Accordingly, for the Court to have power within government it must "follow the election returns," or at the very least, consider them (Barnum 1985). Along similar lines, the Court, at least on occasion, takes cues from the political environment to guide its decisions (Vidmar and Ellsworth 1974). It has even gone so far as to build that dimension into some of its jurisprudential standards. In evaluating whether certain kinds of punishments violate the Eighth Amendment's cruel-and-unusual clause, for example, the Court proclaimed that it would look toward "evolving standards of decency" as defined by public morals.

Given this literature, then, we have sufficient reasons to suspect that the president and Congress can affect Supreme Court decision making or at least that Court rulings encapsulate the preferences of the other branches. What is more, we know something about the direction of this influence. The relationship between the Court and other branches of government, throughout history, has revolved around the ebb and flow of partisan politics. As Nagel observed, these patterns are "significantly shaped by such political considerations as the degree of party difference between Congress and the Court, the nature of the party in power in the national government and the party affiliations" of the individual Justices. (1969, 259). Put in more concrete terms, when the nation and its institutions move toward parties representing "have not" views (e.g., today's Democratic party), the Court tends to adopt more liberal policy postures, particularly on issues of rights and liberties. Conversely, as parties of "haves" (e.g., today's Republican party) come into power, the opposite effect will occur (Epstein, Walker, and Dixon 1989; Nagel 1969). In sum, while we continue to incorporate the premises of the attitudinal approach into our thinking about behavior, we also recognize that pressures external to the Court (be they from attorneys, parties, and litigants or from the larger political environment) supplement behavioral assumptions. We can express this extralegal model as

$$\Pr\ (C_i = 1) = b_0 + \Sigma b_h EL_{ih} + e, \qquad (1)$$

where C_i is the Court's decision in a given case (a binary variable equal to 1 if the Court rules in a conservative direction, 0 otherwise); $\Pr(C_i = 1)$ is the probability of a given Court decision taking a conservative posture; and EL_{ih} are the extralegal variables influencing that decision.

The Legal Model

It is, of course, true that many of those engaged in the study of public law are less than content with this extralegal model, calling for a greater emphasis on doctrine as a valid explanation of case outcomes. Brigham (1978), Johnson (1987), and O'Neill (1981) have stressed that the language of law (precedents, etc.) channels and constrains judicial choices. Shapiro (1972), too, suggests that "we have overemphasized extralegalism to the point [that] we somehow ignore the fact that appellate courts and the lawyers that serve them spend an overwhelming proportion of their energies communicating with one another, and that the judicial opinion itself conforming to the style of *stare decisis*, is the principal mode of communication." Two decades after publication of *The Roosevelt Court*, Pritchett even lamented that behavioralism may have gone too far: "Political scientists who have done so much to put the 'political' in 'political jurisprudence' need to emphasize that it is still 'jurisprudence'. It is judging in a political context, but it is still judging; and, judging is something different from legislating or administering" (1969, 42).

Like behavioral models, legal perspectives have evolved over the years, not so much conceptually as analytically.[2] In particular, work by Kort (1957) and Segal (1984, 1986) demonstrated systematically—not contextually—that the law controls case outcomes. Rather than focus on a specific decision or strain of precedent, Kort and Segal isolated areas of law and used facts and legal doctrine, respectively, to predict judicial determinations. When they found that the Court responded to legal factors in a consistent manner, some social scientists began to question the sweeping generalizations of the behavioralist school.

Kort's (1957) work has been the subject of some criticism. Fisher (1958) chastised his methods; others found his use of legally irrelevant facts puzzling.[3] Segal's analyses overcame some of these shortcomings. He used probit (a multivariate analytic strategy), which allowed him to reach more precise conclusions about the relationship between doctrine and output. In addition, he limited his consideration to facts of legal significance. Virtually all his variables tap doctrine, rather than attributes of the sort incorporated by Kort.[4] Indeed, he labels his analysis "a legal model."

Even so, Segal's model (and similar ones, e.g., Hagle 1989; Ignagni 1990; Wolpert 1991) arguably failed to correct for some of the shortcomings of previous work. Scholars continue to construct legal models in a rather post hoc fashion. Segal, for example, included certain variables (e.g., warrantless searches made prior to arrest and consent searches) because the *Court* said they were important and not because he had a priori knowledge that they should be significant in search-and-seizure analysis. Another potential problem is more temporal in nature: most legal models incorporate doctrine that the Court enunciated *after* it decided some of the cases contemplated in a given analysis. Segal included a variable comprised of six "exceptions" to the warrant and probable cause requirements. Although the Court had not written any of these exceptions into law prior to 1967 (the balance came in the early 1970s), Segal incorporated them into a model developed to explain

Court search-and-seizure decisions between 1962 and 1981.

Do these problems undermine the efforts of Segal and those following in his footsteps? We think not. On the contrary, they are endemic to legal models in general, rather than to specific scholarly efforts. Consider the temporal problem from the perspective of those who view the law as self-contained (as it is often presented to be in law school courses). Because, for them, the core notion is that all decisions are rooted in stare decisis, they would simply respond that the old doctrine anticipated the newer one, that the facts and questions varied, not the doctrine. They would bring much the same argument to bear on the post hoc nature of these models.

Seen in this light, the approach taken by Segal and others (Hagle 1989; Ignagni 1990; Wolpert 1991) neatly parallels the conceptualization offered by legalists. What is more, by providing empirical verification for the legal model, they gave it greater status within the scientific community studying judicial decision making.[5] Though their methods differed from those of the classicists and they often included attitudinal measures in their models, their conclusions lent support to traditional assumptions about the nature of legal deliberations. More formally, we could conceptualize this model as

$$\Pr(C_i = 1) = b_0 + \Sigma b_j L_{ij} + e, \qquad (2)$$

where L_{ij} are the legal factors impinging on the probability of a conservative decision and the other symbols are as in equation 1.

ESTIMATING THE MODELS

Since both legal and extralegal models are well established theoretically and analytically, we evaluate each on two scores: the typical parameter estimation strategy[6] and a strategy based on the ability of the models to classify Court decisions correctly. For this examination, we focus on almost two decades' worth of cases involving capital punishment (N = 64), measuring Court outcomes as a simple binary choice whether it affirmed the imposition of the penalty (coded 1) or not (coded 0). Data are from the *U.S. Reports* for the 1971–88 terms. The mean value across terms is .45 in favor of the death penalty.

In general, our task in operationalizing the independent variables for both the legal and extralegal models is rather straightforward. As we shall describe in some detail, we simply adapt past constructs to our data, rather than improve upon them or introduce *major* innovations. Given our interest in comparing two well-entrenched (and empirically verified) models, we believe this to constitute the most evenhanded approach. Nonetheless, as our discussion has indicated, we are not unaware of the shortfalls of past research exploring the relative effect of extralegal factors and legal doctrine on decision making. There is still debate, for example, over whether factors exogenous to cases actually affect the Court's decisions. By the same token, while Segal's (1984) work markedly advanced the line of inquiry on the impact of doctrine, that does not necessarily mean that it and its progeny are perfect. Nor can we ignore the perspective that *any given* Court producing *any given* legal doctrine is composed of justices possessing particular attitudes and so forth. Thus, a certain degree of interaction undoubtedly exists.

We shall take up some of these concerns later. Our present task is to examine the relative performance of the models as models. In so doing, and for the reason suggested, we feel it important to follow a research tack similar to those which preceded us in terms of the development of the model. With this in mind, we now turn to our specific operationalizations of both models.

The Legal Model

To develop a legal model, we followed Segal's lead. In his research on search-and-seizure cases, he reviewed Court doctrine relating to these suits and specified a concomitant legal model around that doctrine. This strikes us as a sensible approach, one that can clearly delineate whether "the law" determines case resolution. In general, the Court has centered its inquiry into capital punishment on the decision-making process, that is, the decision to impose death or not.[7] More specifically, it has promulgated legal doctrine in response to three orienting questions. First, upon whom can decision makers confer a sentence of death? Rape[8] and, in fact, all crimes for which the defendant did not intend to murder are undeserving of capital punishment. Hence, if the accused was tried for anything less than intentional murder,[9] we would expect to find the Court resolving the case in favor of the criminally accused. To test this proposition, we coded the alleged crime (CR) as one for which a sentence of capital punishment was proportional to the offense (coded 1) or not (coded 0).

Second, who can make the decision between life and death? Although the Court allows judges and/or juries to impose a sentence of death,[10] it has made clear that the latter cannot be biased toward such a verdict. Juries, in short, cannot be *death-qualified* (DQ)[11] that is, the "death penalty may not be imposed if the jury that assessed it was selected so as to exclude anyone who expressed general objections to capital punishment or religious or conscientious scruples against it" (Whitebread and Slobogin 1986, 637). To determine whether the justices responded consistently to issues of death-qualified juries, we coded whether this claim was raised (coded 0) or not (coded 1). If it was part of the legal record, we expect the Court to find in favor of the defendant.

Finally, what factors should decision makers consider in their deliberations? Clearly, judges and juries must, in an evenhanded manner, consider whether a penalty of death is the appropriate one. For instance, they must look at the particularized circumstances

(PC) of the case that may favor the defendant's position.[12] Along similar lines, the death penalty cannot be made mandatory,[13] nor, conversely, can it be applied in an arbitrary (e.g., racially discriminatory) manner.[14] Certainly, then, we would expect the Court to look unfavorably on any attempt to limit factors idiosyncratic to the crime or the accused (coded 0 if present, 1 if not). Concomitantly, judges and juries should contemplate particularized aspects of the crime against those factors in aggravation (AG). Unlike factors mitigating against death, those in aggravation must be specific and appropriate.[15] If they are overly broad or vague, the Court should be inclined to rule against the state. We coded the presence of such a claim as 0, the absence as 1. In addition, juries and judges often consider, at the prosecution's urging, results of state-initiated psychiatric (SP) examinations. The Court, however, has clearly indicated that these must be conducted in accord with strict norms of due process and fairness.[16] We posit, then, that a claim suggesting otherwise (coded 0) would weigh in favor of the accused.

Given these definitions, we can now rewrite equation 2 in the following operational terms:

$$\Pr(C_i = 1) = b_0 + b_1CR_i + b_2DQ_i + b_3PC_i + b_4AG_i + b_5SP_i + e.$$

We expect b_1, b_2, b_3, b_4, $b_5 > 0$. Like Segal, we recognize that some death penalty cases "often have idiosyncratic factors that affect the Court's decision and [that these] factors do not determine the Court's resolution of any case" (1984, 893). Yet we also argue that the factors considered here should strongly predispose the Court toward finding against the imposition of the death penalty.

This noted, one issue remains—the data collection process. Because it is likely that Court opinions filter out specific claims, they are not (for our purposes) reliable or useful sources of information. Accordingly, scholars seeking to estimate legal models turn to outside sources, such as the briefs filed by participating attorneys or the decisions of lower courts. Because attorneys narrow the array of existing legal arguments from which the justices can select to focus their opinions, we used their claims as our primary data source. For some of the doctrinal variables encompassed in our legal model (e.g., death-qualified juries), however, we found it necessary to consult the lower court opinions to ensure that they were, in fact, part of the legal record.

The ExtraLegal Model

Given previous successes at estimating extralegal models, our task is rather straightforward. As we did for the legal model, we simply adapt past constructs to our data. We earlier demonstrated that three elements compose this perspective: the justices' internal predispositions, litigants, and the political environment. Although previous research accounts for internal value changes in diverse ways, one acceptable procedure operates under the assumption that as the number of Nixon, Ford, and Reagan appointees increased, so should the number of conservative (in our case, pro–death penalty) outcomes (e.g., Segal 1984). We can, then, operationalize this Court change (CC) variable as an additive index, taking on the value of 0 until the appointment of Stevens, 1 after Stevens, 2 with the appointment of O'Connor, 3 after Scalia, and 4 with Kennedy.[17]

Measuring the literature's expectations about the role of parties in litigation means adding of four factors. First, we consider whether attorneys representing defendants were experts in death penalty litigation. Following the lead of others, we assume that defense counsel (DC) from the ranks of public interest law firms or interest groups (in our study, virtually all were from the ACLU or the NAACP Legal Defense Fund) and those appointed by the Court were experienced death penalty litigators (coded 0), otherwise not (coded 1). Second, to avoid underestimating our model, we considered the relative expertise and repeat player status of the states (ST). We reasoned that the more a state litigated in this particular area, the greater its expertise (see, generally, Epstein and O'Connor, 1988). Three of the 18 states involved in death penalty litigation—Texas, Georgia, and Florida—accounted for nearly 50% of all cases, each litigating roughly 10 of them. We coded them as repeat players (1), all others as 0. Third, we ascertained whether the solicitor general (SG) had filed a brief amicus curiae (coded 1) or not (coded 0);[18] and we considered who appealed (AP) to the Court, the defendant (coded 0) or the state (coded 1).

Finally, we wanted to capture appropriately the political environment confronting the Court. Not only is this a factor that all extralegal models should consider, but, because the Court has incorporated an "evolving standards of decency" component into its capital punishment jurisprudence, it is of particular importance for our analysis. The logic proffered by other scholars, pointing to the obstacles of relying on public opinion data and of the benefits of considering the composition of the other national institutions, led us to adapt an index to tap political pressures (from Cameron, Cover, and Segal 1989). Here, we assume that as the political environment (PE) becomes more Republican, so will the Court's decisions. That environment takes on a value of 0 if both the president and Congress are Democratic; a value of 1 if one branch is Republican, the other Democratic; and a value of 2 when the Senate and the president are Republican.

In sum, we can now specify the extralegal model, as depicted in equation 1, in the following terms:

$$\Pr(C_i = 1) = b_0 + b_1CC_i + b_2DC_i + b_3ST_i + b_4SG_i + b_5AP_i + b_6PE_i + e.$$

We expect b_1, b_2, b_3, b_4, b_5, $b_6 > 0$.

EVALUATING THE MODELS: PARAMETER ESTIMATES

Table 1 depicts the estimates retrieved from a probit analysis of the legal and extralegal models.[19] As the maximum likelihood estimates and summary measures indicate, both perform quite satisfactorily. The $-2 \times$ log-likelihood ratios lead us to reject the implicit null hypothesis (i.e., all coefficients equal 0) for both. Moreover, they do an equally adequate job in reducing error prediction. If we had no knowledge of legal or extralegal factors affecting death penalty case outcomes and we guessed that all were liberal, we would accurately classify 55%. The legal model, which correctly categorizes 75% of the cases, results in a 44% reduction in error,[20] and the extralegal model, which accurately classifies 81%, reduces error by 58%.[21]

Although our concern lies with the models as models, the individual coefficients of each are rather interesting. In general, the legal model's maximum likelihood estimates meet our expectation. The absence of supportive doctrine positively affects the Court's propensity to take a law-and-order stance. More specifically, we hypothesized that the justices centered their inquiry into capital punishment on three dimensions of the decision-making process. We suggested, for example, that they would be less likely to uphold a death sentence against a defendant who did not intend for murder to occur. As we can see, when such a sentence is imposed, it results in a 1.49-standard-deviation addition to the cumulative normal probability function. More concretely, a death sentence imposed on an intentional murder would have a .51 probability of Court affirmation, one given to any other defendant a .12 probability.

We also suggested that several doctrinal aspects of the jury–judge deliberation process would affect Court disposition: the specifics of the crime, aggravating circumstances, and state-initiated psychiatric inquiries. As noted, all three have some impact on the justices. Failure to consider particularized case circumstances is one of the more important determinants, actually altering institutional choice. If defendants raise this issue, the model predicts an outcome favoring them; if not, the Court will probably find for the state. Charges of overly broad or inappropriate aggravating circumstances also increase substantially the probability of the Court's reaching a liberal outcome. The results of a modal categorization indicate that the state will likely lose if this issue is raised (.47) but will win in the absence of such a claim (.82). So, too, the Court has scrutinized quite closely state-initiated psychiatric proceedings. Sentences of death involving such issues would have only a .28 probability of being upheld as compared to .48 without them.

The third issue we expected the Court to consider—death-qualified juries—failed to produce a significant coefficient. Its estimate, however, requires little explication. The variable adds to the model's explanatory power,[22] and its maximum likelihood estimate is positively correlated with a prodefendant outcome, conforming to our hypothetical model. Further, compare the modal probabilities for sentences reached by death-qualified juries (.50) versus those of all others (.82).

The maximum likelihood estimates of the extralegal model are also satisfactory, with our measures of internal and external factors producing significant results. Consider, first, the internal change variables. We see that the addition of Republican appointees enhanced the Court's propensity toward a law-and-

Table 1

Probit Estimates for Legal and Extralegal Models of Decision Making Capital Punishment Cases, 1971–1988 Terms of the U.S. Supreme Court

VARIABLE	LEGAL MODEL COEFFICIENTS	EXTRALEGAL MODEL COEFFICIENTS
Death-Qualified (DQ)	.94* (.61)	—
Crime (CR)	1.49** (.73)	—
Particularized circumstances (PC)	1.46*** (.44)	—
Aggravating factors (AG)	1.01** (.50)	—
State psychiatric examination (SP)	1.38** (.62)	—
Political environment (PE)	—	1.88*** (.60)
Court change (CC)	—	.70** (.30)
Appellant (AP)	—	1.74*** (.57)
Defendant counsel (DC)	—	1.20** (.56)
State (ST)	—	1.29*** (.49)
Solicitor general (SG)	—	2.67*** (1.07)
Constant	−5.35	−5.87
$-2 \times$ log-likelihood ratio	18.82***	36.69***
% categorized correctly	75	81

Note: Standard errors are in parentheses. N = 64. Of these cases, the Court affirmed the imposition of the death penalty in 45%.
*$p \leq .10$.
**$p \leq .05$.
***$p \leq .01$.

Table 2

Evaluating the Predictions of Legal and Extralegal Models Over Time

NATURAL COURT ERA	NUMBER OF CASES PREDICTED CORRECTLY		TOTAL NUMBER OF CASES
	LEGAL MODEL	EXTRALEGAL MODEL	
1 (1971–74)	3 (100)	3 (100)	3
2 (1975–80)	16 (94)	17 (100)	17
3 (1981–86)	19 (63)	22 (73)	30
4 (1987–88)	10 (71)	10 (71)	14
Total	48 (75)	52 (81)	64

Note: Prediction equations are probits using estimates displayed in Table 1. Percentages are in parentheses.

order stance. A most significant shift, however, occurred when O'Connor joined the Court. Prior to her ascension to the bench, a death sentence probably would not have been upheld (a .20 probability). Upon her arrival in 1981, odds increased to .69 that the Court would hold for the state. The external political environment also exhibited a *strong* effect on Court behavior, the most obvious manifestation occurring in the Carter-to-Reagan transition.[23] During the Carter administration, when the government was uniformly Democratic, sentences of death had only a .37 probability of Court approval. During the first six years of the Reagan presidency, the Court almost always upheld such verdicts (.69). Two of the litigant variables also fared quite well. If the solicitor general filed a brief supporting the state, the odds approach .61 of the Court's concurring. In the absence of solicitor general reinforcement, states will generally lose their cases (.44 probability). Likewise, when the defendant appealed, the Court would probably take a liberal position (.65). If the state appealed, that probability would fall to .24. Hence, once again, the solicitor general and appellant variables produced a radical change in institutional decision making from a stance favoring defendants to one predisposed toward state interests.

Though the attorney and state variables did not have quite as dramatic an effect on average, they performed adequately. States facing a less-than-experienced attorney would have about a 50/50 chance of winning their dispute. But if they opposed interest group counsel or a Court-appointed defender, their odds fell, slightly, to 42%. Conversely, attorneys challenging practices in Texas, Georgia, or Florida faced an uphill battle as the modal probability approached .75 that the Court would find in favor of those states; it neared 0 for other governmental litigators.

Both the overall fits and the estimates of the extralegal and legal models suggest that the Court reacted quite consistently to doctrinal cues *as well as* to changes in political stimuli presented in over 15 years of death penalty litigation.

EVALUATING THE MODELS: CATEGORIZATIONS AND PREDICTIONS

Model evaluation can occur on a number of different dimensions. We already have detailed our models' ability to generate reasonable parameter estimates. Indeed, having found both to generate satisfactory coefficients, we could simply conclude that legal and extralegal explanations provide equally viable evaluations of Court decision making. This sort of analysis, however, reveals little about their relative value and their ability to postdict the range of case outcomes. Moreover, significant maximum likelihood estimates may mask individual, nonrandomized flaws. Because time-boundness has constituted such a problem in previous models of Court decision making (e.g., Ulmer 1986),[24] we are particularly interested to know whether the explanatory power of legal and/or extralegal perspectives diminishes over time. We address these concerns by comparing the percentages of correct and incorrect categorizations generated by both models over time. To facilitate interpretation, we grouped these into four periods roughly corresponding to the Natural Court eras included in our analysis.[25]

As we depict in Table 2, we find little longitudinal erosion, generally speaking. Both models start off quite well, postdicting nearly 100% of the 20 cases decided during the first two Natural Courts. Equally satisfying is that both categorize correctly 10 of the 14 cases occurring in Natural Court 4. Perhaps most important, however, is that their ability to generate correct predictions for the latest round of death penalty cases was nearly as good as those for the first. Our models correctly categorized all three cases decided in 1972. During the 1988 term, the Court decided 7 cases involving imposition of the death penalty. If we withhold those cases and reestimate the models, each accurately forecasts the outcomes in 5 of the 7.

Although no erosion occurred in the utility of either model, a more detailed examination reveals the

Table 3

Evaluating the Predictions of Legal and Extralegal Models by Ideology

	% PREDICTED						
	LIBERAL (AGAINST DEATH PENALTY)			CONSERVATIVE (FOR DEATH PENALTY)			TOTAL NUMBER
NATURAL COURT ERA	LEGAL	EXTRALEGAL	ACTUAL % LIBERAL	LEGAL	EXTRALEGAL	ACTUAL % CONSERVATIVE	OF CASES
1 (1971–74)	100	100	100	0	0	0	3
2 (1975–86)	76	82	82	24	18	18	17
3 (1981–86)	66	43	36	33	56	63	30
4 (1987–88)	79	21	50	21	79	50	14

Note: Prediction equations are probits using estimates displayed in Table 1.

presence of two nontrivial problems. First, as we can see in Table 3, which depicts the percentage of liberal and conservative outcomes produced during each natural court (in contrast to the actual outcomes), our models possess ideological biases. The political model becomes increasingly, monotonically, and *incorrectly* prone toward conservatism. For Natural Court 1, it correctly classified all outcomes as liberal; by Natural Court 4, it placed only 21% of cases into that category, when half actually belonged there. Thus, despite initial appearances, by Natural Court 4 the political model is overestimating conservatism, an important finding, to which we will return.

The legal model exhibits precisely the opposite propensity. Beginning in Natural Court 3, the legal model overestimates the *liberal* inclination of the Court. As Table 2 indicates, it yielded its worst predictions in Natural Court 3. As we can see in Table 3, it produced those errors because it overcategorized liberal outcomes. Natural Court 4 presents much the same picture. It classified all seven cases decided in favor of the defendant accurately, but only three of the seven outcomes supporting the state. In short, our doctrinal model predicts more prodefendant outcomes over time than actually occurred, while the extralegal one is moving in the opposite direction. This reaches a point of virtual juxtaposition in Natural Court 4, when the legal model suggested that 79% of the cases would fall into the liberal column, while its extralegal counterpart placed 79% into the conservative one.

This trended behavior continues to plague the models into the Court's 1989 term, which we did not include in our analysis, but can now consider for diagnostic purposes. During that term, the Court decided 11 death penalty cases, holding for the state in 10. If we did not know the outcomes of those cases and used the estimates generated from our legal and extralegal models (see Table 1) to predict them, our results would be discouraging. Both continued along the ideological paths already described. The legal model predicted that the Court would decide 10 of the 11 1989 term cases in favor of the defendant, and the extralegal model classified all 11 as holding for the state.[26]

Not only are the models exhibiting trended ideological behavior, but (a related problem) they also seem to be working at cross purposes. Consider that the legal model incorrectly classified 16 cases, the extralegal one 13. If the cases missed by the models were simply idiopathic, we would expect many of the total 29 to be shared errors; but in fact, they had only 4 missed cases in common. Put in somewhat different terms, of the 16 outcomes incorrectly classified by the legal model, the extralegal one picked up 12. Conversely, the legal model accurately categorized 9 of the 13 errors produced by the extralegal one.

Hence, what emerges (taken in conjunction with previous findings) is that while the overall fits are quite good, the models exhibit disturbing propensities and, interestingly, ones that move in precisely opposing directions. The political model becomes increasingly conservative, the legal model increasingly liberal. Thus, the errors they produce are largely idiosyncratic.

Explaining the Findings

Taken together, then, the legal and extralegal models seem to be working at cross purposes, producing opposing categorizations over time. Theoretically speaking, this is a puzzling phenomenon but one on which we can speculate. We first consider a traditional criticism of the legal model, namely, that it is too static to account for changes in the law. We found, however, that this was not necessarily the case for death penalty litigation. Rather, it does change over time, forecasting more liberal outcomes with each passing Natural Court era than actually happen.

Why this occurs is rather evident. Once the Court enunciates legal policies, attorneys will take a cue from those precedents, bringing better and better arguments within those doctrinal parameters. Quite exemplary are capital cases involving overly broad aggravating circumstances. In *Gregg* v. *Georgia* 1976, the defendant's attorney claimed that one of the state's aggravating circumstances, namely, that the offense was "outrageously or wantonly vile, horrible or inhuman in that it involved torture, depravity, or

an aggravated battery to the victim," was overly broad and vague. The Court rejected this contention on the grounds that future courts need not construe it as such. This holding voided Gregg's particular claim but opened the doors to new challenges based on future interpretation of that circumstance. This is precisely what happened in *Godfrey* v. *Georgia* 1980, when an attorney challenged Georgia's construction of the very same aggravating circumstance. Based on *Gregg*, the justices agreed that the state courts had interpreted it in such a way as to make it overly broad and vague. Our legal model accurately classified the outcome of *Godfrey*; but it missed a 1989 case also involving aggravating circumstances, *Hildwin* v. *Florida*.[27] In this instance, the Court ignored existing precedent and found for the state.

After this small deviation from *Gregg* and *Godfrey* in 1989, the justices moved more rapidly to distinguish existing precedent. For example, in the 1990 case of *Lewis* v. *Jeffers*, they held that the aggravating factor challenged in *Gregg* was not unconstitutionally vague as applied to the defendant even though a lower court, relying in part on *Godfrey*, held that it was. Once again, we used existing estimates (see Table 1) to try to "postdict" *Lewis*. Not surprisingly, this effort failed. The legal model—built around earlier precedent—failed to categorize correctly the outcome of *Lewis* v. *Jeffers*.

This series of cases (and others) demonstrates that the legal model contains an inherent flaw. Because it only considers legally relevant facts, it will continue to forecast liberal outcomes as attorneys capitalize on existing precedent even though "the law" may not actually move in that direction. This is why our legal model accurately forecasted the Court's early "aggravating" cases but missed those decided in 1989 and 1990.

Does this mean that we should discount the legal model in favor of the extralegal one? We think not, because it, too, exhibits idiopathic behavior, prematurely predicting conservative outcomes. It accurately predicted *Hildwin* but missed several other cases in which the Court adhered to precedent and ruled in favor of the defendant.[28] Particularly interesting is the one 1989 term ruling that it miscategorized, *McKoy* v. *North Carolina*. This case involved the constitutionality of a state sentencing scheme permitting juries to consider only those mitigating circumstances on which they unanimously agreed. Because so many pro-state "buttons" of our existing extralegal model were pushed (e.g., the defendant was not represented by a group, the high number of Nixon–Reagan appointees, etc.), it predicted that this case had virtually no chance of being decided in favor of the defendant. Yet, citing clear-cut doctrine suggesting the need for "flexibility" of mitigating circumstances, the Court held for the defendant.

Seen in this light, the extralegal model is too dynamic. It assumes that changes in the political environment will contemporaneously affect court decisions. Yet as the death penalty data suggest, this may be an incorrect assumption. Why does the model possess this flaw? Most importantly, as *McKoy* indicates, it fails to consider the doctrinal boundaries in which the Court works. If Brigham (1978) and others are right, stare decisis does constrain the array of available legal options. Accordingly, abrupt alterations in the political environment may not necessarily translate into concomitant, contemporaneous doctrinal change.[29]

AN INTEGRATED MODEL

Hence, both models possess weaknesses—weaknesses that run diametrically to one another. The legal perspective, which overpredicts liberal outcomes, needs to be tempered by a consideration of political forces. Conversely, the extralegal model should be checked by doctrinal constraints. Given these weaknesses, should we abandon both models as incomplete portrayals of Court decision making? We think not. Rather, they should be considered codependent explanations. The legal model assumes that the Court cannot ignore its role as an adjudicative body, that it is bound by precedent. The extralegal model assumes that the Court heeds political forces and must do so to retain its institutional legitimacy. Over time, those models may diverge, applying diametric pressure of the Court. For capital punishment cases, the legal model exerts a liberalizing influence on the justices as attorneys capitalize on existing doctrine. The extralegal exerts influence toward a more conservative posture as the external and political environments move in that direction. To this extent, legal and extralegal models present interdependent explanations of judicial decision making.

Seen in this light, then, our most prudent approach, analytically speaking, is to combine the two into an integrated model of decision making, one informed by both legal and extralegal factors:

$$\Pr(C_i = 1) = b_0 + \Sigma b_h EL_{ih} + \Sigma b_j L_{ij} + e. \qquad (3)$$

Table 4 depicts the maximum likelihood estimates and summary statistics retrieved from estimating equation (3) in probit. As we can see, the model is quite well behaved. It is significant at the .01 level; all variables add to the model's explanatory power; and the maximum likelihood estimates are in the hypothesized direction. Even more encouraging is its ability to categorize correctly 88% of all case outcomes, with no evident ideological bias.[30]

Based on these results, it would be tempting to conclude that scholars should consider both legal and extralegal variables in their models of Court decision making, that these factors have an equivalently strong impact on outcomes. But is that true? To explore this, we created instruments for each set of factors (legal and extralegal) and then used those to generate beta weights.[31] Table 5 illustrates the results of that analysis. Overall, extralegal factors exert nearly twice the influence (.35 versus .69) on a given Court decision. Particularly interesting, however, is

Table 4

An Integrated Model of U.S. Supreme Court Decision Making

VARIABLE	COEFFICIENT
Death-qualified (DQ)	1.23 (1.14)
Crime (CR)	2.86** (1.21)
Particularized circumstances (PC)	1.13** (.66)
Aggravating factors (AG)	1.05* (.68)
State psychiatric examination (SP)	2.19*** (.86)
Political environment (PE)	2.25*** (.79)
Court change (CC)	.64* (.39)
Appellant (AP)	1.55** (.79)
Defendant counsel (DC)	1.08** (.62)
State (ST)	1.90*** (.64)
Solicitor general (SG)	1.93* (1.44)
Constant	−13.66
−2 × log-likelihood ratio	48.99***
% categorized correctly	88

Note: Standard errors are in parentheses. N = 64. Of these cases, the Court affirmed the imposition of the death penalty in 45%.
*$p \leq .10$.
**$p \leq .05$.
***$p \leq .01$.

the emerging pattern of influence. At first, doctrinal influences had quite a strong impact on the Court; but after Natural Court 1, they decline as extralegal factors set in.

Given our models juxtaposed against evolving doctrine governing the imposition of capital punishment, the data depicted in Table 5 make a great deal of sense. When the Court handed down *Furman* v. *Georgia*, it was writing on a virtually clean slate. Interestingly, however, extralegal factors have no discernible impact on it. The variables included in our legal model drove the decision. To some extent, this is an artifact of how we derived that model—mainly, though not exclusively, from *Furman* (and *Gregg*). But given the relative weight of extralegal factors (nonexistent), the data do tell us something about the nature of judicial decision making. When the justices placed the relatively novel issue of capital punishment on their agenda in 1972, they were apparently more influenced by the extant legal claims than by political and societal forces, which had yet to gel sufficiently. Such a finding seems to support what others have written about those early death penalty cases (e.g., Kurland 1972).

Table 5

The Relative Influence of Legal and Extralegal Variables on U.S. Supreme Court Decisions

NATURAL COURT ERA	BETA WEIGHTS	
	LEGAL MODEL	EXTRALEGAL MODEL
1 (1971–74)	.52	.00
2 (1975–80)	.35	.67
3 (1981–86)	.48	.72
4 (1987–88)	.56	.80

Note: See note 31 for the computation of the beta weights.

What we see in Natural Courts 2, 3, and 4 also fits compatibly with our existing knowledge of capital punishment doctrine and of Court decision making. As the issue evolved and doctrine was refined (or, in some instances, severely distinguished), the relative weight of legal and extralegal factors shifted markedly. The basic underpinnings of the plurality opinions in *Furman* (and, later, *Gregg*) continue to govern the imposition of the death penalty (i.e., the Court has not yet reverted to a pre-1972 posture). That is why the legal model continues to provide a viable framework by which to assess Court outcomes. Yet the more political and politicized factors exert the greatest influence today on case outcomes.

Again, this is not surprising, given previous research on Court decisions in capital cases in particular (e.g., Zimring and Hawkins 1986). But, it does reveal something about the nature of judicial decision making. In short, as law governing capital punishment evolved, the Court moved away from the original doctrine per se and looked instead to the extralegal environment.

CONCLUSION

We began with a relatively simple purpose—to determine how well legal and extralegal models performed in head-to-head competition. We thought this a significant enterprise because theories of judicial behavior are rarely subjected to this sort of examination. As it turned out, both models performed satisfactorily, evincing strong abilities to account for Court outcomes in capital punishment litigation.

Our analysis could have ended here, concluding that legal and extralegal models provide equally good frameworks by which to assess Court decision making. Such a conclusion, however, would have been premature, because, upon further evaluation, we found that both exhibited disturbing propensities. The legal one prematurely anticipated liberal outcomes, the extralegal model conservative ones. Given these results, we tested another proposition, namely, that extralegal and legal frameworks presented code-

pendent, not mutually exclusive, explanations of decision making. Based on these results, we offered an integrated model of judicial decision making contemplating a range of legal and extralegal factors.

Taken as a whole, what does our model reveal about the nature of Supreme Court decision making? Can we now address our original research question, How does the Supreme Court reach decisions? On some level, its answer is obvious: the most complete explanation of judicial outcomes should incorporate legal and extralegal factors. Seen in this light, the views of neither the classical legal thinkers nor the behavioralists are incorrect; but they are incomplete. Both law and politics, as we would intuitively expect, play significant roles in the Supreme Court's decision-making process.

On another level, however, these roles may not necessarily be equivalent. Our analysis of capital cases reveals that legal factors have the greatest impact at the early stages of an issue's life; as it evolves, however, extralegal variables dominate. In a way, therefore, our results reinforce the legal paradigm set out by the pragmatic Benjamin Cardozo (1921). When the law ceases to be consistent with the Court's "sense of justice," there occurs a greater willingness to abandon it. This, he wrote, *should* be the nature of the judicial process. Our analysis confirms that (at least for death penalty litigation) it *is*.

Where do we go from here? Surely our next research tasks must be to eliminate some of the shortcomings of this effort. In particular, our focus on a single area of the law hampers our ability to reach generalizable conclusions. Hence, future analyses ought to continue where we have left off by exploring other legal issues with an eye toward discerning whether the patterns we found are idiosyncratic or sufficiently universal.

Notes

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1. On the other hand, we could argue that this is more the "norm" than it is an anomaly; that is, research on the judicial process rarely compares models of decision making. Two of the few exceptions are Gibson 1978 and Johnson 1987. The latter, especially, is relevant to our study. Johnson sought to determine whether a legal or political model accounted for "more variation in the use of Supreme Court decisions by lower federal courts" (1987, 325). In the end, his analysis lends support to "those who argue that legal factors in part account for judicial behavior, especially in lower courts" (339).

2. For a vivid illustration, cf. Cushman's and Segal's treatments of search-and-seizure cases (Cushman 1929, 82–84; Segal 1984).

3. Kort corrected some of these problems in later works (e.g., Kort 1973).

4. Many textbooks and readers on the judicial process distinguish *fact pattern* analysis from legal models. As Carp and Stidham note, "While fact pattern scholars also believe that the facts of a case determine its outcome, they quickly part company with the traditional approach after that point" (1985, 193). Among the most important factors setting it apart from a legal model, according to them, is the use of legally irrelevant facts. In this light, Segal, who does not consider such "irrelevant facts," comes far closer to validating doctrinal perspectives than behavioral ones.

5. These studies focus on the U.S. Supreme Court. For an excellent example of the application of the legal model to lower-court decision making, see Johnson 1987.

6. Here we refer to probit—an analytic technique well suited to binary choice variables, such as judicial outcomes—because it operates on a continuous *S*-shaped probability function.

7. *Gregg* v. *Georgia* 1976.

8. *Coker* v. *Georgia* 1977.

9. *Enmund* v. *Florida* 1982.

10. *Gregg* v. *Georgia* 1976.

11. *Witherspoon* v. *Illinois* 1968; *Adams* v. *Texas* 1980.

12. See, generally, *Furman* v. *Georgia* 1972; *H. Roberts* v. *Louisiana* 1977; *Lockett* v. *Ohio* 1978.

13. *Woodson* v. *North Carolina* 1976.

14. *Furman* v. *Georgia* 1972; *Gregg* v. *Georgia* 1976.

15. *Godfrey* v. *Georgia* 1980.

16. *Estelle* v. *Smith* 1980.

17. We adopt this measure from Segal's analysis (1984, 895). But we are not unmindful of its flaws, the most important of which is that it assumes identical changes from one pair of justices to the next. For example, Segal treated the change from Warren to Burger as the equivalent of the change from Douglas to Stevens. Based on Baum's scores and rankings of the justices (1989b, 517), this is somewhat troublesome (i.e., Stevens and Douglas generally supported civil liberties claims, as did Warren; Burger generally did not). We face a similar problem; if anything, our predicament is somewhat more exaggerated than Segal's because, for instance, we are treating the change from a Roosevelt–Ford (Stevens for Douglas) pairing as the equivalent of a Nixon–Reagan (Kennedy for Powell) one.

We offer two reasons for so doing. The first, and more important, one speaks in general terms to this sort of research. Like Segal (1984), we wanted to assess the cumulative impact of ideology on institutional-level decision making and do so in a non–post hoc way. This approach allows us to reach that end in a far more satisfactory manner than would, say, indicators developed to tap the specific propensity of individuals. To demonstrate this, let us compare the bottom lines of Segal's (1984) approach to macro decision making (which we adopt here) with that which he and Cover fashioned in 1989 to identify a priori the ideology of individual justices. If we took the median ideology of the justices from Segal and Cover's (1989) scores, we would expect to observe *no* change in the Court's ideological propensity from 1975 to 1986 and a *decrease* in its overall conservatism from 1987 to 1988. In contrast, our adaptation of Segal's (1984) measure depicts a Court that is, generally speaking, growing more conservative over time. No question exists as to which, on its face, is the more valid measure.

We also, of course, are mindful of the fact that we are dealing with a particular area of the law. Accordingly, our second reason is more substantive and contextual. As we know, the death penalty did not become a major political issue and thus a consideration in the Supreme Court nomination process until *Furman* v. *Georgia* 1972, after Eisenhower and Nixon made their appointments. This is particularly critical for our treatment of the Reagan-to-Nixon appointments. While Nixon ensured that his nominees were law-and-order advocates, he had no reason to identify their position on death penalty. In contrast, later Republican presidents (particularly Reagan) included a pro–death penalty stance as a requirement for nomination (Schwartz 1988). Accordingly, it is possible that some of the Nixon appointees were less predictable on capital punishment than on general issues of criminal law. (In fact, Epstein and Kobylka 1992 empirically

demonstrates it). In short, at least in the death penalty area, we have every reason to suspect that a "conservative" justice was potentially replaced by an even more "conservative" one.

18. The office of the solicitor general never participated as a party, nor did it file any briefs in support of the defendants' position.

19. In the interest of space, we generally limit our discussion to the results uncovered from the probit analyses. For readers seeking more information, we would be most willing to provide copies of the data set. Contact Epstein by post or bitnet (C38054LE @ WUVMD).

20. Brenner, Hagle, and Spaeth (1990) offer the formulation

$$\text{ROE} = 100 \times \frac{\%\text{ correctly classified} - \%\text{ in the modal category}}{100\% - \%\text{ in the modal category}}.$$

21. As is the case for all analytic techniques (e.g., ordinary least squares regression), researchers must concern themselves with interpretive problems that can result from violating the assumptions of the extant underlying statistical theory. In particular, we must at least consider the problem of serial correlation. As Aldrich and Nelson note, "When residuals are serially correlated, maximum likelihood estimates remain unbiased in large samples, but they are not efficient. The result is precisely analogous to that of autocorrelated linear models" (1984, 81). Yet we will not statistically address the issue of serial correlation for the same reasons offered by Marra, Ostrom, and Simon (1989, 605–7). Most important is that "there is no agreed-on test for statistical significance in the nonlinear context" (ibid.). Aldrich and Nelson concur, "Unfortunately, corrections for serial correlation have proven untractable in the logit and probit cases" (1984, 81). Adding to our decision was the fact that like Marra, Ostrom, and Simon (1989, 605), our data lack a constant time interval between observations.

22. To determine this, we removed DQ from the model, reestimated it, and compared the log-likelihood ratio it produced with that listed in Table 1. Overall, the removal of DQ resulted in a significant (at .05) reduction in the model's explanatory power.

23. We stress this point because of the possibility that conceptual overlap between the court change and political environment variables bled into our analysis. After all, some scholars argue that it is through the appointment process that Congress and the president primarily affect the Court. Several diagnostic checks, though, confirm our finding that the variables have independent and significant effects. We first examined whether they were significantly correlated; they were not. We then checked for an interaction effect. We added to the extralegal model an interaction term (PE × CC). While both CC and PE produced significant estimates, the interaction term did not.

24. More specifically, Ulmer addressed the question of whether social background models are time-bound, concluding that, indeed, they "work only for particular sets of judges at particular points in time" (1986, 965). A more recent study by Tate and Handberg (1991) argues that there may be no way to avoid this problem completely. However, they suggest that if researchers develop more theoretically sensitive personal attribute models, the problem of time-boundness can be minimized.

25. They are Natural Court 1 (1971–74), Natural Court 2 (1975–80), Natural Court 3 (1981–86), and Natural Court 4 (1987–88). Natural Courts are periods during which the Court's membership remains stable. We should note, though, that Natural Court 3 is not a pure Natural Court since a membership change did occur during the 1986 term: Scalia replaced Rehnquist, who ascended to the chief justice position left open by Burger's retirement. For purposes of presentation, we grouped 1986 with Natural Court 3.

26. To be sure, then, the categorization accuracy of the extralegal model remained high (91%), but had we merely guessed that all 1989 term cases would result in pro-state outcomes, we would have been equally accurate. Moreover, if these categorizations result from trended behavior (as we suspect), we cannot necessarily conclude that the model is explaining Court outcomes.

27. In *Hildwin*, the Court held that the Sixth Amendment does not require that a jury specify the aggravating circumstances authorizing the imposition of death—that they could be specified by a judge. One of the statutorily allowed aggravating circumstances was that the killing was "especially heinous, atrocious and cruel." The Court concluded that the validity of this circumstance did not need to be addressed, since the imposition was based on other factors provided by statute.

28. In *South Carolina* v. *Gathers* 1989, the Court considered the remarks of a prosecutor at the sentencing stage, remarks that the state supreme court held "conveyed the suggestion appellant deserved a death sentence because the victim was a religious man and a registered voter." The Supreme Court agreed, ruling that "our capital cases have consistently recognized that 'for purposes of imposing the death penalty . . . [the defendant's] punishment must be tailored to his personal responsibility and moral guilt,' citing *Enmund* v. *Florida* 1982 (ibid., 2210).

29. Another explanation centers on how we measured the Court's internal ideological disposition. It is possible that the *Court change* variable, which increases over time, may be responsible for the extralegal model's categorizations. To explore this possibility, we reestimated the model using the means of Segal and Cover's (1989) values instead of our CC measure. As we have pointed out (n. 17), their scores are less than ideal for macro analysis. Therefore, we did not adopt them; and the reestimation reflects this deficiency. More specifically, the results, through Natural Court 3, parallel those we obtained. The model does a satisfactory job in categorizing cases but by Natural Court 3, overestimates conservative outcomes. For Natural Court 4, however, it correctly categorizes only 50% of the cases, a percentage below either of our models for any Court era. Such is hardly surprising, given that Segal and Cover's mean values anticipate that Natural Court 4 would be less conservative than that which preceded it. As a result, we cannot reach any firm conclusions; we can only offer the explanation in text as a strong possibility.

30. Of the eight missed cases, five were liberal outcomes, and three were conservative ones.

31. To create the instruments, we followed the approach outlined by Ostrom and Job, reestimating the equations and then using the standard deviation of the underlying scale and of each instrument to compute the beta weights (1986, 565).

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THE POLITICAL CONSEQUENCES OF INTOLERANCE: CULTURAL CONFORMITY AND POLITICAL FREEDOM

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I demonstrate that the intolerance of ordinary citizens matters for real politics even if strong linkages to policy outputs do not exist. In particular, the model I test posits that cultural intolerance constrains the liberty of individual citizens. Focusing on how people perceive political freedom, several hypotheses coupling tolerance and freedom are explored. Data from a national survey show that tolerance and freedom are connected. Those who do not feel free to express themselves politically are more likely to be intolerant of others, to have less heterogeneous peer groups and less tolerant spouses, and to live in less tolerant communities. Ultimately, the importance of mass political intolerance in the United States is that it establishes a culture of conformity that seems to constrain individual political liberty in many important ways.

When scholarly inquiry into political intolerance began nearly 40 years ago, there was little doubt about the political relevance of mass commitments to democratic values. Having just survived a world war against authoritarian dictators who seemed to have been abetted by their citizenries, there was widespread concern about whether democracy could exist in the face of the antidemocratic tendencies of the people. Just as the authoritarian movements of Europe and the Far East seemed to find succor in the hearts and minds of the people, the antidemocratic movement of McCarthyism threatened liberty in the United States. Few would have doubted whether citizen beliefs about majority rule and minorities rights mattered for politics.

Though 40 years of research has consistently shown a deep strain of intolerance in U.S. mass political culture,[1] there is now a growing concern about whether these research findings have any real political implications. It is clear, for instance, that mass political tolerance is not a prerequisite to democratic government and that relatively democratic regimes can exist even when citizens hold fairly antidemocratic beliefs. Indeed, some have even suggested that the intolerance that we have so long measured and attempted to explain (both in terms of its consequences and etiology) has no meaningful existence in the first place (Mueller 1988). Absent any clear understanding of whether mass political intolerance does or does not matter, there is a growing uncertainty about the utility of further inquiry into the democratic beliefs of ordinary people.

Consequently, it is important for those who study mass political intolerance to move beyond simple descriptions of political beliefs and to demonstrate the political relevance of the antidemocratic values of citizens. I offer a theoretical means of linking these attitudes with politics. The linkage hypothesized is not through the traditional opinion–public policy process but is, instead, directed at the mass political culture. I focus on *perceived political freedom*—how people view the availability of liberty. My basic hypothesis is that intolerance matters for politics by constraining the freedom available to ordinary citizens. On the basis of this theoretical framework, survey data on the political relevance of political intolerance are presented, and several hypotheses are tested.

In particular, I consider this problem of cultural intolerance using three different analytical strategies: (1) I link intolerance and freedom within the minds of individual survey respondents; (2) I examine how intolerance within the peer group (especially the family) constrains political freedom; and (3) I posit that lack of freedom is a function of local cultural values and community sentiments. Throughout much of this investigation, I employ a contextual form of analysis.

Each of the analytical strategies I pursue is assuredly incomplete. None of the analysis is irrefutable. My objective is to explore some of the ways in which cultural intolerance impinges upon the beliefs and behaviors of ordinary citizens. At the risk of some disjointedness, I am therefore presenting three different (but related) processes through which intolerance constrains freedom. The strength of the argument presented here is most fairly judged by the cumulative picture that emerges, not by the results of the tests of individual hypotheses. In light of the paucity of research on the *implications of intolerance*, this incomplete, but broad, research design is preferable to a comprehensive but narrow one (which is at the moment impossible anyway). In keeping with my desire to move beyond a simple micro analysis of intolerance, I close with some observations about the implications of these findings for a broader theory of the role of public opinion in democratic politics and the allocation of political freedom.

CONVENTIONAL UNDERSTANDINGS OF THE RELEVANCE OF MASS POLITICAL INTOLERANCE

The initial impetus for the empirical study of the democratic beliefs of citizens was largely provided by

democratic theorists (e.g., Griffith, Plamenatz, and Pennock 1956). The mass-based totalitarian political movements of the period of World War II and its aftermath led scholars to examine more closely the nexus between democratic political institutions and the beliefs of citizens (e.g., Almond and Verba 1963; Lipset 1960). The debate that ensued was structured around the issue of whether citizen commitments to democracy were a prerequisite to democracy. The assumption that the survival of democracy as a process requires citizens to hold certain values received quite a jolt from the empirical evidence from surveys of the beliefs of ordinary people (e.g., McClosky 1964; Prothro and Grigg 1960). Citizens of the United States in the 1950s and 1960s (assuming this to be a democracy) were found to be quite intolerant of political nonconformists.

Out of a variety of empirical survey evidence was born the *elitist* theory of democracy. This theory, which has many variants,[2] accepts as an empirical fact the intolerance of the mass public but argues that under most circumstances this intolerance is neutralized. Except under extraordinary political conditions, the antidemocratic mass public is immobilized by its own ignorance and apathy, leaving the relatively more democratic elite free to rule in a democratic fashion. This theory argues that mass opinion is important, since in times of political crisis the masses can be mobilized by antidemocratic elites, but that during times of ordinary politics these beliefs have few direct political consequences.

Studies that attempt to test hypotheses linking public opinion and public policy on matters of political tolerance and democratic rights have been rare, however.[3] Focusing on changes in public opinion and their impact on public policy, Page and Shapiro (1983) discovered that in eight of nine policy changes in the area of civil liberties, there was congruence between public policy and public opinion. What the public generally wanted, it seems to have got. On the other hand, I investigated the hypothesis that mass political intolerance was connected to the adoption of repressive public policy by the states during the McCarthy era (Gibson 1988) but found no such relationship. Nor is there evidence that intolerant mass opinion directly spurred the state legislatures to adopt repressive statutes against dissent on university campuses during the Vietnam War era (Gibson 1989a; see also Gibson and Tedin 1988). This research, though limited in many important ways, cast some doubt on the elitist theory's presumption that mass intolerance causes outbreaks of political repression during times of political crisis. Nonetheless, it is important for my concerns here because it *fails to identify any direct political consequences flowing from mass opinion.*[4]

Absent direct linkages to public policy, is mass political opinion irrelevant? Perhaps not. Public policy is not the only dependent variable worthy of consideration. Even when opinion does not directly set policy, it may nonetheless constrict the choices of policymakers by setting broad limits on acceptable policies. And even when government is unresponsive to citizen preferences, citizens may well constrain the actions of their fellow citizens. Indeed, the political life of a community may be strongly influenced by the beliefs and values of its citizens even in the absence of direct citizen influence on public policy. Thus, it is worthwhile to consider other mechanisms through which mass political intolerance matters for politics.

THE CONTEXTUAL–CULTURAL RELEVANCE OF POLITICAL INTOLERANCE

Perhaps the true significance of mass political intolerance lies in its contribution to the creation of a culture of political conformity, a culture in which political liberty is limited by the intolerance of ordinary citizens. In this sense, mass beliefs about the political rights and liberties of those with unpopular political views circumscribe how citizens interact with one another (see Chilton 1988; MacKuen 1990). The political relevance of political intolerance, then, can be found in the constraints on political thought and action that citizens impose *upon each other*. Thus, I hypothesize that people learn from the political culture that intolerance is widespread, that it is acceptable, and that there are tangible risks to asserting views that the intolerant culture finds objectionable. Political intolerance can thus define the context of politics for many citizens.

This suggests that the relevance of intolerance must be understood through a contextual analysis of the political culture. Those who study public opinion and electoral behavior have long been concerned with contextual effects[5] on opinion formation and political action, and there seems to be fairly widespread agreement that individuals form their opinions not in isolation but in interaction with their social environments. For instance, according to McPhee's (1963) theory of social reality testing, individuals consume information from their environments, develop personal understandings of the information, and then test hypotheses through interpersonal interaction (see also Huckfeldt and Sprague 1988, 1989, 1991). Through this reality-testing process, pressures toward conformity occur. It is rare that individuals reject the reactions of their peers. Though some scholars (e.g., Noelle-Neumann 1984) view this as a coercive process in which social isolation is imposed on recalcitrant nonconformists, individual citizens may conform through what they perceive as more or less voluntary processes.

Many have thought about contextual effects of this sort in terms of short-term attitude change and change in announced voting intentions and behavior.[6] While one can imagine that there are such effects on these attitudes and actions, politics is rarely salient enough for people to engage in continuous reality testing. The stronger effects are likely to be in terms

TABLE 1

Levels of Tolerance, United States, 1987 (%)

ACTIVITY PROPOSED	PERCENT BELIEVING GROUP SHOULD BE ALLOWED TO ENGAGE IN ACTIVITY			
	MOST DISLIKED GROUP	2D MOST DISLIKED GROUP	3D MOST DISLIKED GROUP	4TH MOST DISLIKED GROUP
Run for office	26.6	31.2	34.1	35.7
Legally exist as a group	31.5	36.7	37.6	42.1
Make a speech	49.5	38.2	48.3	50.3
Hold public rallies	32.6	34.5	36.3	39.6
Teach in schools	18.3	21.1	—	—
Make phone calls untapped	63.4	61.1	—	—
All activities	6.3	8.0	18.3	13.3

Note: Approximate weighted Ns: most and 2d most disliked, 1,205; 3d most disliked, 1,065; 4th most disliked, 655. The last two items in the table were not asked of the 3d and 4th most disliked groups.

of more basic political orientations and change over longer periods of time. In no area of political life are these social interactions more important than in the area of conformity and political tolerance.[7] In arriving at political beliefs and behaviors, citizens continually test hypotheses about what is and what is not acceptable within the cultural context. The range of acceptable beliefs varies in conjunction with variation in political culture, in conjunction with variation in levels of political tolerance that individuals experience.[8]

It is useful to distinguish between the macro and micro environments of the individual. The macro environment is the larger social and political community within which the respondent resides, while the micro environment is made up of the specific friendship and family groups relevant for the respondent. My guiding hypothesis is actually quite simple: I expect that more political freedom is perceived where these environments are more tolerant and that the environments are the most tolerant when they are the most diverse politically.

Thus, the challenge for research on political intolerance is to demonstrate the *cultural* relevance of these antidemocratic beliefs. Is there a culture of conformity in the contemporary United States? Is Tocqueville's observation, "In America the majority raises formidable barriers around the liberty of opinion" (Tocqueville 1948, 264), a valid assessment of contemporary levels of perceived freedom? Do individuals hold views that they perceive to be unpopular with their fellow citizens? Are they reluctant to express those views? To what degree is there self-censorship in the United States as a result of this perceived political intolerance? I will attempt to address these questions by testing the general hypothesis that mass political intolerance creates a culture of conformity and that this culture significantly affects citizens' perceptions of the freedom that is available to them.

INTOLERANCE AND PERCEIVED POLITICAL FREEDOM IN THE CONTEMPORARY UNITED STATES

Before considering the specific hypotheses, I turn for a moment to the conceptualization and operationalization of the key tolerance and freedom concepts. *Political intolerance* is a concept that has attracted considerable attention from public opinion scholars. *Perceived political freedom* has not. Thus, after a rather cursory review of the measurement of *intolerance*, I will consider in some depth how *freedom* is conceptualized and operationalized.

Levels of Political Intolerance

To what degree are Americans today politically intolerant? Conceptualizing tolerance as "opposition to state actions that limit opportunities for citizens, individually or in groups, to compete for political power" (Gibson and Bingham 1985, 106), we can turn to data from a 1987 survey of the mass public to address this problem (see Appendix).

Table 1 reports willingness to tolerate deviant and unpopular political groups. The data are based on survey items that require the respondents to select the specific political groups that are to be tolerated or not tolerated.[9] The table reports reactions to the four groups in American politics that the respondent most dislikes.[10] Several conclusions are warranted from the data. First, intolerance is quite widespread. This is demonstrated both by the responses to individual items and the overall unwillingness to allow these unpopular political minorities full rights of democratic citizenship. Indeed, it is remarkable that only 6% of the American people would allow the group they most dislike to enjoy the same political rights and opportunities that the rest of the polity enjoys. It

is difficult to imagine how intolerance might be more widespread.

Nor is intolerance limited to a single extreme group. Rather, it is generalized across groups that are greatly (even if not extremely) disliked. Though there is some variation in intolerance in Table 1 that is related to the degree of negative affect toward the group, the relationship is neither monotonic nor strong. People probably tolerate groups they only a little dislike (e.g., Democrats probably tolerate Republicans); but it appears that even moderately intense antipathy often translates into intolerance.

Thus, there is little support for full civil liberties for unpopular political minorities. But these data still do not address the question of whether mass political intolerance matters. Does intolerance contribute to restrictions on freedom, self-censorship, and political conformity?

Levels of Perceived Political Freedom

How strongly does the political culture of the United States encourage and reward conformity and discourage and penalize political nonconformity? Perhaps the most appropriate strategy for considering perceptions of political intolerance would be to survey groups that are the contemporary targets of intolerance, such as atheists, Klansmen, communists, fascists, and homosexuals. Such a research design would be difficult to implement (but see Gibson 1987a on the views of homosexuals). An alternative design would be one in which respondents are asked to report on their own perceived political freedom.

According to Preston (1982), people cannot be free unless they perceive opportunities for self-expression, unless they have the capacities and conditions for deliberative choice.[11] To the extent that some citizens perceive constraints on their freedom, liberty is diminished. This view of freedom is useful because it implies that a key element of freedom is the *perception of the ability to make such choices freely*.[12]

This conceptualization of freedom is also salutary because it renders the concept susceptible to empirical investigation through an assessment of the perceptions of people of the conditions for deliberative choice. Most basically, do people perceive such conditions as available to them? Certainly, those who expect to endure sanctions for political activity have neither the capacity nor the conditions for free action. Though freedom is more than merely perceptual, the beliefs of individuals are important for understanding their own levels of real freedom.

Just what sorts of activities are of concern when we speak of "freedom to act"? Though many domains of activity may be of interest, my concern here is limited to *political* freedom, mainly within the context of liberal democratic polities. Relying on Dahl's (1971, 1989) theory of polyarchy, I postulate that in a liberal democracy, citizens must have the opportunity to communicate with their fellows and to join together in political parties and activist groups to press their political demands. Thus, from the perspective of democratic political systems, political freedom includes institutional guarantees of the right to engage in oppositionist political activity by speaking, assembling, organizing, proselytizing, and competing for political power.[13] From the perspective of the freedom of individual citizens, my concern is whether individuals perceive opportunity to express themselves about political matters.[14]

Within this conceptualization of perceived freedom, I recognize two primary sources of constraints on liberty: the larger political system and the immediate interpersonal context.[15] Citizens may perceive that public expressions of their political views are circumscribed by the laws and actions of the state. This external censorship is more consistent with traditional notions of political repression. In addition, however, both public and private expressions may be constrained by interpersonal pressures toward conformity and sanctions for deviance. Local networks of friends, family, and associates may impose substantial restrictions on their freedom. To the extent that the political culture to which the individual is exposed does not reinforce political diversity and respect nonconformity, individuals with unpopular views may perceive significant repercussions for expressing their opinions. Thus, in understanding personal political freedom, we must be sensitive to constraints emanating from the larger political system, as well as from the personal network of the individual.[16]

Perhaps the easiest to measure of these dimensions is perception of what political activity the government will permit. The top of Table 2 reports responses to the question whether the government would allow the respondents to engage in certain types of political activities, prefaced by the premise "Suppose you felt very strongly that something the government was doing was very wrong and you wanted to do something about it." The responses were collected through a four-point response set, though Table 2 reports percentages based on collapsing the categories *probably not allow* and *definitely not allow*.[17] Perceptions of freedom are closely related to the race of the respondent (Gibson n.d.[b]). Because of this and because the survey included a remarkably large and representative oversample of African-Americans (see Appendix), the data are reported separately for blacks and whites.

Table 2 reveals that significant numbers of Americans perceive that the government would *not* allow them to express their opposition to government policy through conventional and unconventional political activity. Racial differences on these items are dramatic. Blacks are *much* more likely to perceive constraints on their freedom than are whites. Perhaps most surprising is the substantial numbers of both blacks and whites who feel that the government would not allow them ordinary and quite conventional means of political participation. One-quarter to two-fifths of white Americans believe that the government would prohibit them from expressing their

TABLE 2

Perceived Political Freedom, Black and White Mass Publics

PERCEPTION OF FREEDOM	BLACKS	WHITES	FACTOR LOADING[a]
Believe government would *not* allow them to[b]			
Organize nationwide strike	85.9	79.0**	.49
Organize public meetings	63.7	39.5**	.88
Organize protest marches/demonstrations	54.5	35.3**	.85
Make speech criticizing government actions	55.6	29.6**	.78
Publish pamphlets	53.0	28.1**	.86
Are reluctant to talk about politics because they[c]			
Don't like arguments	58.3	46.5**	.58
Don't want to create enemies	42.3	37.3	.61
Worry that people would think their views strange	28.7	18.7**	.59
Worry what people would think of them	18.2	12.5**	.62
Worry the government might find out	12.5	2.4**	.44
Unwilling to[d]			
Put sign in front of home/apartment	71.0	77.2*	.66
Put bumper sticker on car	59.5	68.8**	.75
Participate in demonstration	55.7	61.8*	.65
Wear button to work or in public	51.4	55.8	.83
Sign petition for publication in local paper	51.4	39.1**	.77
Write letter to elected representative	37.1	30.3**	.64

[a]The factor loadings are based on a combined analysis of blacks and whites.
[b]The question leader read: "Suppose you felt very strongly that something the government was doing was very wrong and you wanted to do something about it. Do you think the government would definitely allow, probably allow, probably not allow, or definitely not allow you to. . . ." The percentages shown collapse the *probably not* and *definitely not* responses. The approximate number of black respondents is 434; for whites, N = 796.
[c]The leader of each of the items is "I am sometimes reluctant to talk about politics because. . . ." Response choices were *true* and *false*. The number of black respondents exceeds 420, and the number of whites is at least 785.
[d]The question read: "Let's say you did have a political view that you knew would be very unpopular with others. Would you be willing to . . . ." The percentages reported exclude those who would not do the activity under any circumstances, regardless of the popularity or unpopularity of the view. The number of black respondents exceeds 404 and the number of whites is at least 749.
*$p \leq .05$ (racial difference of uncollapsed responses).
**$p \leq .01$ (racial difference of uncollapsed responses).

opposition through conventional speech and assembly activities. Among blacks, 53–64% believe the same. And although it is perhaps a bit fanciful for ordinary people to imagine organizing a nationwide strike, four in five black and white Americans believe that the government would prohibit such efforts. The level of perceived *governmental* constraints on political opposition is astoundingly high.

Perceptions of governmental limitations on political freedom represent an external form of censorship. Internal censorship, or self-censorship, can be measured by asking whether individuals are willing to express themselves politically when their views are unpopular with the majority. Consequently, I attempted to measure the beliefs Americans hold that might justify self-censorship. The respondents were given the opportunity to accept or reject a variety of statements that might explain reluctance to talk about politics with their families and friends. Each of these statements represents the belief that there are significant personal costs to be paid for expressing one's views. The middle of Table 2 reports their responses to these statements. Many of these responses suggest strong reservations about political expression. Politics is perceived as sometimes leading to arguments (58% of blacks, 47% of whites) and as creating enemies (42% of blacks, 37% of whites). There is also a substantial minority of respondents that worries about nonconformity. Generally, many believe that political discussions can be associated with unwelcomed repercussions from friends and family.

The racial differences observed in perceptions of the governmental constraints on political action are not as stark here; but neither are they trivial. Blacks are significantly more likely to fear expressing their views because people would think them strange, are more fearful of repercussions from the government, more censor themselves due to a distaste for political argument, and are more fearful about their reputations. Special note should be taken of the nearly 13% of the black sample that is reluctant to talk about politics due to a worry "that the government might find out about me." This is an astonishing level of concern among African–Americans and represents a very serious perceived threat to personal political freedom.

I also asked the respondents whether they would be willing to engage in certain sorts of activities to express views in spite of their being unpopular with others.[18] These items represent a form of behavioral self-censorship. The bottom of Table 2 reports the results from these questions. A substantial number of

Americans are reluctant to express unpopular political views. The figures range from roughly three-fourths who are unwilling to place a sign in front of their home to announce an unpopular view to one-third who would not write a letter to one of their elected representatives. It is not particularly surprising that large majorities would be unwilling to announce an unpopular view with a bumper sticker or a yard sign, since to do so makes one's property vulnerable to those who find the position offensive. But a majority of Americans are also unwilling to express an unpopular opinion through less risky activities such as wearing a button or participating in a demonstration. Indeed, nearly one-third of the American people would be unwilling to write a letter to an elected representative to express an unpopular political position! Only one-sixth of the respondents would engage in all six of the activities; fully 28% claim that they would do *none* of the activities. In general, the greater the direct personal accountability for the view, the less likely the respondents are to want to engage in the activity. Once more, we see that a substantial portion of the American people is fearful of public expression of unpopular political views.

Racial differences in behavioral self-censorship are less pronounced than on the other sets of items. Blacks are significantly less likely than whites to be willing to sign a petition that would be published in the newspapers; but for some activities, *whites* are substantially less likely to be willing to express themselves. For instance, nearly 10% more whites claim they would not put a bumper sticker on their cars (a difference that is significant). The differences in these frequencies suggest that the sources of self-censorship may vary according to race.

In order to examine the causes and consequences of perceived political freedom, it is useful to construct some indices of political freedom. I employ factor analysis to construct the measures, since in factor analysis the items are weighted unequally in the construction of the summary indices. Since all of the factor analysis results support the assumption of the unidimensionality of the measures, scores from the first unrotated factors serve as the specific indices. The factor loadings for each of the three factor analyses are also shown in Table 2.[19]

According to the results I have presented, political freedom is not widely perceived to pertain in the United States. Levels of perceived governmental constraints on freedom and self-censorship are surprisingly high. Moreover, perceived freedom seems to be allocated unevenly across racial groups. African-Americans perceive themselves as having less liberty, just as they have less of nearly everything else of value in American society. It remains to consider whether variation *beyond that associated with race* can be accounted for. I begin this inquiry with consideration of the connections between freedom and tolerance within the minds of the individual respondents.

CONNECTING FREEDOM AND TOLERANCE AT THE INDIVIDUAL LEVEL

To what degree do tolerance and perceptions of political freedom go together? Do those who are intolerant perceive more limited freedom for themselves? Figure 1 shows the bivariate correlations among the various indicators of perceptions of freedom and the index of political tolerance for both blacks and whites. Generally, the correlations do not differ substantially across race (although there are a few exceptions); and the magnitude of the black coefficients is generally somewhat smaller than the white coefficients. Beliefs about tolerance and freedom are fairly closely connected. There is a moderately strong relationship between political tolerance and perceptions of repression, with those who are more tolerant perceiving *less* political repression. These people apparently believe not only that they are free to act as they wish but that others should be free as well. Conversely, those who would deny liberty to their political opponents do not necessarily claim it for themselves. Those who are more tolerant are also less likely to engage in self-censorship. Whether tolerance flows from some sort of norm of reciprocity and fairness ("Because I am able to express my views, others should be allowed to express theirs") or individuals are projecting their own tolerance onto others cannot be determined. Tolerance of others is associated with the belief that there are few significant costs to be paid for one's own political self-expression.

It would be tempting to argue that perceptions of government constraints inhibit self-expression; and, indeed, there are significant correlations between these variables for both blacks and whites. Yet I am a bit reluctant to interpret the causal pathway. Those who self-censor may rationalize their behavior through beliefs about repression rather than limiting themselves out of fear of governmental retaliation. At the moment, it is prudent to conclude no more than that most Americans—black and white—hold reasonably consistent views about freedom and tolerance.[20] Tolerance matters because it is connected to a set of beliefs about the legitimacy and appropriateness of self-expression.

CONTEXTUAL ANALYSIS OF CONSTRAINTS ON FREEDOM

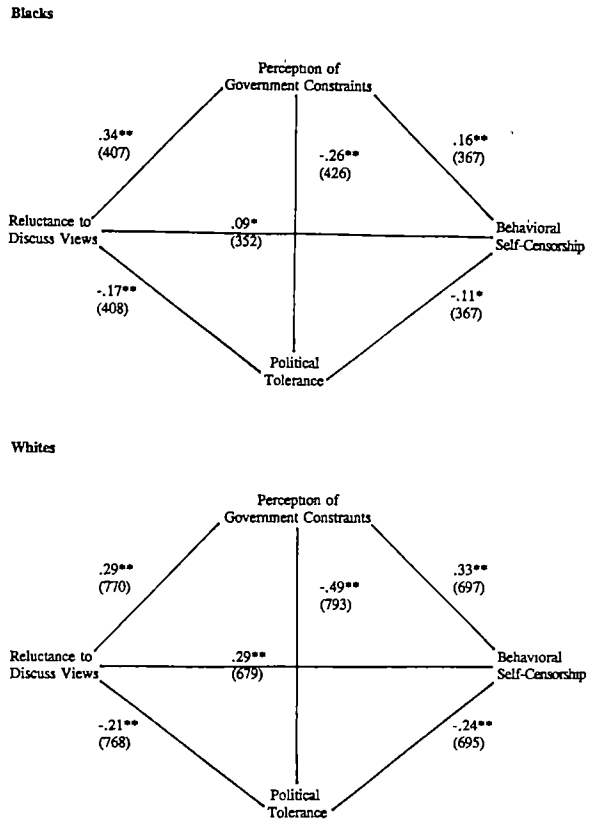
Does cultural intolerance inhibit freedom? While the analysis presented is consistent with that hypothesis, it does little to elucidate the causal pathways. For that, it is necessary to turn to somewhat different data about the interpersonal networks of the respondents.

Networks

The most direct test of the hypothesis that lack of freedom flows from cultural intolerance would re-

FIGURE 1

The Simple Interrelationship of Tolerance and Perceptions of Freedom, Black and White Mass Publics



Note: Entries are bivariate correlation coefficients. The numbers in parentheses are the numbers of cases on which the correlations are based.
*$p \leq .05$.
**$p \leq .01$.

quire that interpersonal networks be mapped and interviewed (e.g., Knoke and Kuklinski 1982). It would then be possible to correlate attributes of the network with perceptions of the individual. This sort of research design is extremely costly and difficult to execute,[21] and because many sanctions for nonconformist behavior are no doubt situationally determined, it would still only provide limited evidence of central tendencies. An alternative research design is therefore necessary.

Following the methodology of earlier research (e.g., Huckfeldt and Sprague 1991; Leighley 1990; MacKuen and Brown 1987), I will test the cultural hypothesis using respondent reports about their immediate set of peers. The respondents were asked to name three individuals with whom they "discuss important matters."[22] They were then asked to specify the party identifications of each of the people, as well as the frequencies with which they talk about political matters. Using these variables, an indirect test of contextual constraints on freedom can be conducted.

One of the most important attributes of the micro environments of individuals is diversity.[23] Social scientists have long argued that exposure to political diversity contributes to political tolerance (e.g., Nunn, Crockett, and Williams 1978). As one is exposed to a greater range of political ideas, one can become aware that alternative viewpoints are possible and legitimate and that one's own view may not be absolutely correct (see Duch and Gibson 1992). Political homogeneity generates and reinforces a closed-mindedness that is not conducive to political tolerance. As Blau noted: "The attenuation of profound social bonds that firmly integrate individuals in their communities is often deplored. But strong ingroup bonds restrain individual freedom and mobility, and they sustain rigidity and bigotry. Diverse intergroup relations, though not intimate, broaden horizons and promote tolerance, and they are the basis of macro-social integration" (1974, 623). Those with more diverse peers are expected to perceive a greater level of political freedom because group diversity nurtures political tolerance.

Using the respondents' reports on their networks, some indication can be got of the political diversity of these peer groups by comparing the party identifications of the respondents with their reports of their friends' party identifications.[24] An overall index of exposure to partisan diversity can be created by simply counting the number of members of the peer group that has perceived affiliations differing from the respondent's party attachment.[25] The index ranges from zero (no friends with different party affiliations) to three (all three friends are of a party different from the respondent's).[26]

Only a small majority (60%) of the respondents can identify the party affiliations of at least two of the three people in their networks. Of those who know the party of at least one of their friends, most report fairly homogeneous peer groups: 50% of the sample identifies no members of the immediate peer group with different party affiliations, while only 8% has three friends with differing party affiliations. This finding of relative network homogeneity is common in the research literature (e.g., Marsden 1987).

This measure of homogeneity can be further weighted by the frequency of political discussion; that is, each respondent was asked to indicate the frequency with which he or she discussed politics with each of the three peers. Thus an overall index score can measure the frequency of discussion of politics with people of differing partisan persuasions.[27] This is a more sensitive measure of exposure to political diversity. I hypothesize that greater exposure to political diversity is associated with greater political tolerance and with a greater sense of political freedom. Table 3 reports the analysis.

The direct relationship of network partisan heterogeneity and perceptions of freedom is tiny. The

TABLE 3

The Impact of Peer Group Heterogeneity on Political Freedom

PERCEPTION OF FREEDOM	PEER GROUP	
	HETEROGENEITY	HETEROGENEITY–POLITICIZATION
Blacks		
Perceptions of government constraints	−.01	−.11*
Reluctance to discuss political views	−.05	−.06
Behavioral self-censorship	−.00	−.14**
Whites		
Perceptions of government constraints	−.03	−.21**
Reluctance to discuss political views	.03	−.10**
Behavioral self-censorship	−.04	−.07*

Note: Entries are bivariate Pearson correlation coefficients.
*$p \leq .05$.
**$p \leq .01$.

correlations for blacks and whites are quite weak. Individuals who have more heterogeneous groups of peers perceive little more freedom available than those with homogeneous peer groups.[28] In light of the variability in political content and the frequency of discussion, this finding is perhaps not surprising.

When the diversity measure is weighted by the frequency of political conversations, the relationships are stronger and, in most cases, statistically significant. When political discussion is common within the peer group *and* the peer group is composed of members with differing party identifications, blacks become significantly less likely to engage in behavioral self-censorship or to perceive governmental constraints on their freedom. Under similar conditions, whites perceive fewer governmental constraints, are less reluctant to talk about politics, and engage in less behavioral self-censorship. Peer group diversity thus contributes to greater perceived freedom. Were a more direct measure of peer group tolerance available, these relationships would likely be stronger. Nonetheless, I take these data as supportive of the basic hypothesis that exposure to diversity legitimizes differences and thereby contributes to political freedom.[29]

This analysis concerns peer groups that are made up of a variety of types of individuals, each treated as having equal importance for the respondent. Peer groups are certainly of some importance for learning about the limits of political freedom. However, it is likely that people also learn lessons about freedom from their experiences within an even more micro environment, the family. In particular, I hypothesize that the political intolerance of a spouse has something to do with how one perceives political freedom. The available data allow at least an indirect test of this proposition.

Intolerance in the Family

For most married couples in the United States, spouses represent an important reference group. When asked to name three other people with whom important matters are discussed, nearly 76% of the married people in this national sample named their spouses. Fully 39% listed a spouse as *the* most important person. Moreover, there is considerable political content to the discussions that husbands and wives have. Nearly 25% of the sample report that they frequently talk to a spouse about politics, while only 14% claim that they talk quite rarely to a spouse about politics. It is reasonable to ask, therefore, to what extent the intolerance of a spouse contributes to perceptions that freedom is best not exercised.

The optimal research design requires that spouses be interviewed and be interviewed independently of their mates. Such a design is feasible (see Brickell, Huckfeldt, and Sprague 1988; Huckfeldt and Sprague 1989, 1991); but it is quite costly and difficult to implement. An alternative is to allow respondents to serve as informants on their spouses. Such a strategy probably cannot produce reliable direct information about spousal tolerance but can certainly be used to gather data about levels of education, religious affiliation, and so on.[30] Perhaps the data that can be reliably collected can be used to estimate the data that cannot be reliably collected.

The respondents in the 1987 survey were queried about a variety of attributes of their spouses. Fortunately, many of the demographic characteristics that can be faithfully reported in the survey are useful predictors of intolerance and can be used to estimate the intolerance of each respondent's spouse. Several steps (each involving important assumptions) must be taken in order to estimate spousal intolerance.[31]

The intolerance of the spring 1987 respondents was regressed on several demographic attributes of the subjects (see Table 4).[32] The regressions were conducted separately for the various combinations of race (blacks and whites) and gender. The sample was separated by gender in order to allow estimation of spousal intolerance. The separation by race was useful because such significant differences in intolerance (as well as the predictors of intolerance) exist by racial

TABLE 4

Equations for Estimating Spousal Tolerance (Married Respondents Only)

PREDICTORS OF POLITICAL TOLERANCE	UNSTANDARDIZED REGRESSION COEFFICIENTS			
	BLACKS		WHITES	
	MEN	WOMEN	MEN	WOMEN
Education	.228	.351	.747	.862
	(.207)	(.208)	(.090)	(.141)
Church attendance	−.539	−.186	−.349	−.452
	(.252)	(.287)	(.101)	(.108)
Occupational prestige	.059	.018	.026	−.005
	(.041)	(.053)	(.020)	(.024)
Religious fundamentalism	.229	.356	−.411	.186
	(.885)	(.865)	(.346)	(.378)
Family income	.158	.078	−.084	.391
	(.242)	(.219)	(.146)	(.165)
Intercept	3.671	1.621	1.387	−5.411
	(3.772)	(3.766)	(1.877)	(.2354)
Minimum pairwise N	67	66	269	224
R^2	.168	.095	.359	.308

Note: Entries shown in parentheses are standard errors. The minimum pairwise Ns shown are the unweighted numbers of cases.

groups. The data reported in Table 4 are unstandardized coefficients, allowing comparison across the samples; and they are derived from only the portion of the sample that reported being currently married.

It should first be noted that the demographic model explains a considerable amount of the variance in tolerance among whites but a much reduced amount among blacks. The tolerance of whites is fairly well predicted by their demographic attributes, while the tolerance of blacks is not. Moreover, there are large differences in the intercepts that vary by race (as well as by gender). The large amount of unexplained variance among blacks should make us cautious in using these equations to estimate spousal intolerance.

Among whites there are some important differences across gender in the predictors of tolerance. Religious fundamentalism contributes somewhat to intolerance among men but has a much reduced effect among women. On the other hand, greater family income enhances the tolerance of women but has little effect on men. The largest difference observed is in the intercept. When education, income, and occupational prestige are at their lowest and when church attendance and religious fundamentalism are at their highest, women are considerably more intolerant than men (and this seems to be true across races). The tolerance levels of men are also slightly more predictable than those of women.

It is not my purpose to present a fully specified model of gender and racial differences on the basis of the data in Table 4. The indicators available are not suitable to that task. Instead, I aim to use these characteristics to generate an *estimated spousal tolerance score*. Because the tolerance of blacks is so unpredictable by this equation, I will exclude them from further consideration. Using the coefficients reported in the table and each respondent's reports of the characteristics of a spouse on these five variables[33] (and assuming that all marriages are heterosexual and not interracial) estimated scores can be derived for the married portion of the sample. The scores are estimated from the equation that is appropriate for the gender and race of the spouse, and the values for the independent variables are the attributes of the spouse (as provided by the respondent). These estimates provide some indication of the level of intolerance of the spouse.[34]

As expected, the correlation of the tolerance of white couples is relatively high (r = .43). Tolerant women tend to marry tolerant men and vice versa. Relative to other characteristics of couples, this is not a strong correlation, however. For instance, levels of education of husbands and wives are correlated at .63, and frequencies of church attendance are related at .72. Though this no doubt reflects in part the estimation of spousal intolerance, rather than the direct measurement of it, there is some degree of heterogeneity within families in levels of intolerance.

It is not possible to sort out the direction of causality in these data. We cannot determine whether the intolerance of husbands is caused by their wife's intolerance, or vice versa. We can, however, test the hypothesis that spousal intolerance is associated with a sense of lack of freedom. Where one's spouse is more intolerant, I expect to see greater self-censorship and a greater sense of constraints on personal political freedom. Because there are important gender differences, the relationships must be considered

TABLE 5

The Impact of Spousal Tolerance on Political Discussions

POLITICAL DISCUSSION WITH FAMILY	SPOUSE'S TOLERANCE		
	LOW	MEDIUM	HIGH
Male respondents			
Percentage talking about politics "often" with family	19.1 (67)	16.1 (87)	42.9 (49)
Percentage reluctant to talk about politics with family	15.2 (66)	13.8 (87)	4.2 (48)
Female respondents			
Percentage talking about politics "often" with family	21.1 (37)	20.9 (91)	22.2 (72)
Percentage reluctant to talk about politics with family	20.5 (39)	15.7 (89)	12.7 (71)

Note. Figures in parentheses are the *total* numbers of cases upon which the percentages are based (white married respondents only).

separately for men and for women. (As before, the analysis is confined to whites.)

Table 5 reports the relationship between spousal intolerance and the frequency and nature of familial political discussions among whites. Two indicators are reported: the percentage of respondents reporting that they "frequently" hold political discussions with their families and the percentage who claim that they sometimes feel reluctant to express their political views to their families.[35] I hypothesize that spousal intolerance contributes to unwillingness to discuss politics openly.

Intolerance limits political discussions, but the findings vary significantly by gender. Among men, the intolerance of wives significantly affects not only the frequency of political discussion but the openness of discussion, as well.[36] Where wives are more tolerant, political discussions are more common, and husbands are more likely to express their true political views. Thus, these data support the hypothesis. However, the intolerance of women's husbands has virtually no effect on the frequency of political discussions and only a slight effect on the openness of conversations. The gender differences are fairly stark: 43% of the men married to tolerant wives often talk about politics with their families, while only 22% of women married to tolerant husbands discuss politics often. Whether under conditions of low spousal tolerance or high spousal tolerance, women are more likely to censor their political views than are men and thus are less likely to realize the benefits of increased tolerance within marriages. Thus (perhaps ironically), the tolerance of wives has a bigger impact on family political discussions than does the tolerance of husbands.

The frequency and content of political discussions is no doubt influenced in part by levels of interest and competence in politics. If gender differences exist, then they might account for the differences shown in Table 5.[37] Instead of pursuing these differences, it might be more useful to turn to the general beliefs respondents hold about personal political freedom and consider the correlations between spousal intolerance and general perceptions of freedom for men and women.

White men who are married to less tolerant women tend to perceive greater governmental repression ($r = -.37$, $p < .01$). The same is true of women who are married to less tolerant men ($r = -.35$, $p < .01$). To the extent that these views are derived from familial experience, men and women react similarly. Moreover, there is an *independent* impact of spousal tolerance on perceptions of government repression; that is, when these perceptions are regressed on the respondent's *own* intolerance and a spouse's intolerance, the latter variable has a significant and nontrivial effect on perceptions. The increment in R-squared is a highly significant .03; and beta is $-.19$ (s.e. = .04).

However, there are hints of some differences between men and women in terms of self-censorship of political talk (if not action). Spousal intolerance has some effect on the willingness of men to express their political views ($r = -.16$, $p < .01$) but a more substantial effect on the general willingness of women to express their views ($r = -.28$, $p < .01$). On the other hand, when it comes to expressing their political views behaviorally (which usually takes place outside the home, anyway), neither men nor women are much affected by their spouse's intolerance (men, $r = -.14$, $p < .05$; women, $r = -.11$, $p > .05$). In terms of general self-censorship, we see an effect on willingness to express opinions that is opposite of that already observed. The views of

women are affected by their husband's intolerance, but the views of men are little affected by the intolerance of their wives.

Thus, we have something of a paradox. Men with less tolerant wives tend to have fewer and less open political discussions at home, but their overall levels of perceived freedom are little affected. On the other hand, women with less tolerant husbands are neither more nor less willing to express themselves at home but generally perceive less freedom. It appears from these data that the experiences at home of married white men are not generalized to politics at large, while the experiences of women at home are generalized. If the South Bend, Indiana findings of Brickell, Huckfeldt, and Sprague (1988) are generalizable, this may be due to the tendency of men to discount and devalue political discussions with their wives. (See also Weiner 1978, which also reports a greater effect of husbands on wives' partisan affiliations than vice versa.) This conclusion, though highly speculative, is worthy of further investigation.

Apparently, people do learn something of the limits of freedom from their experiences in their families. Perhaps, however, this is in part a function of larger political environment in which individuals and families exist. Hence, I will turn my attention to a different sort of contextual analysis.

Community Intolerance and Perceptions of Freedom

Contextual analyses also profit from moving beyond individual networks to the macro environment of the individual (e.g., MacKuen and Brown 1987). It is useful to consider whether intolerance in the larger social and political community within which the respondent resides affects levels of perceived freedom. To explore the impact of community intolerance we first have to consider some important measurement and aggregation issues.

The traditional approach to measuring political tolerance derives from Stouffer's (1955) pathbreaking study and involves asking respondents whether groups assumed to be unpopular should be allowed to engage in certain activities. The General Social Survey frequently includes 15 measures of this sort. The measures are structured around five groups and three activities. The group stimuli are (1) "somebody who is against all churches and religion," (2) "a person who believes that Blacks are genetically inferior," (3) "a man who admits he is a Communist," (4) "a person who advocates doing away with elections and letting the military run country," and (5) "a man who admits he is a homosexual." The activities are (1) making a speech in the community, (2) teaching in a college or university, and (3) such a person's book being in the public library. The responses are collected through a dichotomous response set.[38] Though these items have been criticized (e.g., Sullivan, Piereson, and Marcus 1982) and are certainly limited in important ways, they have been used repeatedly by scholars interested in political tolerance (e.g., McCutcheon 1985; Bobo and Licari 1989; and Sniderman et al. 1989). I have recently shown that the Stouffer measures are remarkably similar to the Sullivan, Piereson, and Marcus "least-liked" approach (Gibson n.d.[a]). Especially since the traditional left-wing targets of intolerance were expanded (in 1976) to include militarists and racists, the items are less susceptible to the critique of Sullivan, Piereson, and Marcus.[39]

A scale based on the overall responses to the items can be of some utility as a measure of community intolerance. The tolerance index is the mean of the responses to these 15 items.[40] The mean index score is .56, with a standard deviation of .33; and the index is extremely reliable (Cronbach's alpha = .92). Generally, these data show remarkable temporal stability.

In order to test the contextual hypothesis, a measure of the intolerance of the local community must be derived. There are two possible geographical units that could serve to define the local community: the primary sampling unit and the block (enumeration district). In the 1980s, the National Opinion Research Center (NORC) consistently used the same 84 primary sampling units. These are fairly large geographical units, consisting of counties, standard metropolitan statistical areas, independent cities, and (in New England) parts of counties. The blocks are much smaller than primary sampling units. The 1980 sampling frame included 562 of these units.[41] The advantages and disadvantages of using the block as the measure of community opinion are obvious: the block has immediacy for the respondent, but the block mean is based on few cases. The primary sampling unit means are based on many more cases, but the primary sampling unit may not impinge much on the individual respondent. In the analysis that follows, I will aggregate by both units, bearing in mind the limitations of each.[42]

To what degree is the intolerance of the community reflected in the intolerance of the individual respondent? We can address this problem by correlating the measure of individual intolerance with the mean tolerance score for the primary sampling unit and block. Note that the tolerance score for the individual is operationalized differently from the tolerance score for the community[43] and that the magnitude of cross-level correlations is invariably quite small. Furthermore, the tolerance scores of the individual respondents are not used in constructing the aggregate scores for the primary sampling units and blocks in which they live. Nonetheless, the correlation between individual tolerance and average tolerance within the block is .33 for whites and .19 for blacks. For average tolerance within the primary sampling unit, the correlation is .22 for whites and .12 for blacks. That people are more like their immediate neighborhood than the larger community is not surprising. However, the more interesting question is whether the tolerance of the community is associated with perceptions of freedom by individual residents. The data relevant to this hypothesis are shown in Table 6.

TABLE 6

Community Tolerance and Perceptions of Freedom

PERCEPTION OF FREEDOM	AVERAGE COMMUNITY TOLERANCE: PRIMARY SAMPLING UNIT	AVERAGE COMMUNITY TOLERANCE: BLOCK
Blacks		
Perceptions of government constraints	−.16**	−.23**
Reluctance to discuss political views	−.13	−.19**
Behavioral self-censorship	−.20**	−.14**
Whites		
Perceptions of government constraints	−.17**	−.28**
Reluctance to discuss political views	−.09*	−.18**
Behavioral self-censorship	−.10**	−.13**

Note: Entries are bivariate Pearson correlation coefficients.
$p \leq .05$.
**$p \leq .01$.

The correlations reported in Table 6 are quite supportive of the hypothesis. Among both blacks and whites, those living in more tolerant communities are considerably more likely to perceive political freedom. For instance, intolerance in the community is significantly associated with perceptions of greater repression and with less willingness to express one's political views. Less tolerant communities seem to be successful at limiting freedom. These coefficients are all the more impressive when it is noted that they are cross-level correlations, so that all within-community variance in levels of perceived freedom must be treated as error variance.

It is impossible to sort out the causal pathways linking community intolerance with self-censorship. It is possible, for instance, that the community creates a set of norms that legitimize intolerance, that communal intolerance gets reflected in individual intolerance, and that citizens would deny themselves freedom just as they would deny their enemies freedom. Other processes are equally plausible. The only statistical test that is helpful in trying to unravel the process is to determine whether community intolerance has an impact on perceptions of freedom *controlling for the intolerance of the individual*. Data relevant to this hypothesis are shown in Table 7.

Table 7 reveals that community intolerance *does* have some effect on perceptions of freedom beyond the individual's own intolerance. For instance, for blacks, the beta coefficient relating community (block) tolerance to perceptions of governmental constraints on freedom is a highly significant −.18. For whites, the coefficient is −.14 (which is also significant at less than .01). These effects are not large either, and they are generally stronger for the block-level data than for the primary sampling unit data. But since the statistical tests are so strongly biased against the hypothesis, I take this as important support for the proposition that intolerance in the community limits available freedom.

Thus, there is fairly persuasive evidence that those who perceive constraints on their freedom live in communities characterized by higher levels of political intolerance. I cannot be certain that perceptions of

TABLE 7

The Independent Impact of Community Tolerance on Freedom

PERCEPTION OF FREEDOM	PRIMARY SAMPLING UNIT	BLOCK
Blacks		
Perceptions of government constraints		
R^2	.08**	.10**
Beta, individual tolerance	−.24**	−.22**
Beta, community tolerance	−.12**	−.18**
Reluctance to discuss political views		
R^2	.04**	.06**
Beta, individual tolerance	−.16**	−.14**
Beta, community tolerance	−.12*	−.17**
Behavioral self-censorship		
R^2	.05**	.03**
Beta, individual tolerance	−.08	−.09
Beta, community tolerance	−.18**	−.12*
Whites		
Perceptions of government constraints		
R^2	.25**	.26**
Beta, individual tolerance	−.48**	−.45**
Beta, community tolerance	−.07*	−.14**
Reluctance to discuss political views		
R^2	.05**	.06**
Beta, individual tolerance	−.20**	−.17**
Beta, community tolerance	−.05	−.12**
Behavioral self-censorship		
R^2	.06**	.06**
Beta, individual tolerance	−.22**	−.21**
Beta, community tolerance	−.06	−.07

*$p \leq .05$.
**$p \leq .01$.

lack of freedom are caused by local cultural values encouraging conformity. To unravel the complex causality involved here is far beyond the limits of the data and research design. Nonetheless, where there is greater intolerance in a community, one is more likely to find citizens who perceive significant costs to the exercise of political liberty.

The analysis presented here supports several conclusions. First, it appears that the perceptions ordinary citizens hold of political freedom can, in fact, be rigorously measured. Second, there is a surprisingly low level of perceived political freedom in the United States. Third, large and very important racial differences exist in how much freedom is perceived, with African-Americans asserting that there are many more constraints on their freedom than do whites.

More important are the findings connecting perceptions of freedom and intolerance. Not only are those who are intolerant more likely to perceive constraints on their own freedom, but I have also adduced evidence that intolerance in the external environment contributes to a lack of perceived freedom. More homogeneous (and presumably less tolerant) peer groups, less tolerant spouses, and less tolerant local communities all seem to limit how much freedom ordinary Americans perceive. One reason why perceived freedom is so low seems to be that intolerance is so pervasive within American political culture.

There are many important limitations to this research; and as a consequence of the limits of this research design, care must be taken in ascribing too much certitude to the conclusions. Nonetheless, there is a strong suggestion of a close connection between how people think about their own freedom and what freedom they would grant to their political enemies, as well as some more inferential evidence of a linkage between cultural intolerance and perceived limits on political freedom.

IMPLICATIONS

Does political tolerance matter? The cumulative weight of evidence from this analysis suggests that tolerance does matter. Though I have taken some risks in these analyses—with the consequence that no single bit of evidence is irrefutable—the data point repeatedly toward a connection between intolerance and lack of freedom. I have traced this relationship at the level of the individual, the family, and the community. Intolerance in American political culture appears to set at least broad constraints on political liberty. Those who study political tolerance can therefore take succor in these findings. It appears that we have not devoted our research efforts to analyzing attitudes of little or no political consequence.

At the same time, however, my analysis points to an entirely new frame of reference for tolerance research. No longer should we focus exclusively on the individual survey respondent as the most interesting unit of analysis. Why people differ in their levels of intolerance—and with what consequences—cannot be well understood by conceptualizing the individual in social isolation. John Donne's "No man is an *Iland*, intire of it selfe; every man is a peece of the *Continent*, a part of the *maine*" is an important and quite relevant prescription for social scientific research on the implications of political intolerance.

In Noelle-Neumann's "spiral of silence" people are encouraged "either to proclaim their views or to swallow them and keep quiet until, in a spiraling process, the one view dominated the public scene and the other disappeared from public awareness as its adherents became mute" (1984, 5). Perhaps more relevant to the case of the United States, Tocqueville observed many years ago, "I know of no country in which there is so little independence of mind and real freedom of discussion as in America" (1948, vol. 1, p. 263). Survey evidence from the contemporary United States seems to support this view, and this is a problem of considerable consequence for democracies. Indeed, the loss of respect for dissent and nonconformity in nominally democratic regimes is perhaps one of the greatest threats to political freedom (see Duch and Gibson 1992).[44] Without a culture that legitimizes political opposition, those outside the centrist mainstream have few political opportunities. Ultimately, the political system loses its democratic vitality. If this is a consequence of mass political intolerance, then, certainly, intolerance matters.

APPENDIX: RESEARCH DESIGN

The analysis I have reported is based on a national survey conducted in 1987. The survey was an extension of the 1987 General Social Survey (GSS). The GSS is a nearly annual survey conducted by the NORC with funding from the National Science Foundation. The sample for the GSS is a full multistage probability sample of English-speaking adults living in the continental United States. The 1987 GSS was conducted in the spring, with a response rate of approximately 75%.

In June and July, the respondents in the 1987 GSS were resurveyed. Of the 1,466 subjects in the spring GSS, 1,106 were eligible to be interviewed. (The NORC reserved approximately 350 respondents for possible use in future panel studies. Those subjects were not eligible to be reinterviewed.) This subsample was selected randomly, within gender strata. Because the 1987 GSS cross-section had a relatively large differential nonresponse rate by gender, there is some gender imbalance in the pool of subjects eligible for the second-wave interview. Consequently, stratified random sampling was thought desirable. For the reinterviews, males and females were selected with equal probability. Approximately 87% of these subjects were reinterviewed. Most of these were in-person interviews although, because some subjects had moved since the earlier interview, some small

percentage of the reinterviews were conducted by telephone.

The 1987 GSS also included a special oversample of 353 black respondents (i.e., beyond the 191 blacks in the main GSS sample). This sample was also a full probability sample. This means that extraordinary efforts had to be mounted in order to draw this supplementary sample. Roughly five thousand households nationwide were sampled and contacted in order to locate the black subjects. The response rate for the black respondents was 79%. All of these subjects were eligible for the reinterview, and reinterviews were successfully completed with nearly 90% of the original subjects. It should be noted that the black oversample also overrepresents females. Because the universe of the black oversample was selected for inclusion in the reinterview project, no sampling techniques could ameliorate this problem. Thus, interviews were completed with a total of 1,267 respondents. Since the sample is stratified by race, most analyses are conducted on weighted data.

Special thanks are due Tom Smith and Jim Davis, coprincipal investigators on the GSS, and the GSS Board of Overseers for their assistance on this project. Dick Rubin of the NORC was instrumental in bringing the reinterview survey to a successful conclusion.

Notes

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1. Highlights in a fairly voluminous literature on political intolerance include Barnum and Sullivan 1989; Bobo and Licari 1989; Lawrence 1976; McClosky 1964; McClosky and Brill 1983; Nunn, Crockett, and Williams 1978; Prothro and Grigg 1960; Sniderman 1975; Stouffer 1955; Sullivan, Piereson, and Marcus 1982; Sullivan et al. 1985. I would also mention my own work: Gibson 1989b; Gibson and Bingham 1985; and Gibson and Duch 1991a.

2. The elitist theory of democracy is actually an amalgam of the work of a variety of theorists, including Berelson, Lazarsfeld, and McPhee 1954; Key 1961; Kornhauser 1959; and Lipset 1960. The most useful analysis of the similarities and differences among the theories can be found in Bachrach 1967. For a popularized summary of the theory see Dye and Zeigler 1987 (see also Dye 1976). My more recent analysis of some of the propositions of elitist theory can be found in Gibson 1988, 1989a; Gibson and Bingham 1984; and Gibson and Duch 1991b.

3. On the other hand, the literature linking opinion and policy in other substantive areas is voluminous. For recent examples, see Erikson, McIver, and Wright 1987; Erikson, Wright, and McIver 1989.

4. However, I have suggested that a climate of tolerant mass opinion during the 1970s may have contributed to acceptance of dissent as a legitimate form of political action (Gibson 1989a). Dissent, in turn, was an important stimulus for repressive policy. Thus, mass opinion indirectly affected policy, but the direction of the relationship is ironic. Where opinion was more tolerant, there was *more* political repression. Moreover, even if opinion does not produce a specific expectation for public policy, it may set broad constraints on the ability of elites to implement their own preferences.

5. Sprague defines *contextual effect* as "variation in political behavior that depends, systematically, on properties of the environment within which that behavior is embedded" (1982, 99). MacKuen writes "The quality of political life produced by *individual* choices made in a *collective* context differs fundamentally from one that might be expected from a simple aggregation of individual preferences" (1990, 61; emphasis original).

6. There is a large and important literature on pressures toward political conformity, beginning in the 1960s, including Cox 1974; Fields and Schuman 1976; Finifter 1974; Krassa 1990; MacKuen 1990; MacKuen and Brown 1987; McPhee 1963; Putnam 1966; Segal and Meyer 1974; Weatherford 1982; and the many works of Huckfeldt and Sprague. For a review of the social psychological literature on conformity see Moscovici 1985.

7. In a brilliant formal analysis of the contextual constraints on political discussion, MacKuen (1990) has addressed many of the processes I consider here. The overall implication of his analysis is that "public, if not private, conformity is likely to occur even when many individuals are perfectly willing to encounter contrary views" (p. 85). The linchpin of his analysis is an individual propensity for political interaction he terms *expressivity*. Were one to pursue an empirical analysis of expressivity, it would surely look something like the perceptions of political freedom that I analyze here. Note also that MacKuen agrees about the sensitivity of political discussion to social environments (pp. 94–95, n. 5).

8. Wildavsky (1987) has argued a similar position. Rejecting the proposition that citizen preferences are exogenous variables for political analysis, he suggests that choices reflect variations in relationships to the political culture, that is, they emerge from social interaction. Individual choices are choices of culture, they are decisions that reflect fundamental orientations toward the shared values that legitimate different patterns of social practices: "Put plainly, people decide for or against existing authority" (p. 5).

9. This technique, often dubbed the "least-liked" approach, was developed by Sullivan, Piereson, and Marcus (1979, 1982).

10. For a more complete explication of the measurement and distribution of opinion see Gibson 1987b, 1989b. The objective of the measures was to insure that all respondents were asked whether they would tolerate political activities by a) the two groups they disliked the most *and* b) Communists and members of the Ku Klux Klan. These objectives produced a somewhat complicated question format. All respondents were asked whether they would tolerate their two most-disliked groups. Those who named Communists and Klansmen as their two most-disliked groups (in either order) were then asked whether they would tolerate their third and fourth most-disliked groups. Respondents naming neither the Klan nor Communists as among the two most-disliked groups were asked about these two groups, rather than their third and fourth most disliked groups. The rest of the respondents were asked questions designed to insure that they evaluated Communists and Klansmen at some point in the question sequence. The precise group stimulus depended upon the

configuration of groups named as first through fourth most disliked.

11. I recognize that a distinction is typically made between negative and positive conceptions of freedom (see Berlin 1958). The negative view of freedom argues that to free people is to remove restraints on their actions. Rights perform this function by guaranteeing broad areas of autonomous activity within which the individual can make free choices. The positive view of freedom emphasizes rational self-determination. Freedom is thus assessed not in terms of whether conditions for free choice prevail but, rather, by the objective rationality of the choice. Decisions that are seemingly unrestrained but irrational are not free choices. The distinction between negative and positive freedom has been subjected to a variety of attacks and has been much debated (e.g., MacCallum 1967). For my purposes, it is more important to distinguish between subjective (perceived) and objective freedom than positive and negative freedom. Preston's (1982) conceptualization allows me to do just that. There is a certain asymmetry to my argument. Perceived restraints on choices—perceptions that certain political activities will generate adverse repercussions—certainly are evidence of the lack of freedom, even if I am on less certain ground when I claim that the *absence* of perceived restraints is equivalent to freedom. Nonetheless, if the absence of perceived constraints is grounded in reality, this is an important component of positive freedom. Clearly, those who expect to be penalized for their political activity are not free. In this sense, my conceptualization of freedom is closer to the negative view than to the positive view.

12. For a more extended discussion of this approach to freedom, see Gibson n.d.(b).

13. There are, of course, alternative conceptualizations of democracy and of democratic freedom, and I certainly do not claim this as the only legitimate approach. The emphasis I place on democracy as procedure (without much regard for substantive public policy) must be born in mind throughout the analysis.

14. This view of personal political freedom is focused on the individual citizen. It looks directly toward the attributes and beliefs of individuals, as well as their interactions with their immediate environments, to gain an understanding of freedom. In adopting this approach, I do not deny the utility of alternatives (e.g., focusing on the attributes of cultures or polities). But my basic premises are that what people believe about their own freedom is important; that what they perceive about the opportunities for political expression and the costs associated with such expression has much to do with their exercise of freedom; and that it is useful to regard the degree of freedom available and the patterns of allocation of freedom within a polity as ultimately reducible to a study of the beliefs of individual citizens. I do not gainsay the utility of considering other sorts of freedom; and I shall later try to identify some linkages between the individuals and the larger social and political environment within which they function. But the approach here must be recognized from the beginning as a particular, and not necessarily inclusive, approach to the problem of perceived personal political freedom.

The most useful conceptualizations of freedom are embedded in larger theories of politics. The theory I employ is most generally liberal–democratic, with specific reference to Dahl's theory of polyarchy. This perspective emphasizes obstacles that individual citizens might perceive to frank political expression. This is not a freedom of unconstrained licentiousness. Instead, it is freedom to engage in political activity that is essential to the effective functioning of polyarchies. It is quite possible for others to conceive of freedom differently, emphasizing, for instance, the potential (whether perceived or not) of individuals to develop nascent capacities for expression.

As much as I might wish to avoid the issue, I also have to acknowledge the distinction between objective and subjective freedom. My approach is to study subjective freedom—the freedom that is perceived by individuals. A different sort of freedom (e.g., the degree of freedom acknowledged by law) might be measured more objectively (and as an attribute of polities, not of individuals). Within my conceptualization of the problem, *there is no necessary relationship between objective and subjective freedom*. People may perceive themselves as free even in the most repressive conditions. (That sort of "false consciousness' is not addressed in this research.) Alternatively, people may not perceive the freedom that is truly available to them. When I refer to *freedom* I refer to the freedom that exists in the minds and hearts of citizens, not to the freedom that exists in the law books or even in the broader culture.

15. Huckfeldt and Sprague (1989, 1201) have quite carefully distinguished between *contexts* and *networks*. For their purposes, it makes a great deal of sense to do so. I am a bit sloppier with the use of the term *context*, but I do use *network* as the respondent's peer group.

16. I recognize that irrespective of the freedom allowed by the larger political system or local networks, individuals may be unwilling to express themselves politically for completely psychological reasons. For instance, lack of self-confidence may lead people to be unwilling to risk the opprobrium of others (Bay 1970 terms this *psychological freedom*). Though the effect of psychological attributes might be interactive (in the sense that personality attributes structure perceptions of, and reactions to, external environmental stimuli), the effect may exist completely apart from the objective attributes of the environment. This sort of psychological freedom is not addressed in this research. Instead, the psychological attributes of individuals are treated as independent variables, as predictors of social and political freedom (see Gibson n.d.[b]). Thus, rather than asserting that psychologically based risk-aversion is a sign of unfreedom, I prefer to test the hypothesis that those who perceive less freedom are more risk-averse and psychologically insecure. These hypotheses are addressed in Gibson n.d.(b).

17. Those who "don't know" whether the government would allow them to engage in the activity might legitimately be included with those believing that they would not be allowed to do so. Since the *don't knows* are quite few, I have simply excluded them from the calculations of the percentages.

18. Note that these figures exclude those who would not engage in the activity under any circumstances, regardless of the popularity or unpopularity of the view. For most of the actions, this number is inconsequential. However, 44 subjects say they would not put a bumper sticker on their car under any circumstances, and 29 would not wear a political button under any circumstances.

19. The first component of freedom is the perception that the government would allow political activity. The first unrotated factor from the common factor analysis had an eigenvalue of 3.4 and accounts for 68% of the variance in the items. The second factor that emerged from the analysis had an eigenvalue of .72 (explaining just 14% of the variance) and was therefore ignored. As indicated by the factor loadings reported in the table, the items contribute roughly equally to the scale, with the exception of the low-variance item on nationwide strikes. This index is quite reliable (alpha = .88) and can serve as a measure of perceived external governmental censorship of political behavior. As expected, racial differences on this index were extremely significant, with blacks perceiving substantially more political repression.

The index of perceived social censorship of political expression was derived by factor-analyzing the items shown in the second portion of the table. The first unrotated factor had an eigenvalue of 2.3 and explained 46% of the variance in the items. The second factor, which was not used, had an eigenvalue of .93 (accounting for 19% of the variance). This index is quite reliable, with an alpha of .70; and only the item about the government is at all weakly related to the factor. On this index, blacks are significantly more fearful of political expression than are whites. An index of behavioral self-censorship was also constructed. The reliability of this index is also quite high (alpha = .86), and all of the items contribute roughly equally to the scale. The first unrotated factor had an

eigenvalue of 3.6 and explained 59% of the variance in the items. The second factor, which was not used, had an eigenvalue of .84 (accounting for 14% of the variance). The differences in the loadings across individual items are not of any substantive significance. Racial differences on this index are *not* significant. A fairly detailed analysis of the validity and reliability of these scales is available on request.

20. An alternative explanation for these findings is that the correlation between perceptions of freedom and tolerance is spurious, reflecting a common psychological origin. Consequently, it is useful to control for some additional variables before concluding that freedom and tolerance are linked. Three psychological attributes closely linked to political intolerance are closed-mindedness, or dogmatism (e.g., Gibson 1987a; Gibson and Tedin 1988; Sullivan, Piereson, and Marcus 1982), low self-esteem (Sniderman 1975), and anomie (Herson and Hofstetter 1975; McClosky and Schaar 1965; Quinley and Glock 1979; Zalkind, Gaugler, and Schwartz 1975). Each of the measures of tolerance and freedom is fairly strongly related to some personality attributes. The best predictor is dogmatism. Those who are more closed-minded are more intolerant, more likely to perceive government repression, and more likely to engage in self-censorship. This is true for both blacks and whites. Self-esteem has only a weak direct effect on tolerance, but is related to perceptions of government repression and willingness to express oneself in political conversations. This is probably due to the facility with which those high in self-esteem interact with others. Those who are high in anomie tend to be more intolerant and perceive more repression and are slightly more likely to engage in self-censorship.

The important question is whether perceptions of freedom and political tolerance are related even after controlling for their common origins in the personality traits of individuals. One fairly conservative test of the hypothesis involves calculating semipartial coefficients for tolerance as a predictor of perceived freedom and for perceived freedom as a predictor of tolerance (in both cases controlling for the personality traits). Among whites, this test leads to the conclusion that tolerance and perceptions of freedom are still connected, even with personality attributes controlled. Perceptions of freedom are related to tolerance, and tolerance is related to perceptions of governmental constraints and behavioral self-censorship but not to reluctance to discuss politics. Among blacks, the relationships are similar but generally weaker. Thus, even with a quite demanding test, there is evidence that tolerance and freedom are connected at the individual level. This is compatible with the hypothesis that intolerance may serve as an important constraint on freedom.

21. For an excellent example of this sort of research design see the series of papers by Huckfeldt and Sprague (e.g. 1988, 1989, 1991).

22. For the approach to discussion networks used in the GSS, see Burt 1984 and Marsden 1987.

23. Networks differ, of course, in a variety of aspects, including size, density, homogeneity, dispersion, span, reachability, and anchorage (and, of course, individual ties within the network can be characterized in a number of ways). By design, some of these attributes do not vary within this data set (e.g., size). From a theoretical point of view, peer group diversity is the most interesting attribute for political tolerance and freedom.

24. It is unclear whether respondents can accurately report the party identifications of their friends. For my purposes, this is not of great concern because it is the respondents' *perceptions* of their friends' affiliations that is crucial, not the true affiliations. Moreover, if there is bias in the reports, it is probably bias that reduces the measure of political diversity of the peer group and so works against the hypothesis under consideration.

25. This concept of network diversity is derived from Burt's (1983) definition of *network range*. This is simply the degree of diversity within the network (see Marsden 1987). The obverse of a broad range is a *dense network*. As Marsden asserts, density "also measures the potential strength of normative pressures toward conformity by indicating the capacity of alters to collectively influence the respondent" (p. 124).

26. Some measures of network diversity consider only the diversity of the network work members without regard to the characteristics of the primary respondent (e.g., Marsden 1987). Since I am specifically concerned with partisan diversity and its impact on freedom and conformity, it is important for my purposes that each dyad be scored for diversity or similarity.

27. This measure is based on six-point frequency-of-discussion variables for each of the three peers. Discussion with a peer of a differing party identification is weighted at 1.00; discussion with a peer of the same identification is discounted by .33; and discussion with a peer of unknown identification is discounted by .67. The overall index of diversity is simply the sum of the three discussion diversity measures (i.e., the measure for each peer). This measure ranges from a minimum of 1 to a high of 18 (mean = 6.8; standard deviation = 3.4). Racial differences are statistically significant, with whites reporting slightly greater peer group diversity than blacks.

28. It should be noted, however, that there are weak but nonzero relationships between the ability to report the party identifications of the peer group and levels of perceived freedom. Those who are more knowledgeable about their friends tend to perceive greater political freedom.

29. Marsden (1987) has shown that network diversity varies according to age (range is greatest among the young and middle-aged), education (more education is associated with greater diversity), race (whites have more diverse networks), and urban residence. In order to guard against the possibility that the findings reported in Table 3 are spurious, I estimated each equation separately by race, controlling for the respondent's age, education, and size of place of residence. The resulting regression coefficients are slightly smaller than those shown in Table 3, but the impact of heterogeneity–politicization on whites' perception of governmental constraints is still highly significant, as is the coefficient for heterogeneity–politicization on blacks' behavioral self-censorship.

30. The most systematic assessment of the reliability of spouse reports is Niemi 1974. Niemi concludes that spouse reports on the sorts of attributes considered here are extremely accurate (see also Weiner 1978). In a somewhat different context, Williams and Thomson have concluded, "Systematic errors in proxy reports are not a major problem. . . . Had we relied only on self and proxy reports collected from wives, our pattern of results would have been about the same as those obtained by using data collected from both spouses, but our estimates would have been attenuated by random measurement error" (1985, 120). Brickell, Huckfeldt, and Sprague (1988) have also shown that spouses are extremely accurate in reporting their mate's political preferences.

31. This technique closely parallels one employed by Huckfeldt and Sprague (1991). Based on their South Bend, Indiana, sample, they estimated a Reagan vote probability for each person with whom the primary respondent reported discussing politics. The equation was estimated from data on the primary respondent, regressing (logistic regression) vote choice on seven demographic attributes (e.g., union membership, education, income, age). The resulting equation was then used to estimate a *discussant* probability, based on the scores of the discussant on the seven demographic attributes. Except for the fact that the discussant attributes were gathered through direct interviews with the discussants, this technique is identical to the one I have employed here. For the specific details of the technique they use, see Huckfeldt and Sprague 1991, p. 132, n. 2.

32. The dependent variable is an index based on the responses to 15 tolerance items in the spring survey. The tolerance items involve three activities and five groups. The spring responses were used instead of the summer responses because (1) there are more respondents in the spring due to the ineligibility and nonresponse of some respondents in the second-wave interviews and (2) the measures of intolerance are temporally separate from the measures of perceptions of

freedom. The effect of the latter consideration is to *reduce* the size of the observed correlations.

33. Note that family income is a variable shared by the respondent and the spouse.

34. This technique is not unlike that developed by Charles Franklin (1989) for estimating missing data across data sets.

35. Note that the independent and dependent variables do not "fit" perfectly. The independent variable characterizes the tolerance of the respondent's spouse, while the dependent variable refers to familial discussions in general. This mismatch can only serve to weaken the observed relationships and thus works against the hypothesis.

36. Note that Huckfeldt and Sprague discovered an especially strong effect of the spouse's vote probability on the main respondent's vote (1991, 133). The effect of nonrelative discussants is quite a bit smaller and does not achieve statistical significance in their model.

37. Men and women differ little in their self-described levels of interest in politics. Gender is not a good predictor of interest (tau-beta = −.11), although about 10% more men than women claim high levels of political interest. Nor are there gender-based differences in levels of political knowledge.

38. The total set of tolerance questions were asked in 1976, 1977, 1980, 1982, 1984, 1985, 1987, 1988, 1989, and 1990. Roughly fourteen thousand respondents answered these questions.

39. The critique of these measures is that there is no assurance that the respondents dislike the groups and that without antipathy, tolerance is not relevant. Though it is presumed that most Americans do not care for communists, homosexuals, etc., no independent measure of affect toward these groups has been available. Without some assurance that these groups present a negative stimulus to most respondents, the conceptual requirement that one cannot *tolerate* that which one does not dislike cannot be met. Affect toward these and a variety of other groups was measured in my 1987 survey using a scale ranging from 1 (*like very much*) to 11 (*dislike very much*). The results were reported in Gibson 1989b. These data reveal that while there is no unanimity that these groups are highly disliked, there is a tremendous amount of agreement on the matter; and for great majorities of the respondents, the groups are a valid tolerance stimulus.

40. Each item was scored as 0 (intolerant), 1 (tolerant) and .5 (uncertain).

41. Some additional blocks are associated with the black oversamples that NORC has employed during the 1980s. These bring the total number of blocks to 612.

42. For the primary sampling units represented in the 1987 survey (i.e., excluding the special black primary sampling units that were not used in 1987), the averaged, weighted numbers of respondents per unit is 92, with a standard deviation of 32 and with a minimum of 55, and a maximum of 269, respondents. The average number of respondents per block is 13, with a standard deviation of 4.8, and a range of weighted Ns from 1 to 40. There are a total of 281 blocks represented in the data file for 1987. Note that this analysis is based only on the data from the surveys using the common 1980s sampling frame.

43. At the level of the individual, the correlation between the two tolerance measures is .63 for whites and .38 for blacks. Note that the Stouffer-based measure was derived from questions asked in the spring 1987 survey, while the generalized tolerance measure was constructed from summer 1987 questions.

44. Jennings (1987) has shown just how powerful the effect of dissent can be. Those who engaged in protest during the Vietnam War era are even today more likely to support civil liberties than those who did not protest, despite the fact that protesters and nonprotesters differed little in their attitudes prior to the college experience. Both groups became more supportive of civil liberties as a result of their college experience, but the experience of having participated in a demonstration or sit-in had a distinctive and lasting effect. To the extent that willingness to engage in protest behavior diminishes, support for the broad exercise of civil liberties will decline. See also Jennings and Niemi 1981, chap. 11.

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BENJAMIN FRANKLIN'S *AUTOBIOGRAPHY* AND THE EDUCATION OF AMERICA

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Franklin's Autobiography *was written in part to provide a model for the emerging democratic individual and democratic culture of America. Franklin's teaching in the work has been subject to severe criticisms from the beginning, though it has had many defenders, too. Neither friend nor foe, however, has taken a sustained look at the* Autobiography *itself to explore its teaching in detail. I look at Franklin's presentation of the relationship of wealth and virtue, his utilitarianism, and his vision of democratic society and find a subtle and robust ideal deftly calculated to educate and elevate American culture.*

Benjamin Franklin's *Autobiography* has lived a curious life. Begun by Franklin as a memoir to his son and his further posterity, as well as a vehicle for the pleasure found in recollection (1964, 43), it quickly took on—if it did not have from the start—a more public purpose (see Dawson 1977/78; Lerner 1987, chap. 1; Seavey 1988). Franklin inserted two letters at the beginning of part 2 to confirm that the *Autobiography*'s serious mission was nothing less than the education of a new nation, using Franklin's life and mind as its model. In fact, the work became not only a staple of American popular culture but a conduit through which that culture passed to other parts of the globe. It was printed and reprinted, in the United States and Europe, through the nineteenth century and beyond. It was influential in Japan at a time when the modern Japanese character was forming (Ki 1987). Parts of it made their way into McGuffy's readers, and Parson Weems did it the dubious honor of producing a bowdlerized version (Franklin 1964, 9–12).[1] Yet the work, like the man himself, has also had fierce critics; and these seem to have got the upper hand in this century, when Franklin's work—indeed, his reputation as a whole—are in profound eclipse. Of course, all the figures and ideas of the founding period are in relative eclipse now, with philosophy, politics, and even constitutional law sometimes claiming to have superseded the eighteenth-century models and popular culture's memory stretching barely two decades back. Still, Franklin's fall, from such a height, has been particularly stunning.

Some of the reaction against Franklin and his *Autobiography* stems from the view that they represent a quasi-Puritan outlook, or the outlook of Puritanism's alleged stepchild, the "Protestant ethic." Max Weber, who first defined this ethic, argued that Franklin's *Autobiography* embodies its "ideal type," in all its grim dutifulness (1958, esp. 48–50).[2] In a similar vein, Labaree notes that Mark Twain half-humorously inveighed against Franklin for producing an impossible deal of industriousness to be thrown up to all boys by their parents (Franklin 1964, 14–15). But if some critics have thought Franklin too strict, another strain of criticism finds him and his *Autobiography* too lax, in a moral sense. In his own day, Abigail Adams thought Franklin almost a libertine. John Adams criticized him for being all appearance, devoid of real substance (though Adams seems to have thought this about most everyone except himself).[3] This criticism, along with the notion that Franklin was morally shallow and self-serving, was carried forward by Adams's posterity and latter-day political sympathizers.[4] In the twentieth century, Franklin has been attacked to much greater effect by D. H. Lawrence for being morally base: materialistic, bourgeois, calculating.[5] And most recently, Alisdair MacIntyre, in his attempt to resuscitate a morality based on the virtues, has dismissed Franklin's moral ideal as shallow and utilitarian (MacIntyre 1984, 198–99, 243).

My purpose here is to reopen the question of Franklin's moral teaching and outlook as he presents it in his *Autobiography*; for while Franklin has had no lack of defenders, surprisingly little close attention has been paid to the text of his *Autobiography* with a view to drawing out the precise character of the moral ideal Franklin is promulgating through it. Such attention reveals a surprisingly subtle and multilayered moral teaching, one that easily escapes most of the criticisms that have been leveled against it. Its presentation is also carefully calibrated to influence the widest and most varied American audience. Education (including moral education) was a matter Franklin took seriously all his life, as many of his writings and projects attest.[6] The *Autobiography*, as it stands, is the final and most comprehensive monument of that concern, undertaking, as it does, to help shape the emerging American character.[7] Despite its casual and almost random appearance, the work contains a mature and consistent outlook on morality and the well-lived life and even a distinctive strategy for persuading its readers of the merits of that outlook.[8]

I do not propose to argue the elevation of Franklin's moral ideal when matched against such competitors as MacIntyre's Aquinas or its purity compared to Parson Weems's puritanism. I do propose, however, that Franklin's *Autobiography* fulfills much better than either of these the purpose of providing a new nation

and a newly emerging culture with a model that is appropriate to them, one that makes a bid to elevate them in a manner and to a degree that they are most likely to benefit from. The Franklin of the *Autobiography* is neither a classical hero nor a saint; but he does embody an ideal of a distinctly modern and democratic sort. In fact, those parts of the *Autobiography*'s teaching that are most decried as vulgar or materialistic stem from Franklin's deep egalitarianism.[9] He gives scope to the American impulse for material advancement (which it would not be possible or *fair* to suppress); but he gives that impulse a push in the direction of virtue and moderation, understood to be sure in a distinctive way. Equally important, he gives Americans an ideal of democratic public service that is quite high-minded, without being beyond the grasp of ordinary citizens. Franklin goes so far as to downplay in the *Autobiography* elements of his own history that are beyond the reach of ordinary human beings. At the same time, the *Autobiography* can inspire those who are drawn to the higher stature of Franklin himself, giving them important lessons on the place of a man of superior ability and accomplishment in a democratic milieu.

WEALTH AND VIRTUE

One of the leading themes of the *Autobiography* and one that Franklin takes evident pleasure in emphasizing is his own astonishing rise from poverty and obscurity to means and celebrity. One of the stated purposes of the work is to accentuate the contrast between his humble beginnings and his later success and to show the means by which it was accomplished (1964, 43). Franklin invites us to contemplate the contrast between his first, bedraggled appearance in Philadelphia and the prosperity and reputation he has since achieved there (p. 75). His first elevation to political office occasions similar reflections (p. 197). It is partly the prominence of this theme that makes the *Autobiography*'s emphasis on worldly success so unmistakable. Permissiveness toward the impulse to material advancement is indeed one of the central—as well as one of the obvious—lessons of the *Autobiography*. It is also one of the most misunderstood.

The passage on the "Art of Virtue" in part 2 (1964, 148–60), which is probably the most famous in the *Autobiography*, is typical. Franklin proposes a list of 13 virtues, along with a method (the Art of Virtue) designed to allow anyone to become proficient in them. As an incentive to follow this method, Franklin portrays the virtues as instrumental, as *useful*. He assures his readers that his own success and happiness in life are owing to their cultivation (p. 157; see also p. 119); he even offers a fundamentally utilitarian general theory of virtue: "Vicious Actions are not hurtful because they are forbidden, but forbidden because they are hurtful, the Nature of Man alone consider'd"; and it is "therefore every one's Interest to be virtuous" (p. 158; see also p. 54). One of the first episodes recorded in the *Autobiography* taught Franklin the lesson "that nothing was useful which was not honest" (p. 54). Franklin's Poor Richard proverbs (which do not form part of the *Autobiography* but are faithful to part of its message) are even more closely associated with this kind of utilitarianism; the compendium of these proverbs that Franklin composed in 1757 is still widely known as *The Way to Wealth*.[10]

This is the side of Franklin that sent D. H. Lawrence into paroxysms of rage and contempt. Lawrence accused Franklin of deforming generations of readers with a stifling moral vision that has no place for true benevolence or nonmercenary virtue, and of turning man into a mechanism by his "method" of self-perfection (Lawrence 1964, chap. 4). We should not, however, let Lawrence's attack color our own reaction to Franklin's utilitarianism; nor should we mistake *The Way to Wealth* as exhaustive of Franklin's moral vision.[11] Franklin had rhetorical reasons for slanting the wisdom of Poor Richard a certain way, as he is careful to tell us in the *Autobiography*. The "Proverbial Sentences" of Poor Richard, he there says, were in general directed toward the humblest audience, the "common People, who bought scarce any other Books" (1964, 164). These proverbs were "chiefly such as inculcated Industry and Frugality, as the Means of procuring Wealth and thereby securing Virtue, it being more difficult for a Man in want to act always honestly, as (to use here one of those Proverbs) *it is hard for an empty Sack to stand upright*" (ibid.).

Franklin is never shy about using money as an incentive to virtue, by insisting that virtue is the surest path to wealth. Precepts of this type are almost as common in the *Autobiography* as in Poor Richard, and it is these that have caused some to paint Franklin as an unregenerate materialist. In fact, though, they reflect only a recognition on Franklin's part of both the inevitable desire for wealth in people who have little and, just as importantly, the poor prospects for virtue in people who have nothing. It is a simple fact that a certain amount of prosperity is a necessary precondition of virtue in a democratic society like America. And Franklin recognizes that material advancement will inevitably preoccupy most of his audience.[12] Tocqueville wrote some fifty years later that effective moral teaching in the new, democratic age would have to reconcile itself, to some degree, to the material and self-centered preoccupations of the majority;[13] and Franklin is not blind to this necessity. He is indeed guilty (as his critics claim) of refusing to regard this necessity as a moral failing. Moral education as envisaged by Franklin never attempts to persuade anyone that the ambition for material advancement should be sacrificed on the alter of virtue. In the American context, (as Tocqueville also implied), such a teaching would be both elitist and ineffectual. Franklin seeks, rather, to begin by directing material ambition to virtue as its means and then as its end, as well. The ultimate purpose of Poor Richard's exhortations, as Franklin says, was

not to produce a full sack but one that will stand upright (see Lerner 1987, 52; Stourzh 1954, 15–16).

The notion that virtue is in one's interest in a narrow, economic sense represents only the first level of the *Autobiography*'s moral teaching. As the work's reference to the wisdom of poor Richard implies, the *Autobiography* seeks to address a wider audience—and deliver a morally less elementary message—than Poor Richard's. This is seen clearly enough in the way the work develops its most materialistic themes. Frugality and industry, key virtues in both Poor Richard and the *Autobiography*, are fairly narrowly connected to money-making; and Franklin recommends them highly as such. But he also is careful to subordinate them to both higher goods and other virtues that economic well-being paves the way to. He defines frugality as making no expense "but to do good to others or yourself" (1964, 149), and he justifies his inclusion of it and industry in his list of virtues by noting that the wealth they accumulate makes possible greater sincerity and justice (p. 151). Virtue may be "the means of procuring wealth" (as the *Autobiography*'s synopsis of Poor Richard has it), but wealth serves the purpose of "securing Virtue" in turn (p. 159). In his own case, Franklin attributes to frugality and industry the "Acquisition of his Fortune, with all that Knowledge which enabled him to be an useful Citizen, and obtain'd for him some Degree of Reputation among the Learned" (p. 157).

If Franklin makes the virtues (especially frugality and industry) instrumental to the accumulation of wealth, the latter is firmly subordinated to the virtues in turn and to more noble pursuits, including, in Franklin's own case, philosophy or science. The *Autobiography*'s elementary and, we might say, propaedeutic, utilitarianism should not conceal the fact that the work as a whole is actually directed strongly away from the concern for wealth as an end in itself. The example of Franklin's own business career is most eloquent. After prospering as a printer with the aid of his frugality and industry, Franklin retired at the age of 42 with a "sufficient tho' moderate Fortune" to allow him to pursue "Philosophical Studies and Amusements" (1964, 196; see also 119, 125). Like almost every American, Franklin had to concern himself with wealth first; but after his needs were comfortably met (Franklin actually became rather wealthy), he retired from commerce for higher pursuits. Franklin's description of these higher pursuits bears more than a passing resemblance to the classical vision of the life of noble leisure, especially when we add to it the public service that was immediately pressed upon him, which he accepted.

The *Autobiography* first caters to the reader's presumptive (and presumptively legitimate) concern with wealth, then directs it toward a much fuller vision of human happiness and the well-lived life. Public service, a reasonable or tolerant skepticism, and benevolent sociability are the elements of this vision that we shall concern ourselves with. For the present, we should notice that while the *Autobiography* shows us a number of ways in which Franklin learned or changed his perspective over the course of his life, it never shows us a time when his concern was purely commercial or monetary. This is true to fact. In one of his youthful "Busy-Body" essays for example, Franklin praises the virtue of the ancient statesman Cato, then asks, "Who would not rather chuse, if it were in his Choice, to merit the above Character, than be the richest, the most learned, or the most powerful Man in the Province without it?" ([1907] 1970, vol. 2, p. 108)[14] Cato is hardly a bourgeois ideal (the essay emphasizes his poverty); yet Franklin leaves very much open the possibility of acquiring riches, learning, and power while at the same time meriting a good character of reputation. It was always Franklin's notion that the four should be combined, which is possible if virtue (as he understands it) becomes the means, as well as the end, of the other ambitions. The moral outlook that this project embodies might be more earth-bound; but it is also more practicable, and safer, than that proposed by D. H. Lawrence in its place.[15]

THE VIRTUE OF REASONABLENESS

Once we get beyond the simple view of the Franklin of the *Autobiography* as a stern master of bourgeois discipline or a mercenary and try to explore more seriously his view of the place of virtue in a well-lived life, we are met with an entirely different set of puzzles. Despite the fact that the *Autobiography* tries to inspire its readers with a sincere devotion to virtue, Franklin's own devotion as portrayed at points in the work has to be described as casual at best. A gentle irony suffuses his account of the art of virtue, which began as a youthful "Project of achieving moral perfection" (see Sayre 1963, 522). "I knew, or thought I knew, what was right and wrong" (1964, 148), Franklin says at the outset of this account; but even with this unguarded presumption the project was not a complete success. Franklin confesses that by the end, he gave up trying to perfect the one virtue he found most difficult, namely, order. It was not worth the effort, given his natural disinclination to it (pp. 155–56). Similarly, perfect humility has escaped him (p. 160). Added to this is the fact that the virtues themselves (as Franklin identifies them) are hardly models of moral strictness. Some of them, like silence, order, and cleanliness—not to mention industry and frugality—would scarcely have merited mention by most earlier moralists (see Pangle 1988, 297–98). These might be thought to belong to a Puritan tradition; but then, there is Franklin's definition of *chastity*: "Rarely [to] use Venery but for Health or Offspring; Never to Dulness, Weakness, or the Injury of your own or another's Peace or Reputation" (p. 150). We learn enough in the *Autobiography* to realize that each of the loopholes in this definition is deliberate; if Franklin had viewed the virtue more strictly, he might have been forced to give up on it, as he did on order. His youthful "Intrigues with low Women"

are by and large excused by him as products of necessity ("that hard-to-be-govern'd Passion of Youth," p. 128; see also p. 115). He does *not* include them among the "Errata" he occasionally atones for in the *Autobiography*.

This remarkable indulgence can be explained only partially by the fact that the work itself is addressed to a son who may have been a product of one of these intrigues. It can also be explained only partially by the fact that the art of virtue is designed to put a level of virtue within the grasp of every man and that like many of the sayings of Poor Richard, it thus represents the beginning, rather than the end, of a full human life (see Meyer 1987, 158). The fact is that Franklin's moral teaching as a whole is conspicuously more tolerant than that of many of his religious or philosophical forebears. In fact, it is one of the principal, if less explicit, purposes of the *Autobiography* to *combat* the kind of moralism that had been characteristic of earlier traditions, particularly those of Puritan America. The moral code Franklin wants to replace them with, however, is somewhat elusive.

One way to get at Franklin's own moral outlook is by asking what in his view justifies a new moral tolerance. In the *Autobiography*, this tolerance is grounded partially in a distinctive notion of *reasonableness*. Franklin's reasonableness is different from the rationalism typical of the eighteenth century. Eighteenth-century rationalism sought answers to scientific, moral, and religious questions by means of unassisted human reason; and Franklin does likewise. But governing the operation of his rationalism is a pragmatic and skeptical *bon sens*, which, among other things, acts as a brake on speculative reason itself. This "reasonableness" is prior to morality and even to a religion, in the sense that it governs and moderates them. It is the resolute enemy of intolerance and zealotry, even the zealotry of reason. It is this trait, as much as wealth *or* virtue, that accounts for Franklin's happiness and the pleasing character of his example. In the course of a rather turbulent life, Franklin remains the picture of equanimity, kept always on an even keel by his earthy reasonableness, an amiable trait that is infectious—and is meant to be.

According to the *Autobiography*, this trait was not native to Franklin. His early inclination was, rather, to dogmatism. Upon reading a tract at age 16 equating flesh eating with murder, he became a principled vegetarian (1964, 63). This lasted until he was sorely tempted by some cod during his first voyage to Philadelphia. "I balanc'd some time between Principle and Inclination," Franklin says, then reasoned that since the cod were seen to have smaller fish in their stomachs, he could eat them in turn (p. 87). He concludes, "So convenient a thing it is to be a *reasonable Creature*, since it enables one to find or make a Reason for every thing one has a mind to do" (p. 88). The same sentiment appears more elaborately in his account of the art of virtue, to justify Franklin's abandonment of the goal of achieving moral perfection: "For something that pretended to be Reason was every now and then suggesting to me, that such extream Nicety as I exacted of my self might be a kind of Foppery in Morals, which if it were known would make me ridiculous; that a perfect Character might be attended with the Inconvenience of being envied and hated; and that a benevolent Man should allow a few Faults in himself, to keep his Friends in Countenance" (p. 156). The obvious playfulness of these two passages does not undercut the principle of reason or reasonableness that they outline, a principle that Franklin clearly acted on in both cases.[16] In typically satiric and self-deprecating fashion, these passages point to a view of the moderating role of reason in moral deliberation that is central to the outlook of the *Autobiography*.

Some of the most revealing applications of this view are seen in Franklin's treatment of religion. Here, Franklin's early dogmatism was at its strongest. Born into a pious Boston household, from an early age he devoured writings of Plutarch, Defoe, and Mather, along with books of "polemic Divinity" that he found in his father's library (1964, 58). The mature Franklin regrets only the last: their argumentative style, he says, made him overly disputatious (pp. 58, 60). The arguments themselves, however, had quite a different effect, causing him to doubt revelation by age 15 (p. 113). Soon after, books written against Deism turned him into "a thorough Deist" (p. 114); but this only fed his taste for speculative religious and moral dispute. While working in London at age 19, he composed a pamphlet called *A Dissertation on Liberty and Necessity, Pleasure and Pain* ([1907] 1970, 57–71), which purported to prove that there is no free will, that all our actions are determined by pleasure and pain, and that evil does not exist.

Franklin's London employer thought this production abominable, and the mature Franklin concurs (1964, 96, 114; see also p. 74). In the *Autobiography*, Franklin mentions the work only to disown it and to make a point about what he learned from it. He says he abandoned its doctrine when he discovered that it "perverted" some of the friends he imparted it to, who "afterwards wrong'd me greatly without the least Compunction" (p. 114; see also pp. 89–90). He concluded that "this Doctrine tho' it might be true, was not very useful" and at the same time "grew convinc'd that *Truth, Sincerity and Integrity* in Dealings between Man and Man"—qualities undermined by the doctrine—are necessary to the felicity of life (ibid.). Guided by this new insight, he gave up his speculative creed for something new.

The lesson we are supposed to draw from this episode is complex. Franklin's abandoning Deism on the grounds of disutility "tho' it might be true" is not as misologist as it sounds; for he immediately adds that he suspected some error had crept into his reasoning, making it neither true *nor* useful. But Franklin claims that such errors and their effects are "common in metaphysical Reasonings" (1964, 114), which constitutes an indictment of at least a certain

type or application of reason. In a letter to Benjamin Vaughan of November 9, 1779, Franklin confided that the *Dissertation*, together with a later essay maintaining more or less the opposite thesis, caused him to abandon this type of speculation altogether: "The great uncertainty I found in metaphysical reasonings disgusted me, and I quitted that kind of reading and study for others more satisfactory" [1907] 1970, vol. 6, pp. 410–13, esp. 412). The abandonment of speculative or metaphysical reason, however, is not the abandonment of reason per se. As the *Autobiography* and his entire oeuvre make clear, Franklin remains a dedicated rationalist. But his rationalism is informed by a strong sense of the limits of pure speculation.

Franklin's mature attitude is captured in his well-known address at the conclusion of the Federal Convention of 1787 in Philadelphia: "I confess, that I do not entirely approve of this Constitution at present; but, Sir, I am not sure I shall never approve it; for, having lived long, I have experienced many instances of being obliged, by better information or fuller consideration, to change my opinions even on important subjects, which I once thought right, but found to be otherwise. It is therefore that, the older I grow, the more apt I am to doubt my own judgment of others." ([1907] 1970, vol. 9, pp. 607–9).[17] The Convention had failed to adopt any of his major proposals (Becker 1931, 595);[18] but by the end, Franklin held that the practical matter of administering the government well was more important than the grand theories of government bandied about the convention ([1907] 1970, vol. 9, pp. 608–9).[19] This does not mean that he despairs of rational guidance in matters of government, only that such guidance will be halting and must constantly be tested against experience.

One of the key lessons of the *Autobiography* concerns the fallibility of human reason when pushed into the realm of pure speculation; but Franklin does not push this too far, either. Readers of the work gain a sense of the incompleteness of our knowledge but in a way that leads to moderation, rather than debilitating skepticism. What Franklin calls *Reasonableness* serves in the *Autobiography* to block the implementation of speculative conclusions in philosophy or theology; but it does so in the name of Franklin's own more earthy and commonsensical understanding of the proper way of life. It is in this spirit that the work launches a gentle but pervasive and insistent assault on religious dogmatism. The *Autobiography* is unquestionably at its most satirical in portraying dogmatic sectarians. Most unforgettable is Samuel Keimer, Franklin's first employer in Philadelphia. Keimer, a man of unconventional (not to say ludicrous) religious beliefs, was so impressed with young Ben's abilities in debate that he proposed to establish a new religious sect, with himself in the place of the prophet and Franklin as the confuter of opponents (1964, 88). Franklin agreed, on condition of making vegetarianism one of the tenets of the sect. For Keimer "was usually a great Glutton, and I promis'd my self some Diversion in half-starving him" (pp. 88–89). This diversion lasted three months, until Keimer, lusting after the "Flesh Pots of Egypt," gave in and devoured an entire roast pig (p. 89).

The humorous juxtaposition of Keimer's religious dogmatism and vegetarianism is an appropriate one, inasmuch as both are associated in the *Autobiography* with a failure of reasonableness in Franklin's sense. There is more impatience than humor, meanwhile, in Franklin's presentation of the Quakers' doctrinaire pacifism. Franklin illustrates how it forced the Quakers into hypocritical subterfuges and disingenuous evasions when faced with the necessity of providing for the common defense (1964, 186–90). As he dryly comments in a parallel context, "Common Sense aided by present Danger, will sometimes be too strong for whimsicall Opinions" (p. 232). The actions of the Quakers, as opposed to their doctrine, testify to the greater power, as well as sagacity, of common sense and practical necessity. At other points in the *Autobiography*, Franklin's response to strong religious devotion is simply to bury it in silence. He conspicuously refuses to be moved by the piety of a devout Catholic woman he encounters in London (p. 103; see Lerner 1987, 57), by the renowned preacher George Whitefield when he visits Philadelphia, or by the whole religious revival of the Great Awakening (pp. 175–80). Readers of the *Autobiography* subtly imbibe a cool and reasonable attitude toward religious matters from Franklin himself.

This does not mean that Franklin is pushing his readers to irreligion, however; for another of the key lessons of the *Autobiography* is that religion can and should be reasonable. Franklin pointedly approves of the Dunkers, a sect that refused to publish its doctrines, on the grounds that they might later have to be changed (1964, 190–91). "This Modesty in a Sect is perhaps a singular Instance in the History of Mankind," he marvels, "every other Sect supposing itself in Possession of all Truth" (p. 191). The modest admission of fallibility, together with the Dunkers' openness to progress, brings them closest to what Franklin seeks in religion. He assures us that he himself never doubted the existence of God (p. 146)[20] and recommends a creed that could be classified as a species of Deism, with a providential God and a strong emphasis on good works. He lists its tenets as follows:

> That there is one God who made all things.
> That he governs the World by his Providence.
> That he ought to be worshipped by Adoration, Prayer and Thanksgiving.
> But that the most acceptable Service of God is doing Good to Man.
> That the soul is immortal.
> And that God will certainly reward Virtue and punish Vice either here or hereafter. (p. 162)

Franklin takes the attitude that any principles beyond these are purely speculative and refuses to get embroiled in what he necessarily regards as fruitless controversy. This does not lead him to intolerance; rather, he considers his list to contain the "Essentials

of every known Religion." He insists he respects every religious belief he has encountered in America (except Keimer's?) and has indeed supported all that came to him for assistance (pp. 146–147).

Franklin's religious tolerance shares some of the characteristic paradoxes of American toleration. His own generic creed paves the way to maximum toleration; but it can have this effect only to the extent that others are brought, like him, to see all doctrines not in it as "nonessential." According to the creed, the divinity of Christ is nonessential,[21] as are myriad doctrines less fundamental but more controversial in the American context. Moreover, Franklin's toleration is qualified by the social thrust of his own religious concern. He finds that individual sects are worthy of more or less respect, depending upon how well they support morality and social concord (1964, 146). Some doctrines might obviously detract from these, but Franklin has little patience even with those that do not contribute adequately to them. He tells us that when he realized that the minister of his own congregation desired "rather to make us Presbyterians than good Citizens," in disgust he stopped attending his services (p. 147).

What Franklin says about his experience with Deism seems to contain the key to his attitude. We recall that that experience taught him the overriding importance of "*Truth, Sincerity and Integrity* in Dealings between Man and Man." This ideal is fully visible in his own creed, and it is by this standard that he now judges at least the extraneous or supernumerary dogmas of all sects. As a result, Franklin's approach to religion is primarily social and moral—indeed, this-worldly rather than otherwordly. It is "reasonable" in the sense of taking its bearing by experience, by what is visible; and it brackets, as much as possible, all purely theological questions. In effect, he subjects religion to a test of social utility. Whether Franklin is an unalloyed utilitarian in religion is a subtle question, depending upon whether even his core religious principles are derived simply from his conviction that they are useful to society.

This question cannot be answered with certainty here—perhaps not anywhere. It does, however, allow us to understand something more about the important question of Franklin's utilitarianism. When explaining his attitude toward biblical revelation, for example, Franklin has the following to say: "I entertain'd an Opinion, that tho' certain Actions might not be bad *because* they were forbidden by it, or good *because* it commanded them; yet probably those Actions might be forbidden *because* they were bad for us, or commanded *because* they were beneficial to us, in their own Natures, all the Circumstances of things considered" (1964, 115). Thus, the Gospels' moral strictures are considered valid not as revealed truth but as principles of utility. And although Franklin specifically denies that his own religious principles are derived from revelation (pp. 114–15), he does apply the same kind of calculus to morals in general. Speaking of the treatise he intended to write on the art of virtue, he says, "It was my Design to explain and enforce this Doctrine, that vicious Actions are not hurtful because they are forbidden, but forbidden because they are hurtful, the Nature of Man alone consider'd: That it was therefore every one's Interest to be virtuous, who wish'd to be happy even in this World" (p. 158). These are the classic statements of Franklin's moral utilitarianism. Since they reduce morality and religion quite clearly to self-interest, they make Franklin appear to some to be nothing but a vulgar mercenary. But Franklin's argument harbors some subtleties that need to be noticed. Utilitarianism can take many forms; and the elevation or debasement of each depends upon the goods, or the vision of the human good, that it rests upon or is directed toward. Things can be judged "useful" or the reverse only with reference to these goods, considered as goals. As the second of these passages points out, the real issue is the nature of man and what truly conduces to his happiness. In the *Autobiography*, this passage follows a general account of the utility of the virtues to Franklin's own happiness, where he makes clear that for him, happiness includes prosperity and reputation, as well as learning and public service (p. 157). In the first passage, the proviso that moral actions are beneficial to us "all the Circumstances of things considered" seems also to allude to a rich understanding of the human good that underlies utility for Franklin. This passage is part of Franklin's account of his turn away from Deism and his adoption of the view that "*Truth, Sincerity and Integrity* in Dealings between Man and Man" are necessary to the felicity of life (p. 114). It is with reference to this ideal that religion, as well as virtue, are recommended as "useful" by Franklin in the *Autobiography*. In addition to its richness, his ideal fully connects the individual and the social good. But it is only adumbrated in these two passages; its full articulation is seen only in the portrait of Franklin's life as a whole in the *Autobiography*.

Utilitarianism, as Franklin envisions it, is anything but mercenary or impoverished, because the goal or purpose that drives it is generous and humane. In fact, Franklin might have found inspiration for it in the Socrates of Xenophon's *Memorabilia*, which he encountered and admired at an early age, and which also makes use of utilitarian logic (Franklin 1964, 64, see also Xenophon *Memorabilia* 3.8, 4.1.1, 4.2.24–36 [1968]). Franklin might have found the inspiration for his reasonableness there, as well; for we may now say that this reasonableness represents a combination of metaphysical agnosticism with strong convictions about certain of the goods of life. The convictions are a product of combined observation and reasoning, and they serve to restrain metaphysical speculation in its excesses. It has been said that Franklin, in the thoroughgoing practicality of his approach to life, sacrificed reason to experience (Stourzh 1953, 1096; idem 1954, 9; see also Seavey 1988, 4). It is more appropriate to say that he concluded, in the face of the limits of speculation, that reason must take a

vision of the human good anchored more securely in experience as its first and guiding principle. For this reason Franklin might be considered among what William James called the forerunners or "preluders" of American pragmatism (James 1981, 27). James also identified Socrates as one of these preluders (ibid.). But Franklin would not endorse the development of a new metaphysics out of his approach. More importantly, he clearly regards his convictions or principles to be objective, not radically bound to his own experience. In this respect, he remains closer to the Socratic approach than to that of Pragmatism.

The indulgent or forgiving character of Franklin's moral outlook, at least when measured against those of some of his critics, thus has two sources. The first is his utilitarianism. Every utilitarian approach to morals refuses to treat virtue as an end in itself, and Franklin quite frankly says that virtue and at least part of religion are normative only because they contribute to individual happiness and the social good. Franklin is fully aware of the character of his argument and how it diverges from other perspectives—pious perspectives, in particular. Yet much of the criticism he has faced—that which makes him a mercenary simply because he regards virtue as a means to something else—is rooted in precisely the "deontological" approach to morals he rejects (see Pangle 1988, 18). For Franklin, there is nothing more natural than for there to be a happy coincidence of virtue and utility and nothing more ill conceived than to propose a radical separation of the two.

In addition, Franklin's distinctive vision of how extramoral human goals and appetites fit into a full and happy life makes his utilitarianism more indulgent than others'. For him, wealth, power, and reputation, as well as virtue, are legitimate parts of such a life. Here, the crucial contribution comes from Franklin's own experience of the felicity of life, which has persuaded him that much more of human nature can safely be indulged than earlier moralists had thought. His good-humored concessions to his own lack of orderliness and pride, his view of chastity, and even his rejection of vegetarianism reflect Franklin's assessment of how far moral strictures may be relaxed. This is reflected, as well, in the half-humorous proposal he once penned to devise scientific means of scenting intestinal gas so that flatulence might become socially acceptable ([1907] 1970 952–55). Franklin is at all times in favor of liberating innocuous natural impulses. The suppression of human appetites in the name of transcendent goals unrelated to human happiness simply appears nonsensical—unreasonable—to him. Reasonableness for Franklin consists precisely in affirming appetite where appetite contributes to human happiness and where nothing more than speculative grounds can be given for disturbing it. It is a kind of moderation directed against the excesses of moralism itself.

Franklin's vision of the good life, which lies behind all of this, is the greatest unifying theme of the *Autobiography*. It is a vision that serves not only individual happiness, but the good of society as a whole. But just as Franklin has altered our understanding of virtue to fit his vision of the good life, so he has thought his way to a new and distinctive view of the best social order and thus of the proper nature of sociability. A vision of society that combines in typical fashion benevolence and utility, together with a rather complicated egalitarianism, might be called the capstone of the *Autobiography* as a whole.

THE ART OF SOCIABILITY

If there is one thing that every reader of the *Autobiography* comes to admire and to envy in Franklin, it is his remarkable sociability. The facility with which he makes friends is a constant source of delight to his readers and an evident source of both delight and utility to himself. It is part of Franklin's uniqueness to have been not only universally renowned but universally beloved in his own day.[22] Still more remarkable was his amazing success in mastering all of the milieux he moved in—and these were as varied almost as the world had to offer—without being the less loved for it. From the Boston of his earliest youth, to the more freewheeling business and intellectual world of Philadelphia, to imperial London, and finally to the royal court of France, he displayed an unerring ability to influence events by the friendly force of his character. In all the machinations of his public career, he seems to have made only one serious misreading of men or events.[23] The *Autobiography*, which covers none of Franklin's career as an international diplomatist, devotes considerable attention to his facility with people and aims to communicate something of that facility to us. In typical fashion, Franklin attributes his social success to a number of *techniques* and distills them into what amounts to an art of sociability that may be practiced by anyone. Also in typical fashion, this art looks to both pleasure and utility. The problems it is designed to overcome are partly those encountered by ordinary citizens and partly those confronted by men of superior achievement or ability who aspire to exert leadership in the novel circumstances of egalitarian society.

In the *Autobiography* Franklin portrays his sociability, like his reasonableness, as an acquisition or contrivance, rather than as something native to him. In some ways, the two are sides of a coin. His early encounter with books of "polemic Divinity" led him not only to the intellectual vice of dogmatism but to the social vice of contentiousness—a defect rarely found in men of good sense, Franklin says, "except Lawyers, University Men, and Men of all Sorts that have been bred at Edinborough" (1964, 60). With this turn of mind, all he seems to have gained from his first encounter with Xenophon's Socrates was proficiency in the method of confuting people by innocent-seeming questions (pp. 64–65). He combined mastery of this method with his youthful religious

skepticism—to his own delight but the consternation (and alarm) of others. Franklin says that his eristic activity eventually made him notorious enough to be "pointed at with Horror by good People" in Boston (p. 71). He left Boston partly in order to escape this bad reputation and gradually gave up his pugnacious method of dispute, retaining only the Socratic habit of expressing himself less positively in argument, replacing words such as *certainly* or *undoubtedly* with expressions like *I conceive, I apprehend,* or *I imagine* (p. 65).

After his change of manner, Franklin says, his conversations went on much more pleasantly. But this was more than a modification of speaking style. It became also a change in Franklin's whole approach to social relations, away from confrontation and toward accommodation. This new sociability was motivated by the hope of doing good, as well as receiving pleasure. Championing his less assertive mode of speech, Franklin says:

> This Habit I Believe has been of great Advantage to me, when I have had occasion to inculcate my Opinions and persuade Men into Measures that I have been from time to time engag'd in promoting. And as the chief Ends of Conversation are to *inform,* or to be *informed,* to *please* or to *persuade,* I wish wellmeaning sensible Men would not lessen their Power of doing Good by a Positive assuming Manner that seldom fails to disgust, tends to create Opposition, and to defeat every one of those Purposes for which Speech was given to us. (1964, 65)

This remarkable passage not only informs us of the true purposes of speech but gives us implicitly a vision of the proper social order. That order is closely bound up with speech and its purposes. Speech brings pleasure, conveys information, and makes persuasion possible. Potentially a great power of social good, it is too often abused and deflected from its proper use. Franklin's art of sociability therefore rests heavily on a self-conscious reform of men's use of their faculty of speech.

The principal obstacle to proper sociability is pride. Arrogant habits of speech block all its potential for good, disrupting society, as well as the genuine pleasure of conversation. Franklin's own experience has taught him the tenacity of this problem. He admits that when he was developing his art of virtue, it took the prodding of a Quaker friend to get him to add the virtue of humility to his list. He made it his thirteenth virtue and in this context associates it primarily with modesty in speech (1964, 159). He also confesses to the difficulty of acquiring it: humility forces him to forgo the "pleasure" of immediately contradicting others' errors, a discipline that does "some violence to natural Inclination" (ibid.).

Due to this difficulty, Franklin informs us, he has not truly succeeded in acquiring humility, only its appearance. But for social purposes (what concerns Franklin primarily here) this has been perfectly adequate. Under the virtue of humility, Franklin had written "Imitate Jesus and Socrates" (1964, 150); but it seems that in this respect his *imitatio Christi* is confined to externals. One reason why Franklin accepts this with such good humor is that neither his view of individual virtue nor of social relations is one from which all pride or even vanity has been purged. That would require a sterner discipline than Franklin is willing to impose, especially since he finds redeeming elements in human pride, beginning with his own. At the onset of the *Autobiography* he admits ("since my Denial of it will be believ'd by no body") that the work is motivated partly by vanity. But vanity, he says, is something he has learned to indulge, since it "is often productive of Good to the Possessor and to others that are within his Sphere of Action," as well as being one of the "Comforts of Life" (p. 144). As always, the combination of private pleasure and social utility is an irresistable one for Franklin. The problems with pride arise largely because of misconceptions on the part of its possessors. As Franklin says in the early essay in which he lionizes Cato, the major problem with man's "strong natural Desire of being valu'd and esteem'd" is simply that too few know the best way of gratifying it, namely, through virtue ([1907], 1970, vol. 2, p. 108).

The social order as Franklin conceives of it is based more on the proper education of vanity and its mutual accommodation by members of society than on its suppression. Showing vanity how useful virtue is to its gratification (one of the themes of the *Autobiography* as a whole) is a part of its education. Franklin's exhortations to modest speech for the sake of both pleasure and social concord is another part of its education. But since Franklin does not imagine that harmful pride can be so easily brooked, further accommodation is required. Thus, among other things, he admonishes us all, under the heading of "Moderation," "Forbear resenting Injuries so much as you think they deserve" (1964, 150). Pride overvalues slights and overvalues its own desert; and those who are concerned about social accommodation will therefore control their own pride, while giving others' wider berth. This kind of moderation is perfectly captured in a subtle emendation Franklin proposes in the *Autobiography* to a famous poetic couplet. Instead of writing "Immodest Words admit of no Defence; / For Want of Modesty is Want of Sense," (p. 66).[24] Franklin suggests the poet should have written, "Immodest Words admit *but this* Defence, / That Want of Modesty is Want of Sense." Franklin takes the view that immodesty is caused by ignorance and in a spirit of Socratic generosity maintains that ignorance provides some excuse for vice.

Franklin's view of sociability, resting on modesty of speech, cultivation of at least the outward forms of humility, and indulgence of mistaken pride in others, is appropriate, especially in the new kind of society Franklin was attempting to shape. In modern democratic or egalitarian society, as much pride must be indulged in each as civility will bear. Special forbearance will be practiced by those who take the lead in sociability, as Franklin hopes each of his readers will

do. But Franklin is the last to think this will be the way with all and is fully aware that those who take such a lead will likely be a minority. The problem of sociability thus opens something of a breech in the smoothly democratic exterior of the *Autobiography*—a breech that becomes wider and more significant, the closer one looks; for out of the art of sociability Franklin develops an art of leadership that is egalitarian in its benevolence toward the public at large but is more exclusive in its audience. In his praise of modest speech, Franklin had put special emphasis on the "Power of doing Good" that modesty would place in the hands of "wellmeaning sensible Men" (1964, 65). These men are like Franklin in his capacity as a projector and a leader—as something other than Everyman. Franklin notes that the humble manner of speech and demeanor he developed was vital not only in smoothing his social relations but in advancing the many projects he undertook during his life and in taking the lead in practical affairs in general (p. 160). It not only brought him good will but allowed him to draw men along by persuasion, without bruising their pride or vanity.

If sociability in a democratic context requires special attention to the universalized claims of pride, the exercise of leadership compounds the difficulties. Leadership, which always requires the careful management of human vanity, is given special delicacy in the democratic milieu as Franklin understands it. Every man demands—and merits—respect; and the leader cannot proceed peremptorily. But Franklin does not take the democratic perspective so exclusively as to think this is the only problem, nor does he think democratic leadership should concern itself only with accommodating the pride of followers. There is also the legitimate pride of the leader. Franklin's own pride is concealed but not conquered; and it stands to reason that the "future great men," for whose benefit, among others, Benjamin Vaughan exhorted Franklin to publish his *Autobiography* (1964, 135), will be at least as proud. In order to put this problem in its proper perspective we must first realize that in predemocratic Western societies (indeed, in the London and Paris of Franklin's experience) such pride was an unquestioned virtue. Under the name of honor, it forms, in fact, the copestone of every aristocratic social order. Franklin is willing, in self-effacing democratic fashion, to call the leader's pride "vanity" and accommodate it to the new social circumstances for which he is writing. But he is not willing to sacrifice it on the alter of egalitarianism. If vanity in its ordinary forms, properly directed, can be productive of much good, the pride that will motivate future Franklins is even more to be cherished. The difficulty is to satisfy both, to give scope to the leader's pride without offending that of his followers.

This cannot be done without a certain amount of subterfuge, and that is one of the things Franklin's art of sociable democratic leadership provides. When seeking supporters for a early project of his, Franklin discovered that resistance was created by the supposition that the project's success would raise his reputation even "the smallest degree" above that of his followers (1964, 143). Here, democratic pride is seen in its ugly form as resentment. Franklin did not, and does not, believe this resentment can simply be done away with, so he developed a ruse for concealing his leadership to get around it. By presenting schemes as the idea of "a number of friends" or of "some *publick-spirited Gentlemen*" who seek wider support, he advises us, opposition rooted in envy or distrust of ambition can be forestalled. He recommends this device to any who would lead effectively (pp. 143, 193). The vanity of followers is assuaged, and the leader's projects go forward more smoothly; but the real beauty of this device, Franklin assures us, is that in the end there is no real sacrifice of the leader's pride: "If it remains a while uncertain to whom the Merit belongs, some one more vain than yourself will be encourag'd to claim it, and then even envy will be dispos'd to do you Justice, by plucking those assum'd Feathers, and restoring them to their right Owner" (p. 143). If Franklin is to be thought of as the apostle of postponed gratification, this might be the best example of it in the *Autobiography*. Precisely by adopting a low profile in deference to the pride of others—by being humble in appearance—the leader garners the praise he wants and deserves. Franklin believes this and devices like it to be necessary to effective leadership in the circumstances he—and we—confront.

The *Autobiography* gives us enough examples of this kind of self-concealment to make it a theme of the work. His first published essays were submitted anonymously to his brother's newspaper, to gain them a fair hearing (1964, 67—68). His long-standing intellectual club, the Junto, was a semisecret organization. Along with ancillary clubs established on the same model, Franklin says the Junto was useful as means of preparing the public mind for projects he later wished to take in hand (pp. 170–71, 173–74). He once formed a plan for a larger group to be called the Society of the Free and Easy, which was to begin as a secret party whose members had trained themselves in his art of virtue and subscribed to his religious creed and who would, in unison, have an impact on public affairs (pp. 161–63). Franklin claims this group was never formed; but he still believes it useful and possible, "as I have always thought that one man of tolerable Abilities may work great Changes, and accomplish great Affairs among Mankind, if he first forms a good Plan" and prosecutes it assiduously to its conclusion (p. 163). This particular plan began with the formation of a secret elite.

The culmination of Franklin's instruction in sociability is an exhortation to public service addressed to everyone; but the most productive kind of public service is carried out by a man who is willing to take the lead while operating within the constraints of the new social setting. Franklin brings a thoroughly democratic sensibility to public service, but he does not blind himself to the fact that the new conditions are

constraints. Franklin's leader operates often by stealth, using devices that we would almost call manipulative but for the spirit in which Franklin employs them. The concealments always fall within Franklin's definition of *sincerity*, "[to] Use no hurtful Deceit" (1964, 150). The deceits he recommends always have the purpose of removing obstacles to projects of public benefit. Yet here, unquestionably, the distance between Franklin and his followers—or between Franklin and most of his audience in the *Autobiography*—is greatest. Despite a spirit of infinite benevolence, Franklin's innocently deceitful way of making his way as a leader through the social milieu bespeaks a certain degree of detachment and even of irony toward his fellows. Paradoxically, in the *Autobiography*, as in many of his writings, Franklin holds himself partly in reserve in order to serve the public—and himself—better.

In his *Autobiography*, as in his life, Franklin cultivates a peculiar mixture of intimacy and distance, transparency and concealment. But Franklin's irony, if Socratic in spirit, is not Socratic in extent. Franklin is a democrat and cannot, for all his irony, be portrayed as an esoteric elitist in the classical sense. In most respects he makes himself and his views completely known to his readers (see Morgan 1991). In the *Autobiography* Franklin makes a deliberate effort to portray himself as a model American, imitable by all of his fellow countrymen. His lessons on effective leadership are a partial exception to this. But of all the deceptions or concealments we might find in the *Autobiography*, the most thoroughgoing is Franklin's downplaying of the aspects of his life and activity that put him too far above the average man. This is especially true in the field of intellectual achievement. Though Franklin makes much of his lifelong love of books and of writing, the *Autobiography* uses these things by and large to convey lessons about diligence and the advantages intellectual cultivation might bring to anyone, not to show how Franklin became an internationally renowned thinker and essayist. Similarly, though Franklin mentions his work with electricity, work for which he was already famous when he began the *Autobiography* and that was by and large complete by time of the latest events narrated in the *Autobiography*, it, too, figures in the work mostly as a demonstration of the kinds of virtues the work's wider audience may aspire to. It is one of the noncommercial pursuits that Franklin retires from business for (1964, 196) and provides an instance of his refusing to embroil himself in unnecessary disputes (pp. 243–44). When his scientific inventiveness leads to the development of his "Pennsylvania Fireplace," he emphasizes primarily that he declined the patent on it and on other inventions, out of generosity to mankind (p. 192). In the end, the *Autobiography* gives us a hero who is more ordinary than Franklin himself actually was.

The *Autobiography* breaks off in the year 1757, around the time when Franklin was becoming a national and international statesman, that is, the time when his public career was beginning to take him decisively beyond the aspirations of the ordinary citizen. Franklin did not intend to stop the work there,[25] but this ending point suits very well the didactic purpose of the work as we have it. The Franklin thus bequeathed to posterity is not quite the historical Franklin (as all his biographers know). But the *Autobiography* is not biography in the rigorous sense. It is, rather, an effort to construct a new model American. Franklin was one of the few to embrace modern egalitarian society in its infancy and to try to shape it. And he wishes us to become, as much as possible, like he has made himself—combining the maximum of both virtue and prosperity and contributing to a society graced with both benevolence and charm.

Notes

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1. For general accounts of the history of the work's reputation, see Buxbaum 1987; Miles 1957; Sayre 1963, intro.

2. For the view that Franklin is a Puritan, see Griswold 1965; see also Sanford 1965.

3. See Adams 1955; see also Miles 1957, 118–19, and Miroff 1986, esp. 128. Adams was one of very few to regard even Washington's reputation as unjustified.

4. Miles (1957) speaks of the tradition of Franklin-bashing carried on by what he calls the "Neanderthal wing" of the Federalist party into the nineteenth century. See also Labaree's citations of Charles Francis Adams in Franklin 1964, 14–15.

5. Lawrence wrote two versions of his critique of Franklin, one in 1918 and one in 1923 (Lawrence 1987 and 1964, chap. 4, respectively). The first is more temperate, the second more widely known.

6. See, e.g., the collection *Benjamin Franklin on Education* (Franklin 1962).

7. See the letters Franklin had reprinted at the beginning of part 2 (1964, 133–40); see also the letter from Franklin to Vaughan after he had completed most or all of the *Autobiography* as we have it (Franklin [1907] 1970, vol. 9, pp. 675–77).

8. The four parts of the *Autobiography* were written at three different times in widely differing places (part 1 at Twyford estate, England, 1771; part 2 at Passy, near Paris, 1784; and parts 3 and 4 in Philadelphia at the very end of Franklin's life, 1788 and after). This has led some to find differences of tone, outlook, and intent among the different parts of the work. I believe variations of tone may be found among the parts, but not of outlook or intent.

9. Although Franklin did not commit himself to democracy as a *political* form until rather late (he was a monarchist, though of a radical Whiggish stamp, for most of his life), his egalitarianism was constant. By the time of his participation in the Federal Convention of 1787, he was more populist than most of the members of that body. See Franklin [1907] 1970, 590–604. See also Farrand 1937, vol. 1, pp. 48, 77–78, 81–85; ibid., vol. 2, p. 542; ibid., vol. 3, p. 297; Rossiter 1952; Stourzh 1953, 1107–14.

10. This was first printed as a preface to the *Poor Richard's Almanac* of 1758. It is also known as *Father Abraham's Speech*. See Franklin [1907] 1970, vol. 3, 407–18. Reprintings of this were immensely popular not only in America but in England and on the continent (see Franklin 1964, 164).

11. Most interpreters of Franklin agree that Lawrence took far too narrow a view of Franklin's teaching. See, e.g., Lerner 1988, chap. 1; Miles 1957, 142; Rossiter 1952, 281; Sayre 1963;

Schneider 1955; Seavey 1988. For another critique of Franklin, see Angoff 1955; see also Lemay 1982.

12. Something of Franklin's understanding of his audience can be seen in a pamphlet he wrote in France in February 1784 entitled *Information to Those Who Would Remove to America* ([1907] 1970, 975–83). The pamphlet's message is that prosperity comes surely to those who work in America but that there is no prospect for idlers, placeholders, or courtiers.

13. Tocqueville speaks, for example, of the American doctrine of "self-interest rightly understood" (1969, vol. 2, pt. 2, chap. 8).

14. The essay was originally published in the *American Weekly Mercury* of February 18, 1729, when Franklin was 23. It is true that the image of Cato had been both appropriated and altered by the London literary culture that the young Franklin idolized; but the Cato of this essay is the Cato of Plutarch, not of Trenchard and Gordon. On the "Cato cult" in England and America at this period, see Bailyn 1967, chap. 2; MacDonald 1985, 190–95. On its nonclassical elements, see Pangle 1988, 30–33.

15. The vulgar Nietzscheanism embraced by Lawrence in his critique of Franklin is considerably more dangerous and irresponsible than the sober moderation of Franklin.

16. For yet another instance of this sentiment, see Franklin's letter to Mary Hewson of May 6, 1786, where he excuses his idle cardplaying with the reflection that his immortal soul has enough time lying before it: "So, being easily convinced, and, like other reasonable creatures, satisfied with a small reason, when it is in favour of doing what I have a mind to, I shuffle the cards again, and begin another game" ([1907] 1970, vol. 9, pp. 510–13). This kind of ironic appeal to reason was obviously a favorite with Franklin, but its irony masks a serious reflection.

17. The whole address is a meditation on this theme.

18. Franklin supported proposals for an executive council, an executive without pay, and a unicameral legislature. See Farrand 1937, vol. 1, pp. 48, 77–78, 81–85; ibid., vol. 2, p. 542; ibid., vol. 3, p. 297. He was, however, instrumental in fashioning the compromise on representation by states and by population (vol. 1, pp. 488–89; ibid., vol. 3, pp. 470–73).

19. Alexander Pope, one of Franklin's favorite authors, produced the famous couplet on this theme: "For forms of government let fools contest; / Whate'er is best administered is best" (*Essay on Man* 3.303–4). The succeeding couplet could serve as motto for Franklin's attitude toward religion: "For modes of faith let graceless zealots fight; / His can't be wrong whose life is in the right."

20. Franklin also says he never doubted either that God governs the world with his providence or that the best service to God is service to man. Yet his *Dissertation on Liberty and Necessity, Pleasure and Pain* at least comes very close to denying these things. When speaking about what the notoriously elusive and ironic Franklin actually believed in religious matters, we must be on our guard. Here, especially, we might be mindful of the loophole in his definition of the virtue of sincerity, "Use no hurtful Deceit" (1964, 150).

21. In a letter to Ezra Stiles of March 9, 1790, very near the end of his life, Franklin repeats his religious principles. In response to a query by Stiles, he allows that the "System of Morals" propounded by Jesus is "the best the World ever saw or is likely to see" but that his divinity "is a question I do not dogmatize upon, having never studied it, and think it needless to busy myself with it now, when I expect soon an Opportunity of knowing the Truth with less Trouble" ([1907] 1970, vol. 10, pp. 83–85).

22. Consider the following testimonial—from John Adams, no less: "[His fame] was more universal than that of Leibnitz or Newton, Frederick or Voltaire, and his character more beloved and esteemed than any of them" (quoted in Stourzh 1954, 2).

23. I am thinking of the affair of the Hutchinson letters, which is not covered by the *Autobiography*. Franklin evidently believed that the divulgence of these letters to selected men in Boston would cool passions there in 1772–73, but it actually had the opposite result. He got into serious trouble in London, where he was living as a colonial agent at the time. See Van Doren [1936] 1964, 444–78; Willcox 1987, 105–8. Franklin's course as a youth in Boston was not entirely smooth, which taught him some important lessons. His overall success in his public career, of course, did not preclude his making enemies—as was inevitable, especially in London during the years of revolutionary ferment.

24. Franklin falsely attributes this couplet to Alexander Pope (1964, 66n.). He also misquotes the second line, substituting *Modesty* for the *Decency* of the original.

25. Franklin was working on the *Autobiography* up to the time of his death, and an outline he prepared for it (1964, 267–72) extends chronologically much beyond the point where the work breaks off.

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INTERESTS AND ETHICS IN POLITICS

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I examine the place of self-interest in political life as given by a conception of politics that invokes ethics. This conception portrays each citizen as an individual with unique hopes and desires who is at the same time joined with others—part of, and continually giving shape to, a shared social and political life. It sees in political diversity and controversy not just conflicting interests but also competing claims about what "we"—unique individuals, linked to particular others through social roles and relationships, and together forming a single citizenry—ought to do or seek. Research that simply adopts a broad conception of utility or interest to admit nonselfish preferences or that employs typologies contrasting self-interested with non-self-interested motives will reveal neither the significance nor the limits of self-interest in this politics. Rather, we must explore how citizens' interests are both championed and challenged by the understandings of "good" and "right" to which our politics gives voice.

> Perhaps what characterizes political life is precisely the problem of continually *creating* unity, a public, in a context of diversity, rival claims, unequal power, and conflicting interests.
>
> —Hanna Pitkin, *Wittgenstein and Justice*

Mathematical and empirical scholars of political behavior have frequently subscribed to what James Coleman describes as the "central postulate of economic theory," conceiving of the political actor as *homo oeconomicus*, a "rational man attempting to pursue his selfish interests" (1986, 15). Yet empirical evidence for this self-interest assumption has often been weak, and skeptics of a self-interest-based politics abound (Bellah et al. 1986; Citrin and Green 1990; Cohen 1986; Dagger 1986; Dobel 1986; Elster 1986; Goodin 1990; Grofman and Feld 1988; Kingdon 1988; Mansbridge 1980, 1990a; Reich 1988; Sagoff 1986; Sears and Funk 1990; Sunstein 1990; Tyler, Rasinski, and Griffin 1986). Those rejecting a conception of political actors as self-serving egoists have reached out in two prominent directions.

The first approach retains an assumption that citizens seek to satisfy their preferences, or maximize their utility, or advance their interests, while redefining preferences, utility, or interests. Rational and social choice modelers have commonly rejected the stipulation that preferences reflect only self-regarding or selfish desires by placing no substantive constraints on utility whatsoever: utility can embrace anything that an individual might value (e.g., Arrow 1967; Becker 1976; Downs 1957; Hechter, Opp, and Wippler 1990). As Mancur Olson explains it, "To say that rational decision-making maximizes 'expected utility' says little more than that decision-makers take *all* of their objectives, material and intangible, selfish and altruistic, into account" (1987, 206; emphasis original).[1] Comparable strategies have arisen among scholars who consider the content of "interests": interests need not necessarily be self-regarding but might be other-regarding or principled, as well (e.g., Connolly 1983; Flathman 1966; Held 1970; Mansbridge 1980; Schlozman and Tierney 1986).[2]

The second approach preserves a narrow conception of what constitutes self-interest and then explores the significance of a number of motives each held as conceptually distinct from self-interest. Work in this genre has generated a number of motivational typologies, each contrasting self-regarding or selfish preferences with those reflecting nonselfish concerns: "varieties of unselfishness" (Jencks 1990), duty and love (Mansbridge 1990b), symbolic predispositions (Sears et al 1980), sympathy and commitment (Sen 1990), and justice (Tyler 1990), among others.

Both of these approaches replace a conception of citizens as purely self-regarding or selfish with a more generous view of the ends people might be seeking in politics, while providing different accounts of "self-interest." The first rejects a narrow (and objective) version of self-interest, while retaining the premise that people do seek whatever it is that they find to be in their interest. It conceives of political actors as egoists but not as egotistical. They pursue their interests, but those interests may not be entirely selfish. The second adopts a narrow (and objective) account of self-interest as self-regarding, seeking to understand its unique empirical significance vis-à-vis alternative bases of public opinion and political choices. By confining what it counts as self-interest, it carves out a space for a nonegoist politics.

I offer an alternative account of the place of self-interest in political life, one based upon a conception of politics that invokes ethics. In this politics, I will argue, citizens are not simply seeking to satisfy their personal preferences, be they self-regarding or not; but neither are they simply setting aside their personal preferences or self-regarding interests and adopting some non-self-interested aim. The citizen, instead, must attempt to resolve what Nagel identifies as the central problem of ethics: "how the lives, interests, and welfare of others make claims on us, and how these claims, of various forms, are to be reconciled with the aim of living our own lives"

(1986, 142). When judged against the perspective on citizenship that I will sketch here, simply adopting a broad conception of utility or interest to acknowledge that political preferences might not be purely self-regarding, or employing typologies that contrast self-interested with non-self-interested preferences, are each inadequate as a response to the failures of the economic approach. The first fails to recognize judgments of value that are articulated as public and not personal claims; the second imposes a dichotomy that is either false or incomplete. We limit our understanding of both interests and ethics in politics when we either subsume anything citizens can value under the rubric of personal preferences or focus on adjudicating between rival explanations defined by some conception of a self-interest/altruism divide. Instead, we need to bring conceptions of interests and ethics *together* as we seek to develop a more comprehensive account of public opinion and political choices.

PREFERENCE, JUDGMENT, AND JUSTIFICATION

Bringing ethical ideas to the study of American public opinion and political action means exploring a particular conception of politics and political actors. This politics is not simply an arena where citizen preferences are articulated and aggregated but one where public goals and policies are debated and political choices must be publicly justified.[3] When citizens assess public policies, events, and leaders or consider their own political choices, they are not merely trying to figure out what or who they like or what it is they want. They are also trying to figure out what or who is *good* or what is *right*. The objects of judgment are political, but these questions are ethical.[4] And the answers to ethical questions rest upon justifications.[5]

Ethical justifications may take many forms and may make any number of substantive claims. Consequentialist moral arguments, for example, rest the conclusion of right on a theory of the good: that which is right maximizes good. Utilitarian consequentialists apply a single metric when representing what is good in states of affairs, looking to maximize social utility.[6] Nonutilitarian consequentialists deny the practical intelligibility and moral significance of the reductive utility metric, recognizing multiple standards of value. But such arguments still rest the description "right" on the choice with the best consequences. From nonconsequentialist or deontological perspectives, in contrast, what is right is not what maximizes the good. Instead, some conception of nature, or persons, or rationality, or divine will might be cited as supporting claims about rights, duty, obligation, or justice. Quite apart from the consequences of an action (under any description of consequences), these premises serve to establish a judgment that the action (or practice or policy) is, at least prima facie, right or wrong.

Judgments expressed in the evaluative language of ethics as to what is "good" or "right" lay claim to what *we* should value or what *one* should value, resting upon interpersonal justifications that we can (and often do) disagree about.[7] If, for example, you suggest that busing is a good policy, it implies that you have reasons that sustain that evaluative conclusion, reasons that express your understanding of that policy and what it is that you see as *of value*. In contrast, subjective preferences, offered in the first person as a claim about what "I" value (want, like, desire), are intrinsically personal, implying no interpersonal standard for assessment and no basis for public discussion.[8] They simply express what appeals "to me." Different evaluations or opinions, taken subjectively, indicate only diversity among preferrers, not disagreement about what can justifiably be sanctioned as good or right.

Of course, we can give reasons for our likes and wants. We can explain our personal preferences, autobiographically. Or we can explain our preferences by identifying the features in them that we find valuable. I can say "I like George Bush more than Mario Cuomo," for example, and explain that preference by referring to Bush's position on abortion and defense spending, his partisanship, and his previous experience in office. I can insist that these are characteristics that I value, and no one need quarrel with me. But if I claim, further, that George Bush is *better* than Mario Cuomo as a choice for president, then I am making a judgment of value that goes further in implying that my reasons for liking Bush call upon interpersonal standards of value that others must acknowledge.

Scholars employing subjective conceptions of value often recognize an equality among opinions that derives from a conception of equality among persons. Social choice theories express this notion by giving equal weight to the subjectively conceived utility of each person they recognize. Opinions (as expressions of utility) can be criticized relative to each person by reference to procedural norms of rationality, but no social or interpersonal standard of value provides a basis for criticism. According to a subjective perspective, good opinions in politics are opinions that can pass a test of procedural rationality, that evidence some desirable bond to the person giving them expression. If value is conceived subjectively, what people need for political expression are the resources to know, in some thick sense, what they care about.

But an ethical perspective recognizes distinctions among opinions by virtue of social standards of justification, expecting more of public opinion. Opinions must be sustainable by reference to justifications that can be offered to others, expressing value judgments that find some social niche. They must provide a basis for challenging alternative contentions about what is good or right. The issue here is not one of knowing what you want—of having formed a preference in that sense—but of having what it takes to make an arguable case. To express themselves or act politically, individuals must not merely "know them-

selves" but have the resources to express themselves in ways that are compelling to others in society.

When reflecting upon his interviewing experiences, Converse commented that "very many respondents could not understand that a battery of pure opinion items had no objective 'right–wrong' scoring" (1974, 650). This phenomenon loses what oddity it may otherwise have once we conceive of opinion as that which others can and do judge, critique, and even condemn. When we understand opinions as claims requiring justification, claims that compete with alternative views in conveying that which is good or right, we cannot rely on a simple notion of equality among persons to reach a conclusion that one opinion is just as good or valid as any other. Opinions can and will be criticized on grounds that question their roots in facts and values, and citizens will feel discomfort when expressing opinions that they feel may be inadequately bolstered.

The differences between ethical judgments and personal preferences are tied to the kinds of reasons that are offered to sustain them and to how each kind of claim functions in public discourse. Ethical disagreements (disagreements about what is of value or whether some person or act or policy has value) reflect disagreements that can be probed by examining the nature of the justifications upon which they rest: factual premises, fundamental goals, understandings of the consequences that flow from action, and guiding principles of right.[9] Ethical judgments rest on interpersonal justifications and not just on unique personal explanations. They appeal to others and do not just describe the self.

SELF-INTEREST *AS* ETHICS

Justification is always at the root, if not at the forefront, of ethical claims. If we simply begin with that premise, then it is clear that there is no inherent contradiction between self-interested and ethical claims, no reason why self-interest could not be cited in attempting to justify some outcome or action as right. Indeed, if by virtue of serving one's self-interest a political choice could be sanctioned as justified, then there would be no incompatibility between the dictates of self-interest and ethics. Some version of self-interest—that it is "what I want" or "best advances my interest in X"—could be invoked as an ethical argument (or a necessary component thereof), advanced to sustain a judgment that a policy or course of action is right.

I will take a brief look at three different arguments that each try to justify self-interested action as invariably right: Sidgwick's *ethical egoism*, Ayn Rand's *objectivist ethics*, and a *utilitarian* perspective on self-interest. I do not present these perspectives as exemplars of the kinds of ethical arguments we might find people adopting empirically. Indeed, the notion of equating self-interest and ethics in this way may well seem odd if not downright preposterous.[10] The purpose of considering these arguments is, instead, to dislodge any impression that acting on ethical grounds invariably requires setting aside self-interest and at the same time to expose the difficulties encountered by a self-interest-based ethics. Acting ethically does not require a subversion of self-regard or interest, but it does minimally seek a reconciliation of that self-regard or interest with the self-regard and interests of others. It is in the face of this requirement that self-interest-based ethical arguments fail.

Ethical egoism, in a 1907 formulation by Henry Sidgwick, is the view that morally, one ought to pursue one's self-interest. Self-interested choices as self-interested are justified as right.[11] Following Sidgwick's account, this means that an individual should choose from among the alternatives faced the course of action that would yield "the greatest surplus of pleasure over pain" (1981, 121).[12] For the egoist, "a tendency to promote his own interest is the sole possible, and sufficient, justification of all his actions" (Moore 1988, 97) and also serves to justify the moral rightness of self-interest-based action by others. The egoist as actor is doing that which is morally right in choosing actions, in politics or in private life, that maximally advance his self-interest. The egoist as observer or judge of others, must, as such, conclude that they, too, are justified in pursuing their self-interest if he allows that his justification is as valid as one that any person might offer. It does not matter if these self-interests collide. Each is nonetheless justified in pursuing their individual aims. Margolis elaborates the egoist's stance: "The ultimate justification the egoist is prepared to offer is that he is uniquely himself, that what suits *him* justifies his conduct since no one else is, ex hypothesi, sufficiently like him (being different from him) to extend the justification to another in suitable circumstances. . . . [The egoist] does what he pleases, but there may be no other significant regularities that may be singled out [to justify the conduct]" (1971, 180–82). The ethical egoist cites no features of any action or choice when justifying it as good or right. Instead, the egoist defends his choices with the simple justification, "It is what *I* want."

As a public account of what is right, ethical egoism suffers from one primary flaw. Precisely when the egoist would be called upon to justify some action or proposal (when people's self-interests are in conflict) he will, in fact, fail to justify his conduct to others. The egoist, says Kalin, is thus "doomed to frustration . . . because justifying his behavior in terms of [his self-interest] gives an opponent no reason to cease his opposition if maintaining it would be in his own interest" (1970, 82). Falling back upon the subjective plea, "It is what I want," fails to serve as a justification at all. If challenged, the egoist can do little more than continue to repeat that simple claim.

Because of this key problem, it is implausible to expect that contentions about the significance of preference satisfaction will provide compelling justifications for the existence or the form of political action.[13] Self-interest (as given by ethical egoism) fails to provide a sufficient basis for policy choices if we

understand those choices as requiring public justification. It doesn't fail *because* the actions are self-interested, however. It fails because subjective claims of want are baldly devoid of any compelling interpersonal basis for support.[14]

But ethical egoism does not exhaust the ways in which someone might try to justify self-interested action as right. Ayn Rand's (1964) objectivist ethics, an ethics of "rational selfishness," sidesteps the problems facing ethical egoism. Her egoist must adopt a "rational, objectively demonstrated and validated code of moral principles which define and determine his self-interest" (p. xiv). That code incorporates three cardinal values: reason, purpose, and self-esteem, which entails that people act rationally, act productively, and aim to "acquire the values of character that make . . . life worth sustaining" (p. 22). According to Rand, these are the "values required for man's survival *qua* man—which means: the values required for *human* survival" (p. 28). According to her ethics, "the *rational* interests of men do not clash" (ibid.); hence no contradiction is implied by a person advocating egoism as the moral stance that each person should adopt. Her version of egoism ostensibly avoids the dilemma of conflicting self-interests and provides explicit (if contentious) standards of moral justification, allowing for the possibility of moral appeal and debate.

Early utilitarian theories, modern welfare economics theories, and social choice theories offer us yet another perspective by which we might sanction self-interested action. This normative vision understands the collective welfare as maximally advanced if each individual strives to attain his or her own interests.[15] An individual accepting this causal theory would view acting on her self-interested preferences as justified because that self-interested preference should merge with others' preferences to produce the collectively best outcome. (Of course, such an individual would argue that others should be acting on their self-interest, too.) Formally, this is not an egoist normative theory because the contention that an action advances one's self-interest is not itself invoked in justification; rather, the justification for self-interested action derives from collective considerations. Nevertheless, action aimed at advancing one's own interests is instrumentally justified as better able to achieve collective well-being.[16]

Armed with Ayn Rand's account of self-interest or this utilitarian justification, self-interested citizens would act not as consumers of the goods of politics but as individual legislators of the good. These ethics do not subvert egoism. On the contrary, each perspective develops a conception of morality that is consistent with its conception of each actor's self-interest and thus gives self-interested action moral justification. By joining self-interested value and moral right they provide self-interest with a language of public justification.

Ethical egoism, Rand's objectivist ethics, and this utilitarian account each confronts—and putatively resolves—a central problem that also guides the development of nonegoist moral theories, namely, the problem of formulating an understanding of self-interest and of morality such that acting on the demands of morality does not require that individuals subvert their own good. Kurt Baier refers to this problem as the "inconsistent triad" where:

1. Proving the rationality of some conduct to someone consists in showing him that it would be for his good.
2. Compelling moral considerations sometimes require us to do things quite irrespective of whether doing them would be for our good.
3. Moral considerations could not require this unless one could be shown that acting from them was rational whereas ignoring them was not. (1986, 13)

The problem of an incompatibility between self-interested and moral dictates is central to ethics, the philosophical solution elusive.[17] Yet (as I will argue shortly) the existence of this dilemma yokes our moral arguments to a concern for individuals and their interests.

We have no good reason to expect that our actual moral beliefs and conceptions of self-interest constitute a system that has overcome this dilemma. It is unlikely that either Ayn Rand's theory or the utilitarian conception capture our ideas about self-interest and morality. Nor is it likely that we always recognize some other form of compatibility between what we care about as individuals and the moral considerations that we find ourselves confronted with. Just as this dilemma permeates moral theorizing, it may well be a basic tension in many individuals' lives. Faced with this tension, the ethical individual does not simply evaluate and act so as to produce a maximally advantageous state of affairs for herself. But she need not ignore her own welfare or autonomy, or integrity when taking moral claims seriously and offering public justifications for her judgments and choices. What public justification requires is, instead, some account of how those interests are to be *reconciled* with the interests of others—some recognition of the requirements of community life.

Thus, while we may find it hard to imagine that human motivation could be at odds with the interests of the self, recognizing Baier's dilemma as a real, empirical dilemma means allowing for moral imperatives that confront and challenge one's interests. Even those who would confine the role of public justification in politics to the context of justifying democratic procedures for the aggregation of citizen preferences (as per social choice theory) must still acknowledge the limits to egoism that this recognition of the collectivity introduces. Consider, in this respect, how we might conceive of the point of justification in the social choice project.

First of all, establishing the goodness of some procedure might be something that could be used in a normative appeal to urge citizen compliance with its outcomes. This, however, makes no sense if people are pure egoists. They would not be willing to accept any particular outcome of a "good" or "just" demo-

cratic procedure if it were contrary to their own interests (however defined).[18]

Alternatively, we might see the mass public as never willing to go along with the outcome of any democratic procedure if it contradicts their interests. Questions of justice and good procedure do not matter to these "ordinary" people; they matter to certain disinterested parties in academia or in positions of legislative or judicial power. If these elites can certify procedures as just, then the weapons of the state can be legitimately used to enforce compliance among the egoists (citizens). This appears to be the view of Jules Coleman and John Ferejohn, for example. They suggest that "to assure *voluntary* compliance in a joint venture, cooperation must be to each person's advantage"; that in order to achieve successful collective action, policies "must be coercively enforceable"; and also that "coercion requires justification" (1986, 6). According to this interpretation, there really is no reconciliation problem for individuals but simply different kinds of individuals. Someone with the power to implement collective decision rules cares about justice (a dictator perhaps?), but the rest of the citizens, who will be subject to those rules, do not. Given the egalitarian premises represented in social choice axioms, this elitist interpretation introduces a certain irony—if not an inconsistency—into the social choice project.

If, therefore, advancing a conception of political actors as egoists, social choice theory's own account of the problem of justifying democratic procedures becomes problematic. However, we might, instead, understand social choice theories as saying that citizens are egoists in all circumstances *except* when it comes to questions of democratic procedure. Then they reveal a concern with justice or other standards of moral right and will go along with the outcome of any procedure that in the abstract, does a "good" job of aggregating preferences even if it conflicts with what they want. While this particular division between egoism and ethical concern does admit of a reconciliation problem facing citizens, it is also, of course, ad hoc.

SELF-INTEREST *IN* ETHICS

Acting on the basis of ethical concerns may conflict with acting on the basis of self-interest. What cases of divergence we recognize will, of course, first depend on how we define and study "self-interest" and the demands or dictates of ethics. That given, whether their dictates converge or diverge, and for whom, will also depend on the particular empirical circumstance at hand. In any given context, that which is to someone's material benefit, or maximizes someone's relative advantage over others, or satisfies someone's personal preferences may or may not be what that person or others judge as right or good—may not best advance the welfare of persons taken collectively, may not be best for society under some alternative consequentialist construction, may not be fair, may violate some right, and may not adequately fulfill commitments made to particular people or those made as a citizen.

I will look more closely at the possible divergence and (especially) convergence of self-interested and ethical claims. I will not say anything as yet about the purely circumstantial convergence of self-interested and ethical directives. Nor will I return to egoist arguments that always stipulate a convergence between the dictates of self-interest and ethics. Rather, I will discuss more limited ways in which preserving or advancing one's self-interest might be sanctioned by ethics. For even though some conceptions of good and right might confront and challenge our interests, we may also turn to normative arguments in their defense.

We might begin by defining "self-interest" as whatever it is that an individual values, noting that moral claims of goodness or moral directives of right may conflict with value as *defined* by the self. Wollheim conveys the essence of the subjective claim:

> The wants which the citizens' choices express need not, of course, be selfish or egotistical wants. When a man decides that he wants policy A more than policy B, he may well be moved not just by his own interests but by a concern for the welfare of others. But all the same, in choosing A he is not asserting that others want A, nor that A is in their interests, nor that A would be an ideal solution, nor that A ought to be realized; he would be asserting *tout court* that he wants A. (1964, 76)

This version of self-interest refers simply to that which an individual wants or desires—in rational choice language, that which yields maximal personal utility. It focuses not on personal welfare (as self-regard) but on personal autonomy (as self-definition). Analytically significant here is not the substance of what I value but that it is what *I* value, given recognition simply because it is mine. A disjuncture between self-interested and moral claims will arise if one's personal preference (whether self-regarding or not) fails to coincide with that which is justified as right or good. What I want to do might not be right (according to some persons, some argument, or some government); or what I like might not be that which I or others judge good.[19]

Yet these personal preferences need not be silenced by the din of contradictory moral claims. Our ethics recognizes the moral significance of personal desires *qua* personal, a significance that philosophers have represented by making a distinction between agent-neutral and agent-relative justification (Nagel 1986; Parfit 1984). Agent-*neutral* justifications base judgments of right in reasons that make no special reference to particular persons or their preferences, yielding *impartial* judgments. Bernard Williams describes such justification as requiring

> abstraction from particular circumstances and particular characteristics of the parties, including the agent, except in so far as these can be treated as universal features of any morally similar situation. . . . The motivations of a moral agent, correspondingly, involve a rational application of impartial principle and are thus different in kind

> from the sorts of motivations that he might have for treating some particular persons (for instance, though not exclusively, himself) differently because he happened to have some particular interest towards them. (1981, 2).[20]

Agent-*relative* justifications, in contrast, pay particular attention to the identity of particular persons, endorsing *partial* judgments. Focusing attention on the nature of social roles and relationships, some relative justifications sanction preferential treatment or concern for people who stand in particular relationships with the agent, under any number of descriptions, including friends, spouses, children, colleagues, and in some contexts, fellow citizens (call these group-relative). Other relative justifications give special attention to the dignity and worth of individuals and, consequently, to the subjective preferences of the agent (call these self-relative). It is by making such arguments about the dignity and worth of individuals that self-interest-as-subjective-preference finds its own normative voice.

In an important sense, however, both self- and group-relative justifications pay attention to the (judging, evaluating) self. Group-relative justifications do so by locating the self in social roles and relationships, giving special recognition in moral argument to a person's particular concern for his family, friends, colleagues, and so on. When set against impartial dictates, a concern for these individuals (or one of these individuals) is something of particular importance to the self. For example, what I or others judge fair might conflict with what I owe to my family; or my concern for some person *qua* person might conflict with my concern for another person who is my friend. Group-relative arguments about duty and obligation speak in defense of these partial concerns.

When set against subjective wants, however, those same partial concerns can be restrictive to the self. What I want may conflict with what I owe to my family or friends, or my interests may conflict with those of some group to which I have ties.[21] And these same subjective wants may also conflict with moral right defined in impartial terms. I may want to keep the wallet I found in the corridor, to forgo paying taxes, or to hire persons who look just like me; but these may not be the actions that an impartial morality sanctions as right.

Confronted with such conflict, the agent might defend acting on the basis of her personal preference by drawing on self-relative normative arguments—commonly, by appeals to a principle of autonomy or freedom.[22] Consider an example. A homeowners group proposal, if accepted, will dictate that each homeowner must build the same kind of fence. As a single homeowner, I may have a purely personal preference for a fence different from that described under the proposal. In fighting the proposal, I would not back away from an understanding of my preference as subjective by arguing that my fence preference is the preference that everyone should adopt or that it is not just my preference but is better in some way (of course I might, but the point here is to assume I won't). But in recognition of the conflict between my preference and the proposal, I might appeal to some understanding of property rights or rely on some other general (and agent-relative) argument about autonomy in an effort to protect my fence preference and defeat the proposal.

Partiality toward oneself has received a great deal of recent attention among philosophers (e.g., Nagel 1991; Railton 1988; Raz 1986; Scheffler 1982; Williams 1973, 1981, chaps. 1, 3). For example, Scheffler (1982) has argued that morality must recognize an "agent-centered prerogative" that releases people from acting in accord with the dictates of an impartial moral rule when it conflicts with important personal projects and commitments. He describes the potential conflict:

> People typically acquire and care about their commitments quite independently of, and out of proportion to the value that their having them and caring about them is assigned in an impersonal ranking of overall states of affairs. . . . Each person has a point of view, a perspective from which projects are undertaken, plans are developed, events are observed, and life is lived. Each point of view constitutes, among other things, a locus relative to which harms and benefits can be assessed and *are* typically assessed by the person who has the point of view. This assessment is both different from and incompatible with the assessment of overall state of affairs from an impersonal standpoint. (pp. 9, 56)

Incorporating such self-partiality within a moral argument allows a conception of subjective self-interest as "important personal projects and commitments" to bear some moral weight when in conflict with other moral claims. Thus, my commitment to nonviolence—rather than nonviolence per se—is accorded moral status. Or my hard-earned money is given status as mine, something central to my plans of achievement, not to be transferred unconditionally to meet a neutrally justified goal.

Some components of our ethics seek to bind the community of individuals, construing moral right in terms that are authoritative and transindividual, placing demands on people that may constrain them from pursuing their personal interests, whatever they may happen to be.[23] Other components seek to recognize the dignity and worth of each individual and of their particular intracommunity ties, providing individuals with a voice of dissent against the dictates of an impartial morality. By distinguishing among impartial, group-partial, and self-partial claims, we can capture the tensions that may erupt. What some person, because of her individuality, particular goals, and life experiences, has come to value may diverge from other partial claims (what one must recognize as owed to others by virtue of particular ties and relationships with them) or from impartial claims (what "anyone" must recognize as right or good). A member of Congress, for example, might judge a policy as best for *his or her* constituency (a group-partial claim) but not best for *any* constituency or for society writ large (impartial claims), nor for his or her own inter-

ests. In the face of such conflicts, personal interests are not inherently trumped by any properly "moral" claim.[24] If impartial or group-partial moral dictates contradict personal preferences, then some resolution must be sought; and such resolutions may delineate trade-offs that are defended as *justified*.[25]

Thus, by appealing to principles of freedom or autonomy, we may defend acting on the basis of our purely personal preferences when doing so conflicts with that deemed right on other grounds. My preference may not be that which I or others deem good or right, but it is nonetheless morally significant *as mine*. We embrace our own individuality in calling upon such principles, while acknowledging the individuality of every other citizen, as well.

But we might instead deny our uniqueness or individuality when speaking in defense of our interests. People may (and perhaps often will) appreciate what they value as *valuable*—commendable for reasons that others should acknowledge. Personal "preferences" may not be purely personal. Our member of Congress, for example, may *want* what he judges best for his constituency or deems best for society as a whole. Under such circumstances, value as assessed by the self may be value defended as "good"; actions of interest to the self may be actions defended as "right". Any distinction we might want to make between an individual's conception of her interests and her conception of moral good or right could collapse.[26] Consider some examples.

Parents who value what they deem good for their children may argue, further, that what is good for their children is good for any child or good for all of the children who live in their community. For example, in expressing a wish that their child be educated in small classes, parents may not be merely offering a personal preference. They may contend that others must recognize—and government must protect—such education as a *good*. Or, while someone may favor a policy that prohibits logging in United States parkland, they may further defend that position as *right*.[27]

Equally well, one's self-regarding interests may themselves be defended as good or right. For example, people might advocate public health care provision while defending that policy through arguments citing "urgent interests" (Scanlon 1975), needs, or rights. Or, consider the poor and the wealthy each turning to government in search of economic advancement. They may not *simply* be seeking to advance their "self-interest" (*qua* material gain). Some argument about distributive justice might be invoked to justify the efforts of the former (though not the efforts of the latter). Some utilitarian account, calling upon supply-side economics, might be invoked to justify the efforts of the latter (though not the efforts of the former). Even if we bring to our political inquiries some self-regarding definition of self-interest, we are not defining something that falls outside of ethics.

Thus, while some understandings of what is good or right will sanction policies that do not maximize a person's self-regarding concerns or that fail to coincide with a person's personal likes or wants, people may also turn to normative arguments in defense of their interests. They may defend acting on their purely personal preferences or their concern for particular others. They may defend what they value as "good" or "right". Absent any smooth resolution of Baier's "inconsistent triad," we will find our interests to be both challenged and championed by our ethics.

CONFRONTING THE ETHICAL CYNIC

"All this talk about ethics is baloney," says the ethical cynic. The cynic recognizes the existence of moral views—expressions of concern about other's interests, assessments of goodness and right, claims of virtue and obligation. But, to use Kenneth Thompson's characterization of the "realist," the cynic sees these as "calculation, rationalization and self-justification concealing the taint of self-interest" (1987, 12). Moral concepts only muddy the picture of what truly can only be understood as the operation of self-interest. Individuals who purport to take moral values seriously are either insincere, or sincerely misguided, or simply confused. But that does not mean that researchers should be taken in.

I have several responses to the cynic. First, if moral claims are all rhetoric and mystification, of no significance to anyone, then how could they become a strategic tool? Why is self-interest something that must be concealed? How could normative arguments facilitate self-interested achievements? If everyone were purely self-interested and immune to moral claims of value, they would be of no strategic value whatsoever. Why would we "speak the language of morality, impressing upon our fellows their duties and obligations, urging them with appeals to what is right and good, if we could speak to the same effect in the language of prudence, appealing to considerations of interest and advantage?" (Gauthier 1967, 460). Ultimately, then, if Grofman and Feld are right that "politicians obviously recognize that appeals to 'right', 'good', and 'fair' policies have some political appeal" (1988, 572), then at least *some* of the people whose evaluations and choices we study cannot be ethical cynics themselves.

Second (as I have already argued), there is no simple division between moral and self-interested claims. Self-interest is not something that always lurks sneakily beneath moral rhetoric. In many cases, particular interests are a critical and acknowledged component of the moral justification itself. In such cases, in fact, one problem is to provide a sufficiently compelling argument about the interest involved, so that the preservation or enhancement of the interest can be given a rights-based justification or one that makes use of liberal ideas about need or harm.[28] In other circumstances, of course, the justification for right action will not make reference to the interests of the actor, and the conjunction of interest and moral value can be a problematic one. One variant of this problem cites the importance of intention to the

judgment of acts and persons. The evaluation of acts and actors can be sensitive to the imputed intentions of the actor, recognizing, for example, distinctions between an act of charity designed to enhance one's political popularity and one undertaken with charitable intentions. If the overlap between moral and self-interested value is a transparent one, then even the "sincere" charitable actor might want to conceal the coincidence of personal gain.

None of this means that all moral claims are necessarily sincere. *Any* policy given sanction as "right" will always have consequences that affect people differently. If we take self-interest to be concerned with self-regarding consequences, then we can always divide people into categories according to some objective understanding of how favorable or unfavorable the consequences are for them. It is this inevitable overlap that raises the specter of ethical claims advanced purely in the service of one's self-interest. But as I see it, this establishes nothing except, perhaps, to point to a problem that moral justification is invoked to solve: It is because we are different and yet interdependent that we call upon moral argument to justify political outcomes, seeking solutions upon which all can agree. What matters to the sincerity question is not the convergence or divergence between self-interested and ethical dictates but the consistency with which ethical principles find application across circumstances under which they might be invoked:

> They must not recognize some particular principle as important and weighty in one case and then wholly ignore it or treat it as inconsequential in another, refusing to apply it even though no other, competing principle of comparable power is involved. That kind of incoherence is hypocrisy; someone who is unwilling to apply a principle consistently does not really accept it, and so has no genuine justification even for those decisions in which he claims to be following it. He cites the principle then only to disguise the fact that his decision has some other, less creditable, basis.[29]

Moral arguments might be used by some simply as a tool for the advancement of their personal interests; but such insincerity does not simply follow from the recognition that any claim of ethics can be described as advancing some, and not others', interests. We should recognize the necessary overlap between these consequences for "self-interest" and moral sanction and subject it to careful theorizing and investigation.[30]

Scholars typically solve this problem of overlap by defining it away—commonly by stipulating that explanatory priority belongs to explanations based on self-regard (the *insincerity strategy*). For example, Citrin and Green classify as self-interested "those choices that enhance the actor's own welfare regardless of whether others, including his reference group, benefit or suffer" (1990, 6). And Schlozman and Tierney reason as follows: "When professors plead for a strong university system in the name of . . . general benefits, there is an ambiguity to their position. On the one hand, they surely believe sincerely in the importance of higher education to the good of the nation; if they did not, they probably would not have become professors. On the other hand, their own livelihoods are at stake. Hence, it is not unreasonable to consider their seemingly public-spirited rhetoric as merely a cloak for the cynical advancement of their own private interest" (1986, 34). If a moral justification is available for actions or policies that advance the interests of the self, the former is deemed irrelevant by definition. Others place explanatory priority on competitors to self-interested explanations (the *sincerity strategy*). For example, in Sears, Hensler, and Speer's "symbolic politics" alternative, "whether or not the issue has some tangible consequences for the adult voter's personal life is irrelevant" (1979, 371). In such cases, so long as evidence for an alternative to self-regarding explanations is available, the existence of any self-regarding benefit is immaterial.[31]

Aside from the fact that they simply define away the problem, each of these stipulative strategies is vulnerable on other grounds, as well. The insincerity strategy has potent normative implications since *selfish* is itself a normatively loaded term. Poor people supporting poverty programs, women supporting abortion rights, disenfranchised groups seeking the vote, and slaves seeking freedom cannot be "sincere" in this view. Only the wealthy, the male, the franchised, the slaveowners can express a "disinterested" point of view. The sincerity strategy, in contrast, mutes a number of questions about how—and for whom—self-regarding and moral claims overlap. Especially if some moral claims are insincere, perhaps, but even if all moral claims were sincere, we would want to know how features of society (historical traditions, contemporary institutions, political elites) constrain what constitute "good reasons" or "acceptable justifications" so as to advance the interests of some and not others.[32]

CONCLUSION

Viewing citizens as ethical actors is not the perspective of a hopeless idealist. Though rejecting a conception of politics based wholly on self-interest, it does not deny the significance of personal preferences and self-regarding interests or imply a blanket acceptance of all normative justifications as sincere. Nor does it wander off into other absurdities by, say, denying the reality of political conflict and controversy and presuming a citizenry oriented happily toward the pursuit of common interests. It means regarding each citizen as an individual with her own unique hopes and desires who is at the same time joined with others, part of and continually giving shape to a common social and political life. It sees in political diversity and controversy not just conflicting interests, but also competing claims about what "we"—unique individuals, linked to particular others through social roles and relationships, and together forming a single citizenry—*ought* to do or seek. It challenges us to bring

to political inquiry an account of interests and ethics that allows us to explore how they merge and diverge, an account that neither denies the real concerns of individuals nor disregards the exigencies of society and the requirements of communal life.

When judged from this perspective, simply expanding our notions of interests or utility to include "global values and group identification"; "the desire to maintain an established pattern of social relations or to defend distinctive cultural values and practices"; or "a concern for the welfare of reference groups or the pursuit of solidarity, status, self-respect or cultural hegemony"[33] is not a sufficient response to the failures of the economic approach. We cannot just expand our understanding of what an individual might value without recognizing how these values might uniquely function in sustaining political claims. If we reject the subjective account of value as an adequate representation of our experience of valuing, then we must recognize that evaluative judgments vary in intelligibility to others and in the force of the justifications that sustain them.

At the same time, going beyond self-interest in accounting for public opinion and political choices requires more than the application of typologies which juxtapose self-interested and non-self-interested concerns. Characterizing politics as ethical does not invoke "something that is not self-interest"; and the project of studying ethics in politics does mean adopting an ethical account of the bases of political judgment and choice to serve as a competitor to a self-interested account. Such typologies impose a dichotomy that is false, erasing from our inquiries the very questions about interests that are raised by an account of politics that invokes ethics, namely, to what extent *do* citizens find their interests to be challenged by what they or others deem good or right? And how are such contests resolved?

In practice, invoking ethics in politics means expanding our understanding of the criteria by which the citizen may judge political options, recognizing that together with any consequences for one's "self-interest" are features that one's ethics might commend or condemn. We might, for example, bring survey or intensive interviewing techniques to bear on these questions by asking people to judge persons or policies both on relevant moral grounds and in terms of the consequences for their lives.[34] This would allow us to investigate whether similarly situated individuals vary in their perception of whether some policy advances their interests (as they themselves define them); whether people differ in the extent to which they see any policy or option as meeting some criterion for justification (e.g., some, but not others, might see a policy as violating a particular right or causing a particular harm); and whether there are variations among people in the weight they give to different moral criteria when forming their policy opinions (e.g., appeals to autonomy or obligation might be more significant among those whose autonomy or commitments to others are in fact challenged by some policy or proposal, compared to their significance among those situated otherwise).

If politics articulates interpersonal standards of value and justification and if political expression requires justifying one's views as good or right, then we must recognize these judgments as significant to political behavior research. This challenges empirical researchers to recognize that doing social science does not mean that we should avoid anything remotely "normative" but, in a way, the reverse. Public standards of value found in the discussions of moral and political theorists must be incorporated into empirical work, finding service in an explanatory role. As we proceed with our political inquiries, we will want to develop an account of self-interest that is faithful to the concerns of real people and also a sophisticated account of the conceptions of good and right to which our politics gives voice. These developments are necessary if we want to expand our understanding of how interests and ethics function in a politics that requires public justification.

Notes

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1. Relatedly, some speak of "a taste for honesty" (Frank 1990, 93) or argue "[that] individuals have different tastes, that one man's meat is another's endangered species" (Schlozman and Tierney 1986, 23).

2. Interests are usually not equated with desires or wants but, rather, functionally related to them. A policy or action is in some person's or some group's interest if it is instrumental to the attainment of something that they (individually or collectively) desire or want (Barry 1965; Held 1970).

3. Those arguing recently for a conception of politics that deemphasizes preference aggregation and emphasizes debate over, and judgments about, public goals and policies include Cohen 1986; Dagger 1986; Grofman and Feld 1988; Mansbridge 1980; Sagoff 1986; Sunstein 1990; and Waldron 1988.

4. The normative questions of politics are not, however, merely the questions of ethics asked in politics. Moral justifications define social dictates but not necessarily political solutions; and "political", not "ethical", justifications speak directly to the functioning of political institutions and to the bases and scope of government action. While I will speak of "ethics," much of what I have to say refers more generally to publicly justified (or justifiable) judgments, whether political or moral.

5. Of course, ethical judgments may not always be accompanied by public justifications; but they are implicit and may be called for in a context of discussion and debate. Clearly, public justification will be most evident in some institutional settings (e.g., Congress, courts, juries, public meetings).

6. The structure of utilitarianism is identical to that offered by rational choice theories' calculi of decision tying maximum personal utility to choice. Whereas the utilitarian looks to social utility when formulating the judgment of right, the rational actor looks to personal utility in determining what is the best alternative. Although their theories of value divide them, their formal framework is otherwise the same.

7. While I will speak of good versus bad and right versus wrong, these are not the only evaluative categories important

to ethics and thus to the general conception of evaluation I am advancing here. Our evaluative language is much richer than is revealed by these dichotomies, as those working in a virtue-theoretic tradition have emphasized (e.g., Meilaender 1984; Pincoffs 1986).

8. On this point, see also Gaus 1986 and Parks 1982. By saying that ethical claims of value are different from personal desires, I am edging toward issues of major philosophical controversy. That controversy centers on the ontological status of value and the epistemological status of judgments of value. The philosophical literature on these issues is vast, fluid, and replete with many terminological nuances. One recent review addresses eleven relevant "isms" (Sayre-McCord 1988, intro.). Here, I am simply trying to sketch distinctions we might find empirically important, rather than adopting one or another of the positions in these philosophical controversies. I give closer attention to these issues in Stoker 1990.

9. This is so whether evaluative conclusions are conceived as resulting from deliberation about justification or not; for even if they arise from other processes (e.g., learning processes), justificatory premises and beliefs may hold them intact.

10. Numerous scholars have contended that in democratic politics, self-interested claims simply cannot be advanced to justify political outcomes. Anderson, for example, suggests, "One cannot justify a policy recommendation on the ground that 'it would make me and my friends richer'. However refreshing the candor of such an argument may be, it does not and cannot stand as a legitimate warrant for public action" (1979, 718). Elster maintains, "There are certain arguments that simply cannot be stated publicly in the political setting. In political discussion, it is pragmatically impossible to assert that a given solution be chosen simply because it favors oneself or the group to which one belongs" (1983, 35). Citrin and Green contrast the "normative and institutional contexts" of political and economic choices, suggesting that in politics, "people are frequently expected to decide issues on the basis of principle, to make decisions about what is right or wrong, . . . to behave in the general interest and to justify their choices in ethical terms" and that "selfish actions are often viewed as illegitimate" (1990, 20).

11. Ethical egoism does not assert that people *are* motivated by the singular pursuit of their self-interest (however understood). Nor does it advocate that people adopt, merely for themselves, a self-interest maximizing principle of action. Such a person may be acting on principle, but not a moral one. Ethical egoism requires that a person adopt the self-interested maxim as alone prescribing morally right action and, further, advocate that maxim for all persons. The ethical egoist's justification of self-interested action as "right" requires acknowledging the force of those reasons for defining the right action of others. This extension to others of the egoist's justification reflects what moral philosophers refer to as the universalization of moral judgment. Without such universalization, the egoist arbitrarily restricts the set of persons given access to his justification, saying, "I am justified in doing what I want, but you are not justified in doing what you want." With this arbitrary restriction, the view is not a moral one; and if some restriction is justified, the theory is no longer ethical egoism.

12. For Sidgwick, *pleasure* is "taken in its widest sense, as including every species of 'delight', 'enjoyment', or 'satisfaction'," where "all qualitative comparison of pleasures must really resolve itself into quantitative" (1981, 93–94). Ethical egoism could, of course, be formulated nonhedonistically. (Sidgwick was not writing as an unequivocal advocate of ethical egoism; he also found utilitarianism rationally compelling.)

13. When considered in empirical terms, ethical egoism's implausibility is also found in its inability to account for significant components of our ordinary moral thinking and debate, namely, the components that identify value in actions or states of affairs not just on the grounds that it "advances my interests" or "is what I want" but on the grounds that it is "fair," advances collective welfare, and so on.

14. In contrast, we can, for example, envision the following (Pareto optimizing) claim being offered in defense of some policy: "It will be to my benefit and not impose any costs on anyone else." Notice that ethical egoism does conceive of citizens as agreeing that "each should pursue his or her interests." That general contention can ostensibly be interpersonally justified. But when the egoists find themselves with incompatible wants, they are left without any argument about how to proceed.

15. The prisoner's dilemma, a strategic circumstance that contradicts this normative conclusion, is a "dilemma" defined within this framework. Rational self-interested actors will not opt for actions that are collectively best. Because the prisoner's dilemma outcome is also not individually optimal, this context often raises a different claim about the convergence between morality and self-interest (one also found in utilitarian thought), namely, that self-interested people may best achieve well-being by not always directly aiming to achieve their own well-being. Axelrod's (1984) work on iterated prisoner's dilemmas might be seen as suggesting this conception of self-interest and morality. Strategies that are "nice" and "forgiving" are evolutionarily successful.

16. Sen (1987) focuses some (critical) attention on the empirical accuracy of this kind of claim. Hirschman (1977) traces the historical development of the idea.

17. This tension is vividly evident in Brock's definition of *morality*: "Morality in the narrow sense: includes all those principles that restrict the individual's personal goals and his advancement of his self-interest. Morality in the broad sense: is 'the art of life', that is, the precepts instructing people as how to live, and what makes for a successful, meaningful, worthwhile life" (1988, 570). In other words, morality is both limiting individuals' pursuit of their "self-interest" and telling them what it is!

18. Social choice theorists might, of course, claim that the outcomes of a good or just procedure will always be in the interests of each citizen. But this would be simply denying the possibility that people will ever face a problem of reconciliation.

19. The latter, among other things, captures purely expressive conflict. It recognizes that we sometimes want to preserve our opinions from outside scrutiny and remove them from a context of public justification. We sometimes say, "It's just my opinion." We fall back on a subjective account of value that while it may or may not be rooted in some principled objection to an objective metaphysic, has the main consequence that it takes our opinions out of the public spotlight, attempting to protect them from the criticism of others.

20. Williams is writing here about "the moral point of view" and does not use the term *agent-neutral justification*. When writing about these issues, others speak of judging from the "impersonal standpoint," of "impartial justification," and so on.

21. Altruism (persons' forgoing their self-regarding interests in favor of the interests of others) is captured by this tension between self- and group-partial claims. As such, contrasting altruism with egoism does represent the potential for conflict between some claims of morality and of self-interest. But they are conflicts of a particular kind and presented by a particular, and perhaps narrow, collection of circumstances, namely, those where we can establish a discrepancy between self- and other-regard, where gain to the self may be denied to enable others to profit instead. Altruism is also particularly suited to addressing the philosophical question whether it is ever rational to deny one's self-interest (e.g., Nagel 1970). Altruism finds its niche here, not in somehow representing the "essence" of morality.

22. Speaking more generally, partial claims justified in agent-relative terms can be formalized into rules or principles, which are then justified in agent-neutral terms. Acting on the basis of one's subjective preferences can, as such, be neutrally justified if universalized for any person or persons so described. "It's what I chose" could (in some specific context) be defended as important, universalized for any "I." Similarly, while group-relative justifications take note of the particular

identities of persons by virtue of socially defined relationships, they also can be universalized to refer to any particular set of persons so described (Nagel 1986).

23. Such claims may be advanced in the name of maximizing social welfare (as in utilitarianism) or some larger set of intrinsic goods or in the name of some conception of community, nature, God's will, or the like.

24. A philosophical caveat: How to give weight to partial and impartial claims of value is always explicitly or implicitly resolved by philosophical theories of ethics, but they do not always recognize partial claims as "moral." There are two bounding perspectives. The first sees morality as yielding dictates grounded in impartial justification (as with a utilitarian morality), with partial concern for the self or others lying outside morality. If the interest of the self does not coincide with that deemed morally right, a contest between self-interest and morality ensues. The second seeks to bring partial claims within the bounds of morality, giving self-referential partiality the force of moral justification when it confronts other moral dictates. While the first sees the individual as asking, "Why should I be moral?," the second sees the person as asking, "What does morality require?" According to either perspective, the person may reject group-partial or impartial dictates in deference to his self-interest, but the second might sanction such choices as moral. Still other ethical theories might accept some forms of partiality as moral (e.g., partiality to one's children) but exclude partiality to oneself. In such cases, the individual is still asking, "Why should I be moral?" However, a distinction is maintained between self-relative and group-relative partiality. The distinctions among self-partial, group-partial, and impartial judgment are important whether the lines of confrontation are drawn across, or within, the boundaries of "morality."

25. If the individual's personal latitude is too narrowly constrained, individual integrity (Williams 1973) and autonomy (Raz 1986) may be threatened. Yet widening that preserve draws accusations of self-serving egoism: "In defending the legitimacy of agent-relative principles we must guard against self-deception and the escalation of personal claims simply to resist burdensome moral demands. It is not always easy to tell, for example, whether a morality that leaves extensive free space in each individual's life for the pursuit of personal interests is not just a disguise for the simplest form of badness: selfishness in the face of the legitimate claims of others" (Nagel 1988, 170). Scheffler imbeds the agent's prerogative to pursue his own commitments in an otherwise demanding ethics, combining strong consequentialist claims on an individual's attention with a special preserve where the individual's own conceptions of the good may predominate. This hybrid between a strict consequentialist and a purely rule-based approach flips the usual emphasis of the latter. Rule-based deontological views (like most accounts of liberalism) often narrowly demarcate the zone of moral constraint, leaving a wide field of individual prerogative.

26. The distinctions among partial and impartial judgments depend on the identity of the evaluator, not on the substantive reasons underlying a personal or a moral evaluation. As such, there is no inherent conflict among the policies or outcomes that each evaluative perspective may sanction as best: the evaluation of the self *qua* self, may coincide with the evaluation of the self *qua* located in some role or relationship and of the self *qua* person. In other words, it is entirely possible to conceive of a person's own evaluative claim as capable of being supported by group-relative or -neutral justifications.

27. Lurking in these examples are further possible distinctions. For example, we might find important a distinction between people who value something *because* it is good or right and those who value something for some other reason ("It happens to be what I like") but argue its moral or political significance on the basis of its goodness or rightness.

28. These are, of course, only compelling as liberal standards and in a liberal society such as ours.

29. Ronald Dworkin to the Editor, *New York Review of Books*, 1991. p. 65.

30. The question of sincerity will not be easy to settle empirically. First of all, we would need data that meet the comparative requirement set out in the text. Further, the consistent application of a principle or rule does not imply that we will observe a series of judgments or decisions each manifestly instantiating that rule. Countervailing moral considerations particular to certain circumstances may undermine any simple principle–decision correspondence.

31. Things are sometimes murkier than this—at least in part because of the statistical methodology often used to study these questions and the corresponding application of statistical controls. Nevertheless, the conjunction of self-interested and non-self-interested reasons is not itself usually studied (but see Kinder and Kiewiet 1981; Tyler 1990).

32. Examples of systematic convergence between interests and ethics abound. Individuals may enter adulthood with or without the character that our society rewards, an aspect of what Nagel (1979, chap. 3) and Williams (1981, chap. 2) have called "moral luck." The American moral culture defines the maternal role as more constraining to women than the paternal role to men, delineating gender-specific trade-offs between the interests of selves and duties toward children.

33. Quoted values come from Citrin and Green 1990, 11, 17, 23.

34. We might also pursue these questions using observational methods, albeit in a more limited fashion, by applying to a set of public policies some objective coding scheme defining whose self-interests are advanced and what moral arguments might be mustered to sustain support or opposition. Making comparisons across policies would allow us to observe how support varies with the interests that are served by them and with the moral justifications that sustain them. For example, a policy that is fair according to one criterion of fairness may receive more support than policy that is fair according to another criterion of fairness. Some might never act against their self-interest in supporting one or another of these policies, while others may do so in support of one and not the other; and so on.

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HEGEL'S THEORY OF MODERN BUREAUCRACY

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I make three contentions. First, Hegel's theory of bureaucracy is as comprehensive as Weber's ideal type of bureaucratic organization. I argue this by making a sociological reformulation of Hegel's model and comparing it with the Weberian paradigm. Second, Hegel adapts the Aristotelian category of practical judgment in characterizing the bureaucratic activity as subsumption. *This characterization is contrary to the dominant view that bureaucracy embodies mere instrumental rationality and solves some difficulties in Hegel's political thought. Third, Hegel's conceptualization can contribute to a liberal theory of bureaucracy that apprehends modern political reality more adequately than the skepticism of classical liberals and contemporary libertarians about bureaucratic organization.*

Bureaucracy, a conspicuous political institution in the modern state, is also a major theme in contemporary sociological literature. Hegel formulated the first theory of modern bureaucracy in the *Philosophy of Right* (pars. 287–97 [1976]).[1] However, this work has not exerted much influence on the sociology of bureaucracy due to the philosophical way of treatment. Instead, Max Weber's ideal type of bureaucracy (Weber 1978, 217–26, 956–1003) has had a pervasive influence in the development of the sociological tradition. Weber's conceptual and historical analysis provides a comprehensive framework for the empirical study of bureaucratic organization, though his analysis has been modified in details (Luhmann 1982, 30–41). In its normative dimension, the Weberian paradigm assumes that bureaucratization expresses the technical rationalization of modern politics. This normative assumption underlies critical theorists' critique of instrumental rationality thesis (Habermas 1970, 1984–89, vol. 2).

I shall demonstrate that on the level of institutional analysis, Hegel's characterization of the structural attributes of bureaucratic organization is as comprehensive as Weber's ideal type. The affinity between Weber's and Hegel's theories of bureaucracy has been observed by Avineri (1972, 160) and Jackson (1986),[2] but a more theoretical treatment is needed beyond the mere comparison. I shall also argue that on the level of normative evaluation, Hegel proposes an interpretation of bureaucratic activity entirely different from Weber's, one that may be articulated into an alternative to Weber's instrumental rationality thesis. Hegel shows that the rationality of modern bureaucratic activity is not technical but practical (in the Aristotelian sense); and he correlates bureaucratic and judicial activities. The implications of this view will be analyzed by way of Gadamer's theory of hermeneutics.

The recent trend to depoliticize Hegel's political philosophy also urges a reexamination of his theory of the state. Even the influential studies by Ritter (1982) and Riedel (1984) have focused too much on civil society and not enough on Hegel's theory of the state. Since Hegel sees bureaucracy as the main governing organization in the modern state, his contribution to the sociology of bureaucracy is impeded by this depoliticization tendency.[3]

A more philosophic reading of Hegel's *Philosophy of Right* has been launched by Michael Oakeshott. In his seminal *On Human Conduct* (1975), Oakeshott distinguishes two concepts of the modern state: the paradigm of *societas* (the state based on the rule of law) and the antithetical *universitas* (enterprise or purposive organization). Unlike most commentators of Hegel's political theory, Oakeshott identifies Hegel's theory with the concept of *societas* (pp. 259–63), paving the way for a liberal reinterpretation of Hegel's thought that is also philosophically interesting (Smith 1989a, 1989b). However, Hegel's view that bureaucracy is the principal governing organization poses a major difficulty for Oakeshott. Like other classical liberals, including Tocqueville and John Stuart Mill, he is deeply skeptical about bureaucracy. For Oakeshott, bureaucracy is one of the prototypes of *universitas* (1975, 267). As a result, he emphasizes Hegel's theory of recognition but overlooks Hegel's theory of bureaucracy.

I shall show that Hegel's embrace of bureaucracy can be explained within the liberal framework. I shall also argue that for Hegel, bureaucracy is not only compatible with, but indispensable to, the modern constitutional state in view of the disruptive effects of civil society. In this respect, his ideas may contribute to a liberal theory of bureaucracy.

THE IDEAL TYPE OF BUREAUCRATIC ORGANIZATION

Hegel discusses bureaucracy in his treatment of the executive power (*Right*, pars. 287–97). The discussion is brief; but this does not imply that Hegel considers bureaucracy less important than other constitutional powers. In his treatment of the executive power, Hegel analyzes modern bureaucracy as a form of political organization, its constituent members, and its relation with the social environment.

Hegel describes the executive power as follows:

> There is a distinction between the monarch's decisions and their execution and application or, in general, between his decisions and the continued execution or maintenance of past decisions, existing laws, regulations, organizations for the common ends, and so forth. This task of *subsumption* conceptualizes the *executive power* in itself, which includes the powers of the *judiciary* and the *police*. It has immediate bearing on the particularity of civil society and makes the universal interest valid over particular aims. (*Right*, par. 287; emphasis original; trans. based on Knox's)

For him, the executive mediates between the state and civil society. Its distinctive function is to execute and maintain existing legal norms. In its relation with civil society, the executive upholds universal interest among particular pursuits. Its specific mode of activity is designated as *subsumption*.

The organizational characteristics that Hegel attributes to the executive indicate that it is a bureaucracy in the modern sense:

1. It has a functional division of jurisdictions in accordance with different tasks. (*Right*, par. 290)
2. The divided branches are articulated by the principle of hierarchy. (par. 290)
3. The office is separated from its incumbent; there is no natural or necessary link between them. (par. 291)
4. Since the operations of bureaucracy have been objectified, bureaucrats need not be geniuses. The criteria for entering the civil service are knowledge and proof of ability through examinations, not birth or status. (par. 291)
5. Modern bureaucracy tends to diminish the traditional type of social stratification, because talent becomes the main criterion of recruitment. It is the most appropriate administrative organization for an egalitarian society. (par. 291)
6. Bureaucrats live off the fixed salary remunerated by the state in order to be independent from external influences. They should exercise their authority according to the common good. (par. 294)
7. The main advantages of a centralized bureaucratic organization are maximum simplification, speed, and efficiency in handling state affairs. (par. 290, add.)

Concerning civil officials, Hegel designates them as the "universal class" because the end of their activities is to realize the universal interests (*Right*, par. 205). Their universality is contrasted with the pursuits of the particular interests by the members of civil society. Civil officials are the pillars of the state because they are endowed with the highest political consciousness and knowledge about public affairs (par. 297, add.).

As to its scope of jurisdiction, bureaucracy has two functions. On the one hand, it superintends the activities of corporations in civil society so that the latter do not degenerate into egoistic interest groups. On the other hand, the highest officials constitute the advisory committee for the monarch and report to him the concrete situations of public affairs for his final decisions (Hegel, *Right*, pars. 283, 289).

Hegel is also aware of the hazards of modern bureaucracy and sees it resulting in two major problems. First, if the functional division of labor is excessive, incumbents' activities become mechanized. Bureaucrats therefore need an ethical education to cultivate their capacity to evaluate public affairs from universal perspective (*Right*, par. 296).[4] Second, bureaucrats must be accountable for their actions. Hegel proposes two methods of control, one internal and one external. Internal control consists in the ethical education and organizational discipline of bureaucrats, external control in supervision by the sovereign and the corporations of civil society (pars. 295, 297).

These are the sociological aspects of Hegel's theory of modern bureaucracy. Table 1 compares his model with Weber's ideal type. It is clear from the table that Hegel and Weber agree about most organizational characteristics of modern bureaucracy, the exceptions being criterion for recruitment, type of stratification, and mode of activity. The disagreements will be dealt with further on; here we are interested in the issues on which they agree.

Hegel and Weber recognize the novelty of modern bureaucratic organization. For both of them, the emergence of the modern state from feudalism is a gradual but irreversible process by which sociopolitical relations have become both institutionalized and impersonalized. Under feudalism, aristocrats appropriated administrative positions and resources as their own privileges. During the era of absolute monarchy, feudalism faded away and was replaced by the modern administrative apparatus (Weber 1978, 983–87). This course of political change, which culminated in the French Revolution and its aftermath, was the concern of many nineteenth-century thinkers (Bendix and Roth 1971, 131–40). The controversial question was whether the newly emerging bureaucratic state was different from the traditional bureaucratic empire. Tocqueville, in a well-known criticism, regards the modern centralized state as the result of the "leveling-up," or equalization process (1955, 81; 1966, 10, 690). He emphasizes the continuity between the ancient regime and the modern state, thus undermining the new source of legitimacy declared in the French Revolution. From the Tocquevillian perspective, the modern state with its centralized bureaucratic apparatus falls into the category of despotism in the classic typology of Montesquieu (Tocqueville 1966, 678–79, 690).

Both Hegel's and Weber's concepts of modern bureaucracy are antithetical to this line of criticism. Their common approach is to distinguish the genesis of a social formation from its function. Bureaucracy undeniably originated from the struggle between monarch and aristocrats; the crown used the efficient and merit-based bureaucratic apparatus to encroach on the privileges of aristocrats. However, having accomplished this historical role, bureaucracy be-

TABLE 1

Weber's and Hegel's Theories of Modern Political Authority

CRITERIA	LEGAL-RATIONAL AUTHORITY (WEBER)	THE POLITICAL STATE (HEGEL)
Legitimacy	legality	political sentiment
Organization		
Division of powers	constitutional division of jurisdiction	
Top positions	organs, elected or appointed political leaders, notables, or professional politicians	
Administrative staff	officials (civil servants)	
Structure	hierarchy of jurisdiction of "agencies"	
Office	defined as "impersonal rights"; separation of official and private sphere; fixed remuneration	
Criterion for recruitment	expertise	knowledge
Type of stratification	commercial classes and status groups	universal class
Mode of activity	technical application of expertise	"subsumption"
The ruled	citizens	
Relation of the partial orders	functional differentiation; state administration	
Economic basis	market economy	civil society

Note: The criteria, except "mode of activity," are adopted from Schluchter 1981, 120. Schluchter is utilized on account of his systematic arrangements. For a direct comparison between Hegel and Weber, see Jackson 1986, 148–49.

comes an institutionalized organization for dealing with public affairs. Its structural functions are independent from its historical origin.[5] Based on this methodological view, Weber constructs the ideal type of legal–rational authority with its bureaucratic apparatus in contrast to the ideal type of traditional authority, especially patrimonialism (Schluchter 1981, 118–21). Hegel's theory of bureaucracy is in the same vein (*Right*, par. 236, rem.). The administrative staff in a patrimonial regime depends on the personal will of the ruler, while bureaucracy in the modern state is autonomous from the ruler's arbitrary will and regulated by objective laws. The modern constitutional state comes into being as civil society separates from the political state and as the governing norms, as well as organizations, become impersonalized. The modern state, radically different from any traditional type of political organization, provides the foundation of the liberal, rights-based concept of politics (par. 273, rem.; Weber 1978, 642). Within the framework of rule of law (in the case of Hegel) or formal legality (in the case of Weber), the traditional concept of privileges is transformed into the modern concept of rights:

> Only within the bureaucratization of the state and of law in general can one see a definite possibility of a sharp conceptual separation of an "objective" legal order from the "subjective" rights of the individual which it guarantees. . . . These distinctions presuppose the conceptual separation of the "state," as an abstract bearer of rights of domination and the creator of legal norms, from all personal authority of individuals. *These conceptual distinctions are necessarily remote from pre-bureaucratic, especially from patrimonial and feudal, structures of authority.* (Weber 1978, 998; emphasis mine)

Notwithstanding that bureaucracy has its own shortcomings, Hegel and Weber agree that it should be treated as a new political phenomenon. Simply applying the traditional category such as despotism in this context is not appropriate.

THE MODE OF BUREAUCRATIC ACTIVITIES

Although Hegel and Weber agree about the organizational characteristics of bureaucracy and its modernity, they disagree about the nature of bureaucratic activity. For Hegel, it is based on knowledge; for Weber, it is mere technical application of expertise. This difference is even more important than their concord because it has great bearing on the current debates in social theory.

The opposition between Weber and Hegel can be traced to the Aristotelian distinction between craft (*techne*) and practical wisdom (*phronesis*). This distinction has been revitalized recently (Arendt 1959; Gadamer 1983; Habermas 1973; MacIntyre 1984; Oakeshott 1975). As recapitulated by Steven Smith, Aristotelian *techne* is "the orderly, methodical application of intelligence to some problems for the sake of gaining control over future contingencies" (1989a, 137). It is universal, instrumental, and teachable (p. 138). Practical wisdom, according to Aristotle, is "the capacity of deliberating well about what is good and advantageous for oneself regarded as typical of a mon of practical wisdom, . . . [about] what sort of things contribute to the good life in general" (*Ethics* 1140a, [1962]). Practical wisdom is experiential, in-

trinsic (i.e., noninstrumental), and contextual (Smith 1989a, 138–39). The pertinence of this distinction for understanding modern politics has been explored by Arendt and numerous other authors. As to its implication for the theory of bureaucracy, the dominant view is that bureaucracy is a teleological organization based on *techne*.

The dominant view is based on Weber's analysis. Weber emphasizes the definitive importance of technical expertise for bureaucracy: "The management of the office follows *general rules*, which are more or less stable, more or less exhaustive, and which can be learned. Knowledge of these rules represents a special technical expertise which the officials possess. It involves jurisprudence, administrative or business management" (Weber 1978, 958; emphasis original). Weber's specifications of the bureaucratic rules—stable, exhaustive, and learnable—indicate that they belong to the realm of *techne*.

The modality of "application" is dealt with in Weber's general theory of social action. He distinguishes four types of human action: purposive-rational or instrumental-rational (*zweckrational*); value-rational (*wertrational*); affectual; and traditional. For Weber, technical application belongs to purposive-rational action. This type of action involves calculating and choosing the appropriate means to achieve a given end. It cannot determine the end of the action except by making pragmatic judgments whether a specific end is achievable (Weber 1978, 24–26). The distinction between purposive rationality and value rationality parallels Weber's distinction between the ethic of responsibility and the ethic of conviction. As a neo-Kantian, Weber separates value from fact and adopts the position of "ethical polytheism," postulating that political values cannot be rationally determined but can only be acquired through an existential decision (1947, 152). Politicians settle political value in the battleground of political struggle. Bureaucracy, by contrast, is a "neutral agent," whose task is to execute, with technical precision and impartiality, the policies that emerge from that struggle. Politics and administration (end and means, value and fact) are separated radically in this perspective (p. 95). For Weber, the administrative activity of bureaucrats is *techne*: they can deliberate only about the means; their concerns are only expediency and technical feasibility.

Weber's interpretation of bureaucratic activity is adopted by most political theorists and social scientists, even though their philosophical backgrounds may differ widely. For example, Arendt views bureaucratization and the modern social sphere as an extended form of household management (1959, 29). Oakeshott considers bureaucracy as an administrative apparatus correlated with the idea of "lordship," according to which the modern state is an economic enterprise (1975, 268). Habermas regards bureaucratization as the main symptom of modern "scientization of politics" (1970, 62) or "mediatization of the life-world" (1984–89, vol. 2, 186). Hegel's theory of the executive power has also been interpreted in this way. Foster draws a parallel between the guardians in Plato's *Republic* and Hegel's "universal class" and suggests that the actions of both classes are *techne* (1935, 143, 182). Riedel characterizes bureaucratic activities as mere administration, thus tacitly accepting the *techne* model (1984, 152).

In my interpretation, however, Hegel's understanding of bureaucratic activity is entirely different from this dominant view. It constitutes Hegel's original contribution to a liberal theory of bureaucracy and is a creative transformation of classical political philosophy.

Hegel depicts the activity of civil officials as subsumption, a concept whose meaning needs to be scrutinized.[6] He employs this term in only two contexts in the *Philosophy of Right*: bureaucratic activity and adjudication (*Right*, pars. 225–28).[7] He specifies the operation of subsumption in detail when explaining the judiciary:

> The task of the court is the application of the law to a *single case*, and the work of judgment has *two aspects*: *first*, knowledge [*Erkenntniss*] of the nature of the case as an *immediate, single, occurrence* . . . and, in *criminal* cases, reflection to determine the *substantial*, criminal, character of the deeds; *secondly*, the subsumption of the case under the *law* so that right be restored. (par. 225; emphasis original; trans. based on Knox's).

The process of subsumption has three characteristics: it is an activity based on knowledge,[8] it mediates between universal legal norms and individual cases, and its objective is to fit norms into concrete situations so that right can be ascertained. Since Hegel draws a parallel between adjudication and execution, the question then becomes whether judicial judgment is a technical or a practical task—whether subsumption is a mere application (thus a kind of *techne*) or involves the use of practical judgement (thus a kind of *phronesis*).

Hegel takes the category of subsumption to be a familiar term in jurisprudence and does not explicate it further in his lectures (Hegel 1973–74, 1983). It is therefore difficult to determine to which type of action subsumption belongs. If compared with the dominant view of bureaucracy, however, it is notable that the realm of bureaucratic action is constituted by reference to the relation between universal norms and particular events. In other words, bureaucratic action is not merely problem solving or maximum realization of policy goals already set by politicians.

Contrary to the dominant view, hermeneutics addresses the issue of mediation between the universal and the particular from a different perspective. The philosophical dimension of the jurisprudence is formulated by Gadamer:

> What the law prescribes, what a case of a given law is, is only determined unequivocally in the eyes of a formalist who endangers life. Finding the law means thinking the case together with the law so that what is actually just or the law gets concretized. For this reason the body of precedents (the decision already laid down) is more crucial for the legal system than the universal laws in accord with which the decisions are made. This is correct

> insofar as the meaning of any universal, of any norm, is only justified and determined in and through its concretization. (1983, 82).[9]

He highlights that "concretization of norms" is the crucial element in the adjudication process. Similarly, Hegel draws a parallel between the judiciary and executive in that they have the common modality of concretization. Subsumption, like concretization or "thinking the case together with the law," is a dialectical process in which the universal and the particular encounter each other and become related by means of human deliberation. It requires a hermeneutics of the concrete, an ability to absorb sufficient contextual knowledge and relevant legal norms. It is a standard case of hermeneutic circle. The whole (norms) and part (occurrences) cannot be related except by the practical judgment of a prudent human being (*spoudaios*, in the sense of Aristotle, *Ethics* 1113a).[10] Since Gadamer proposes to reformulate Aristotelian practical philosophy through hermeneutics, his theory of hermeneutic circle is a fine reference framework for interpreting Hegel's notion of subsumption. For the sake of comparison, the competing positions suggested by Plato and Weber need to be addressed.

For practical philosophers, the Weberian dichotomy of value-rational and purposive-rational actions has shortcomings similar to those of Plato's idea of *techne*. The Platonic *techne* assumes that it is possible to realize a given scheme without changing the scheme itself in the process (Foster 1935, 18). Plato's theory is untenable because it presupposes an Archimedean point transcending the phenomenal world, by which the current order of things can be radically reformulated. The philosopher–king is the *demiourgon* (supreme craftsman) in the human world. As Arendt points out, the rule of the philosopher–king is, in fact, one-man rule in the modality of tyranny (1959, 197–203).

The Weberian politics/administration distinction does not deal adequately with bureaucratic activities either, because no set of rules could really foresee all possible occurrences. Since Weber does not admit any creativity in bureaucratic action, those decisions not stipulated by general rules can only be designated negatively as "discretion" (Weber 1978, 979). This approach results in a paradox: bureaucracy is supposed to implement political decisions with technical precision, but this ideal can never be fully realized because bureaucrats would always have some space of discretion. As a result, Weber resorts to the dubious charismatic–plebiscitary democracy to ensure the "autonomy" of political leadership; but the danger of such leadership for a liberal–constitutional regime is obvious (Mommsen 1984, 390).

Compared with the positions of Plato and Weber, practical philosophy (adopted by Aristotle, Hegel, and Gadamer) grasps political reality more adequately. For practical philosophers, there are no norms that would not themselves be modified in the process of concretization. By the same token, sociopolitical reality cannot be known apart from its normative dimension. Norms and occurrences interact continuously to form social practices (MacIntyre 1984, 187). This line of consideration, which emphasizes the immanent, instead of transcendent, nature of political action, make it easier to comprehend and appreciate the category of concretization proposed by practical philosophers. Political action is not foundational but contextual. Practices constitute the boundary of human deliberation and action. Bureaucrats, like judges, can work only under norms and use their practical judgement to arrive at the best results within the framework of laws. By placing it in the category of subsumption, Hegel indicates that bureaucratic activity is *phronesis*, not *techne*. He "judicializes" the exercise of executive power: both the judiciary and executive concretize legal norms in particular situations.[11] They are institutionalized *phronesis* in the modern state.[12]

Hegel specifies the form of practical judgment not in the *Philosophy of Right* but in the *Science of Logic*. He defines judgement as the connection between universal and particular in the concrete form of a proposition. Two aspects of his theory are relevant here. First, the attributes of the abstract universality can be recognized only through the faculty of judgment,[13] in which the universal and the particular become subject and predicate so that their relation is illuminated (*Logic*, par. 170 [1975]). Second, the attributes of the abstract universality can be specified only through a continual process of making judgment. These two aspects bear notable similarity to the category of subsumption. The second aspect is especially pertinent. It shows that judgment and subsumption are not static but are in a process of development, since the universal always has potential to realize itself in different forms while circumstances of the particular are always changing (par. 161). In a constitutional state, where norms are ultimate, institutional agents for concretizing norms are therefore indispensable. The process of subsumption coexists with the normal activities of the state.

The interpretation suggested here is supported by Hegel's discussion of the parallel bases of the judiciary and bureaucracy. Aristotle points out that since practical wisdom exercised in concrete situations depends on judgment of the agent concerning contextual circumstances, it is not possible for practical science to attain the same degree of certainty as theoretical science (*Ethics* 1094b). This inherently conditional precision poses a predicament for the judiciary and bureaucracy as institutionalized *phronesis*. If the involved parties contest this uncertainty, these institutions can no longer execute their tasks adequately. In the case of the judiciary, since judgment is necessary for the judges and jury to evaluate concrete situations, trust (*Zutrauen*) about their rectitude is the foundation of the judicial system (*Right*, par. 228).[14] The modern concept of civil rights, especially in countries with a common law judiciary, presupposes that citizens have a shared belief in the institutional arrangements for adjudication.[15]

The principle of trust has its parallel in the state, namely, *political sentiment*, or patriotism, which Hegel defines as "the consciousness that my interest, both substantive and particular, is contained and preserved in another's (i.e., in the state's) interest and end, i.e., in the other's relation to me as an individual" (*Right*, par. 268). One might conclude that Hegel is proposing authoritarianism here in linking individual ends with those of the state. However, if this theory of political sentiment is compared with his idea about trust as the underpinning of the judiciary, it is clear that political sentiment is also a form of shared belief that is essential for a political institution performing practical deliberation. Bureaucracy, like the judicial system, would collapse if the element of trust were not presented as its constituent basis. Hegel's concepts of trust and political sentiment can be best understood as the Weberian notion of legitimacy—the indispensable belief that the claims of authority has normative validity (Weber 1978, 213, 915).

An important issue arises here: How can this belief be justified or grounded theoretically? The *techne* model of bureaucracy has the seeming advantage of explaining the legitimacy of bureaucratic domination simply by technical expertise. But Weber's concept of legality is a circular argument, as Habermas has shown (1984–89, vol. 1, 265). In the interpretation proposed here, practical wisdom constitutes the crucial qualification for bureaucrats to rule. This idea is neither an elitist idea of governing nor self-consecration by the bureaucrats, as Marx insists it is (1975, 115). The ground of bureaucrats' capacity for practical judgement lies rather in the specific form of institutionalization of the modern state and the effects this form has on the officeholders. Contrary to Habermas' contention (1984–89, vol. 2, 306), the separation of public offices from their incumbents makes the cognitive orientation toward others more feasible than in any other political institutions because political resources are no longer viewed as a ruler's private property or privileges. Although every rational human being is endowed with the capacity to make moral judgment, self-interest is also a contrary compulsion with binding force. While modern economic society materialize the idea of self-interest, the modern state institutionalizes the impersonal aspect of sociopolitical relations.[16] In the modern bureaucratic organization, the officeholders must deliberate state affairs not from the egoistic perspective (prudential judgment) but from the universal-normative perspective (moral judgment). (See Manzer 1984, 579–80.) This distinction between the moral judgment of state officials and the prudential judgment of the members in civil society corresponds to the Hegelian distinction between universality and particularity and constitutes the ground of the legitimacy of modern bureaucratic rule.[17] In his theoretical sense, modern bureaucracy and judiciary could be viewed as institutionalized *phronesis*.

SOME ISSUES IN HEGEL'S POLITICAL THOUGHT

The interpretation of bureaucratic activity as *phronesis* can solve some difficulties in Hegel's peculiar theory concerning the division of constitutional powers. He opposes the concept of checks and balances (*Right*, par. 272, rem.) and proposes a constitutional division of powers into monarch, executive, and legislature. One point is noticeable: Hegel makes the judiciary part of the executive. In the interpretation advanced here, this attribution does not signify that he rejects liberalism or constitutionalism. The reason is rather that since Hegel views the two jurisdictions as endowed with the same principle, he merges them in his theory of the state.

Judges and state officials are engaged in the similar mode of activity, but they work within different domains. The universal for the judges consists of private (civil and criminal) laws, the particular of individual interactions in civil society. Judges maintain rectitude by implementing civil and criminal laws. By contrast, the universal for state officials is public laws, the particular the external effects of private pursuits in civil society: "The *maintenance* of the *state's universal interest* and of *legal norms* in this sphere of particular rights, and the work of bringing these rights back to the universal, have to be superintended by holders of the executive power." (*Right*, par. 289; emphasis original; trans. based on Knox's) Although the distinction between public and private laws is more notable in the continental legal tradition than in the common law tradition, the jurisdictions of state officials delineated by Hegel still have relevance for contemporary liberal regimes. Governmental intervention is needed for pursuing undertakings of common good (par. 235), adjusting the competing interests of producers and consumers (par. 236), and supervising education crucial for training individuals to earn an independent living (pars. 237, 239).[18] Hegel's theory of the state is close to modern welfare economics, because the functions mentioned concern matters that economists (Lindblom 1977, 78–85; North 1981, 20–44) designate as needing governmental intervention: either as externalities (contending interests of producers and consumers) or as public goods (undertakings of common good and education).

These roles indicate that for Hegel, bureaucracy is *not* a teleological organization with an externally imposed end to implement. Since bureaucrats perform their function under a framework of legal norms, bureaucratic organization is regulative (i.e., it concretizes legal norms), rather than teleological in the strong sense of being related to certain substantive goals.[19] Hegel's outline of the role of bureaucracy in the modern state should mitigate much liberal suspicion about it.[20]

The task of bureaucracy, then, is to preserve the legal norms of the state. Heretofore, the political communities had been maintained by tradition, cus-

toms, and tacit shared understanding. With the growth of modern civil society, the private sphere, and the secularization process, these traditional mechanisms become less effective. Consequently, Hegel designs an institutionalized agent that can conserve the integrity of the political community. Only through the continuous subsuming activities of bureaucracy could the constitutional state sustain itself in the face of the disruptive forces of civil society. State officials constitute a "universal class," not because they are endowed with some metaphysical knowledge of universal character like Plato's philosopher–king or Marx's proletariat but because they actualize universal constitutional norms.

Hegel's theory of bureaucratic activity, formulated by the categories of practical philosophy, is based on his evaluation of modern civil society. The quintessence of his theory is that bureaucracy is a new means of sustaining the constitution. Put in a philosophic way, bureaucracy embodies the principle of individualization of abstract legal norms in the modern constitutional state. Hegel's theory is not an anachronism, as Ilting suggests (in Pelczynski ed. 1971, 99), but a well-founded effort to overcome the destructive potential of modern subjective freedom vividly portrayed in his *Phenomenology of Spirit*. Hegel's effort is more realistic and prudent than other diagnoses of the predicaments of modernity, especially the aporia of the critique-of-instrumental-reason thesis.[21]

Hegel's liberal theory of bureaucracy overcomes the traditional dichotomy of politics and administration. Its ideal, *Rechtsstaat* (the state based on laws) is a constitutionalist concept of politics—supremacy of the principle of the rule of law. But unlike neo-Kantian liberals, Hegel endowed liberalism with a historicist, or contextual, dimension. Based on the idea of freedom realized through history, liberal constitutionalism is a politics of immanent reform of current practices (Smith 1989c). Norms and circumstances are in an unending process of mutual adjustment because present practices are continuously modified within the framework of norms. It is politics of mediation between civil society and the state (which are relatively autonomous toward each other) through the practical judgement of state officials' handling situations in which norms are challenged or in need of new interpretations. Hegel's idea of politics bears resemblance to the civil association described by Oakeshott: "The idea of *civitas*, not itself an enterprise, . . . [is] an association of *civis*; a relation of equals, and a self-sufficient condition in being relationship in terms of the conditions of a practice which are not used up in being used and not in terms of a substantive purpose or purposes to be pursued" (1975, 183). Bureaucracy in the sense specified by Hegel is *compatible* with the basic principle of constitutional liberalism. The task of modern bureaucracy —to realize the political norms in concrete situations and to subsume the latter under the universal norms—is a type of "ruling," not mere management. Moreover, for Hegel, bureaucracy is *indispensable* for the modern constitutional state. This difference between Hegel and orthodox constitutionalists like Oakeshott results from their different evaluations of modern civil society. For the latter, civil society with the rule of law is enough to form *societas*. For Hegel, in the modern constitutional state, an institutionalized agent is needed to ascertain the normative validity of constitutional norms in view of the disruptive potential of civil society. Only from this perspective can one explain why Hegel embraces modern bureaucracy, given his liberal position concerning individual rights and the rule of law.

CONCLUSION

Bureaucracy has become an indispensable governing organization in the modern state. For most liberals and constitutionalists, it may be irresistible but suspect. Hegel's theory can contribute to a liberal reinterpretation of modern bureaucracy. His ideas can be summarized into three theses. First, the rationale of bureaucracy is the division between modern economic society and the constitutional state. Second, bureaucracy is an impersonal organization in which the separation between office and its incumbent is essential. Third, the mode of bureaucratic activities is mediation between constitutional norms and concrete situations, subsuming the particular into the universal and concretizing universal norms. These three are necessary conditions of modern bureaucracy.

In organizational characterizations, Hegel's model is close to Weber's ideal type of bureaucracy but lacks Weber's antiliberal preference for plebiscitary democracy. Hegel's interpretation of bureaucratic activity as *phronesis* provides an alternative set of normative theses to the Weberian approach.[22]

Hegel's theory of bureaucracy is compatible with the liberal and constitutionalist concept of politics. In the twentieth century, bureaucracy and the scope of civil liberties have simultaneously expanded in the liberal states. This historical development illustrates that bureaucracy and liberalism do not contradict each other in essence. By contrast, the recent breakdown of socialist regimes shows that without an autonomous civil society, there is no *modern* bureaucracy, but only traditional bureaucratic empire or despotism. In both respects, Hegel's conceptualization is more appropriate than Tocqueville's or Marx's.

But Hegel is first and foremost a philosopher. His Aristotelian reconciliation between constitutionalism and bureaucracy shows that the classical political philosophy can be synthesized with the liberal rights-based problematic to gain fruitful results.

Notes

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1. This work will be cited as *Right*, with paragraph numbers. Rem. refers to Hegel's remarks; add. refers to the additions edited by Edward Gans. Hegel's emphases, omitted by Knox, are restored.

2. I thank Rogan Kersh for drawing my attention to Jackson's article.

3. It is symptomatic that no articles in Pelczynski 1971, 1984; Stepelevich and Lamb 1983; Verene 1980—all anthologies on Hegel's political philosophy—deal with his theory of bureaucracy. Bernard Cullen states explicitly that Hegel's theory of the rational state is an "unsuccessful attempt" (1979, 115).

4. The ethical dimension is viewed by Jackson as the most original aspect of Hegel's theory of bureaucracy (1986, 150–53).

5. This methodological distinction plays a prominent role in Weber's interpretation of the rise of capitalism. The innerworldly asceticism of Calvinism has an elective affinity with the capitalistic spirit of rational calculation. However, once capitalism becomes a social reality, its origin is no longer relevant to its operation.

6. Marx notices the importance of this category but insists that "Hegel does not inquire whether this mode of subsumption is adequate or rational" (1975, 109). This verdict is incorrect, as will be shown.

7. To be exact, the term *subsumption* occurs four times in the *Philosophy of Right* (pars. 225, 228, 283, 287).

8. Knox obscures the original meaning by rendering *Erkenntniss* as "ascertainment."

9. For a fuller treatment of legal hermeneutics see Gadamer 1975, 274–305.

10. There is nevertheless an important difference between Aristotle and Hegel. For Aristotle, the moral capacity of the *spoudaios* is derived from embracing the cosmic or divine order (Voegelin 1989, 68). The *spoudaios* is therefore above the laws. Aristotle's view represents the typical Greek attitude, which regards the rule of the wise person the best form of governing. For Hegel, the prudence of modern bureaucrats originates from the specific institutionalization of the modern state (separation between offices and incumbents). Due to this difference between Aristotle and Hegel, it is more to my purpose to use Gadamer's "historicized Aristotelianism." It should be noted that Gadamer's view is not without textual evidence in the Aristotelian corpus; and it is closer to Aristotle's idea of justice (*Ethics* 1134a).

11. That many executive decisions are endowed with judicial nature has been noticed by scholars of constitutional law (Friedrich 1968, 115).

12. Manzer (1984) undertakes to incorporate the Aristotelian practical judgment into the theoretical framework of policy analysis. He demonstrates that two dominant models in public policy literature—elite–planning and pluralist–exchange models—are insufficient for analyzing bureaucratic behavior. The category of practical reasoning can extend the sequential dimension and typological sophistication of the current research. Manzer highlights the judicial policymaking and commission policymaking (concerning education and welfare) as especially embodying the moral dimension of practical reasoning.

13. As Hegel (*Logic*, par. 166) points out, the etymology of the German word *Urteil* (judgment) means "original partition or distinction."

14. *Zutrauen* is rendered "confidence" in this paragraph but "trust" in par. 268. Knox thus misses the parallel drawn by Hegel.

15. Avineri has pointed out that many Hegelian concepts are developed from English practice (1972, 192).

16. Hegel's theory of the separation between civil society and the state is strongly opposed by Marx (1975, 221, 228–34). For Marx, the universality of the modern constitutional state is merely a guise for veiling exploitation. He proposes that abolishing the differentiation between society and the state is the main task for human emancipation. The historical experience of socialism shows that the separated civil society and constitutional state functions better than the unified totalitarian state, whether considered in terms of efficiency or equity. The theoretical grounds for the plurality of human associations have been tackled by Aristotle (*Politics* 1261a, 1263b [1984]) in his criticisms of the Platonic idea of a unified polis.

17. A related issue—how the belief in legitimacy is generated—is an empirical one and may vary according to specific circumstances of every state.

18. These are jurisdictions of the "police," through which the executive power functions in the civil society.

19. The term *regulative* is borrowed from Rawls (1971, 528). It means that the "larger plan" of the community should realize individual plans but that the only legitimate larger plan is justice, rather than any dominant end. The rule of law plays this regulative role in Hegel's system. Bureaucrats cannot transgress this limit. Cf. Weber 1978, 51.

20. Teleology is the underlying reason for liberals and libertarians to suspect bureaucracy. See Hayek for the distinction between "nomocratic" and "telocratic" orders (1967, 163).

21. The aporia consists in this thesis' total rejection of both modernity and the romanticist reaction. The only direction left is utopian anticipation (Benhabib 1986, 163–82).

22. They may contribute to the task proposed by Luhmann, namely, to transcend Weber's premises so as to get a critical standpoint for examining the Weberian paradigm (1982, 21).

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BUSY VOTERS, AGENDA CONTROL, AND THE POWER OF INFORMATION

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The correspondence between individual preferences and electoral outcomes is often affected by the existence of information asymmetries among electoral participants and the presence of individuals or groups who exercise some form of agenda control. While the effects of agenda control in political decision making are widely recognized, the effects of information asymmetries are not as well understood. Since information asymmetries are fundamental characteristics of most elections, a deep understanding of the correspondence between individual preferences and electoral outcomes requires a serious consideration of the "effects" of information. I develop a generalizable agenda control model that takes as given the observation that most voters are not naturally inclined to invest in political information. The model allows me to provide a dynamic description of how voters and political elites can adapt to the information problems that characterize political decision making. It also allows me to demonstrate the effect of these adaptations on electoral outcomes.

I develop a spatial election model that demonstrates the effect of information asymmetries (the fact that some electoral participants have more accurate information than others) on electoral outcomes. The original motivation for the development of this model came from the debate about the use of the initiative and referendum in the determination of public policy.[1] My intent is to advance this debate by identifying conditions under which voters are able to use these electoral institutions in order to enhance the "majority will" and conditions under which these institutions are likely to serve as effective policy-influencing tools for well-organized minority interests that obtain agenda control. In developing the model, I introduce assumptions that follow quite naturally from a study of these institutions (costly ballot access, the existence of credible endorsements, the assumption that voters know more about the status quo than a newly proposed alternative, and the assumption that voters are uncertain about the agenda-setter's preferences). In addition, the wide use of the initiative and referendum provides many opportunities for empirical tests of the model's predictions.

I define the *direct legislation environment* to be a one-shot interaction where a completely informed monopoly agenda-setter proposes one "alternative" to an existing status quo. Incompletely informed voters (direct legislators) choose one of two actions, *vote for the status quo* or *vote for the alternative*. Majority (or supermajority) rule determines a policy outcome. Both the agenda-setter and the voters have preferences over outcomes.

The binary nature of the choice offered to voters in direct legislation environments ensures that the electoral outcome is the "stated preference" of a majority of voters. However, when voters possess incomplete information about the content of the electoral alternatives, and are therefore uncertain about the relationship between their preferences and possible electoral outcomes, the relationship between the majority's "underlying preferences" and the majority's "stated preferences" may no longer be straightforward or obvious. For instance, when voters possess incomplete information, they may cast a different vote than they would have cast if they had possessed better (or complete) information. When information affects voting behavior in this way, the electoral outcome may not be same as the outcome that a better informed electorate would have chosen. Since voters are rarely, if ever, completely informed, I think that it is important to consider the use of different information conditions when approaching the study of elections.

I use the model to identify the effects of information asymmetries and communication opportunities on the correspondence between individual policy preferences and electoral outcomes in the direct legislation environment. I show conditions under which monopoly agenda control and *any* degree of voter uncertainty force direct legislation voters into having to choose between the status quo and the agenda-setter's most preferred policy. (This also implies that there exist preference profiles for which *any* policy can be reached in one vote.) Under these conditions, neither the status quo nor the setter's most preferred policy need be closely related to the median voter's (or any other voter's) preferences. I also show that the electoral winner between these two alternatives need not be the outcome that a completely informed electorate would choose.

While the lack of dependence between electoral outcomes and voter preferences, identified herein, may trouble students and supporters of democratic institutions, there is a way that a majority of voters can more effectively influence direct legislation outcomes. One of my objectives is to demonstrate that the observation, "Most voters are not naturally inclined to invest in information about politics," does not necessarily lead to the conclusion that voters are completely uninformed or that such voters will behave differently than they would have if they had more accurate information. I show conditions under which the presence of groups or individuals who are

willing to undertake costly actions in an attempt to influence electoral outcomes provide incompletely informed voters with low-cost information cues. These cues allow voters to make more accurate inferences about the electoral alternatives, which increases the likelihood that they cast the same vote they would have cast if they had acquired more accurate information. In other words, I show how both self-interested voters and political elites can adapt to the information problems that characterize political decision making. Voters can adapt by using certain types of low-cost information cues to make more accurate inferences. If the agenda-setter's actions affect (do not affect) the content of the cues that will be available to voters, then the agenda-setter can adapt by giving a greater (lesser) consideration to voter preferences when choosing to take an action.

The design of the model is sufficiently generalizable to allow the results to address controversies associated with other decision-making institutions. For example, this model can provide greater insight into the actions of Denzau and Mackay's (1983) gatekeeper (a committee or an individual who has the opportunity to present a bill to a legislative body under a "closed rule") and Niskanen's (1971) bureaucrat (an individual within a bureaucratic agency who possesses better information than legislative counterparts and can offer all-or-nothing packages of bureaucratic services). I also believe that the communication apparatus incorporated into this model can be transferred to a wider range of decision-making models than I discussed.

Next, I discuss the foundations of this spatial model and establish preliminary results. I then derive electoral equilibria that allow me to demonstrate how information asymmetries and communication opportunities affect player strategies and electoral outcomes. A concluding section summarizes the findings.

FOUNDATIONS OF THE MODEL

To study the effect of information on the correspondence between individual policy preferences and electoral outcomes, I develop a model whose predominant features are *monopoly agenda control, incompletely informed voters, and communication opportunities.* Before embarking on the model's development, I briefly review the origins of the model's predominant features.

The Relation of This Model to Previous Work

The effect of introducing a monopoly agenda-setter to a spatial election model was first detailed in the "setter model" of Romer and Rosenthal (1978, 1979). Romer–Rosenthal is a complete information model where voters know the exact location of the status quo, the alternative proposed by the agenda-setter, and the agenda-setter's preferences. The agenda-setter has the opportunity to make one take-it-or-leave-it offer to the voters. In equilibrium, the setter makes an offer that maximizes setter payoff subject to the constraint that the median voter prefer the offer to the preexisting status quo. The Romer–Rosenthal model demonstrates how the presence of a monopoly agenda-setter can produce a difference between the median voter's most preferred policy outcome and the electoral outcome.[2]

The decision makers in my model are an electorate of incompletely informed voters. It is especially true in large electorates that voters who consider their opportunity costs may discover that they have little incentive to acquire detailed information (Downs 1957; Popkin 1991; Popkin et al. 1976). However, uninformed voters do not necessarily cast different votes than they would have cast if they had possessed better (or complete) information. McKelvey and Ordeshook's (1985, 1986) spatial two-candidate incomplete information models provide examples of how incompletely informed voters can use low-cost information sources to cast the same votes they would have cast if they were completely informed.[3]

The development of an incomplete information agenda control model may bring to mind the models of Denzau and Mackay (1983) and Banks (1990). In the former, the setter has incomplete information about voter preferences, and the voters are completely informed. Denzau and Mackay find equilibria for a number of different behavioral assumptions. Banks's model is more closely related to the model I am developing for it seeks to analyze the effects of voter uncertainty. A comparison of the fundamental assumptions underlying the Romer–Rosenthal, Denzau–Mackay, Banks, and my own model are presented in Table 1.

While Banks's assumptions provide the foundation for a reasonable model of some decision-making institutions, they are not as useful for the analysis of many others, like ballot initiatives or referenda. For instance, my model assumes that voters know more about the status quo than about the alternative; Banks assumes the opposite. The assumption that voters know more about the status quo is better suited for models of voting on constitutions, zoning ordinances, or taxation rules (to name a few). In these circumstances, voters must choose between status quo policies, (whose effects they might be able to observe) and alternative policies (whose effects cannot be observed until they are implemented). My model also assumes that voters can be uncertain about the agenda-setter's preferences; Banks assumes that voters know the agenda-setter's preferences. Banks's assumption requires that voters know exactly what trade-offs a setter is willing to make under any possible circumstance. The assumption that voters are uncertain about the agenda-setters preferences is much less restrictive and thus more broadly applicable to the study of political decision making. A third difference between the models is the assumptions made about communication between the agenda-setter and the voters. Signaling opportunities exist in both models, but the structure and generalizability of

TABLE 1.

Comparison of Fundamental Setter Model Assumptions

CHARACTERISTIC	ROMER AND ROSENTHAL 1978	DENZAU AND MACKAY 1983	BANKS 1990	LUPIA
Single-shot game	Yes	Yes	Yes	Yes
Single-peaked voter preferences	Yes	Yes	Yes	Yes
Perfectly informed setter	Yes	No	Yes	Yes
Voters always know				
Location of status quo	Yes	Yes	No	Yes
Setter's Preferences	Yes	Yes	Yes	No
Location of Alternative	Yes	Yes	Yes	No

the interactions are very different. Banks's voters make inferences about the status quo from the setter's choice of alternative. My voters draw information from beliefs about the setter's preferences, the observation that an election takes place, and the presence of credible endorsements. The communication opportunities in my model are more dynamic and thus more appropriate for the class of examples I am interested in.

Definition of the Model

I model the *direct legislation environment* as a one-period, multistage game of incomplete information. The object of the game is to choose one policy from a finite continuum of possible policy alternatives. One completely informed monopoly agenda-setter (*the setter*) can propose one alternative to a common knowledge status quo. The setter's willingness to propose an alternative may be affected by the cost of contesting the election. If the setter decides that proposing an alternative is not worthwhile, the game ends and the status quo is the outcome. If the setter decides to propose an alternative, voters must vote for either the status quo (about which they are completely informed) or the setter's proposed alternative (about which they possess incomplete information). Some of the setter's actions may provide additional information to voters about the alternative. All players have policy preferences, and majority (or supermajority) rule determines the outcome of the election.

In order to identify the effects of information in the direct legislation environment, I solve for equilibria in three substantively relevant game types and one control case. In the control case, the setter and the voters do not communicate. In the other game types, distinct forms of communication are available. Since the only difference between a game type and the control case is the presence of a specific form of communication, a comparison of equilibrium behavior in the control case to equilibrium behavior in the game types allows me to identify the effect of these communication opportunities on voting behavior and electoral outcomes. All players know the game type (what communication opportunities will be available) with certainty when it is their turn to choose a strategy. While the substantive motivation for each game type is provided later, the game types are briefly listed here to facilitate a compact description of the model.

Control case. No information is transmitted between potential information providers and voters.
Costly entry. Voters observe setter's decision to pay a price to contest the election.
Endorsement. Voters observe a perfectly reliable endorsement.
Learn setter's ideal point. Voters learn the setter's ideal point.

Consider the policy space $[0, 1]$.[4] There exists a common knowledge status quo $SQ \in [0, 1]$. It is common knowledge that the game is being played by $n + 1$ players. n of the players ($N = \{1, \ldots, n\}$, where $n > 1$) are called voters and one other player is called the setter.

Each player's preferences over the policy space can be distinguished by the location of his or her ideal point. The setter's ideal point, $X \in [0, 1]$, is drawn from the cumulative distribution function F, which has density f. Each voter's ideal point, $T_i \in [0, 1]$, is drawn from the cumulative distribution function G, which has density g. All players know their own ideal points and the distribution from which any other player's ideal point is drawn. For example, the distribution F represents the prior beliefs that voters have about the setter's ideal point.[5] In short, no voter knows any other voter's ideal point, whether voters know the setter's ideal point is modeled as a variable, and the setter has complete information.[6]

After player ideal points are determined, the setter chooses a strategy. The setter's strategy, $s(X)$, has two components. The first strategic decision is whether to contest the election. In this model, the decision to contest the election is nontrivial because the setter who decides to contest will face a nonnegative, common knowledge *cost of entry*, $K \in \Re^+$. I define the *setter's entry decision* as $s_1(X) \in \{0, 1\}$, which equals 1 if the setter decides to contest the election, 0 otherwise. If the setter decides not to contest the election, the game ends and SQ is the outcome. Otherwise, the setter enters and chooses the second component of the strategy (a location for [the exact

TABLE 2.

Comparison of Endorsement Content

GAME TYPE	CONTENT	VALUE: $M_2(S_2)$ =
Control case	no endorsement	0
Costly entry	no endorsement	0
Endorsement	s_2 is to the left of SQ	−1
	s_2 is the same as SQ	0
	s_2 is to the right of SQ	1
Learn setter's ideal point	setter's ideal point	$X \in [0, 1]$

content of] the "alternative to the status quo")—$s_2(X) \in [0, 1]$.[7]

The setter's strategy set is $S_0 = \{s|$ where s: $[0, 1] \rightarrow \{0, 1\} \times [0, 1]\}$, where the particular strategy chosen by the setter takes the form: $s(X) = (s_1(X), s_2(X))$. For notational simplicity, I denote $s(X)$ as s, $s_1(X)$ as s_1, and $s_2(X)$ as s_2. It follows that when an election is held it is of the form: SQ versus s_2.

After the setter moves, the voters choose a strategy (cast a vote). All actions taken, and all information obtained, by voters are assumed to be costless to them. A voter's strategy is a binary decision, $v_i = \{-1, 1\}$, where $v_i = -1$ represents a vote for SQ and $v_i = 1$ represents a vote for s_2.

In each game type, voters can condition their choice of strategy on a piece of information provided to them about the setter's strategy. Voters observe the message $M(s, X) = (M_1(s_1), M_2(s_2))$. The value of the message is revealed to voters after the setter chooses a strategy but before the voters vote. The first component of the message allows voters to observe whether or not the election is being contested. For all game types, $M_1(s_1) = s_1$; that is, the voters directly observe whether the setter enters or not.

The second component of the message allows voters to observe an *endorsement*. The motivation for introducing the endorsement comes from the fact that voters (collective decision makers in general) may not have an incentive to acquire costly political information. These decision makers may have a greater incentive to use reliable low-cost information cues if they are available. Examples of such cues are the use of brand names in the purchase of consumer goods, induced party loyalty in legislatures (Huber 1990), the advice or statements of opinion leaders (Berelson, Lazarsfeld, and McPhee 1954), partisan identification (Campbell et al. 1960), "biased" information providers (Calvert 1985), particular events that take place during a campaign (Popkin 1991), and "the past" (Fiorina 1981). The "endorsement" provides voters with information about the location of s_2. In the model, it is common knowledge that the endorser possesses complete information and is a perfectly reliable source of information. The content of the endorsement in each game type is specified in Table 2 (the substantive motivation for each is provided later in the text).[8]

The particular strategy chosen by voter i takes the form $v_i(T_i M)$. The voter's strategy sets depend on the game type and are as follows.

Game Type — *Voter Strategy*

Control case S_i: $= \{v_i|$ where v_i: $[0, 1] \times \{0, 1\} \rightarrow \{-1, 1\}\}$

Costly entry S_i: $= \{v_i|$ where v_i: $[0, 1] \times \{0, 1\} \rightarrow \{-1, 1\}\}$

Endorsement S_i: $= \{v_i|$ where v_i: $[0, 1] \times \{0, 1\} \times \{-1, 0, 1\} \rightarrow -1, 1\}\}$

Learn setter's ideal point S_i: $= \{v_i|$ where v_i: $[0, 1] \times \{0, 1\} \times [0, 1] \rightarrow \{-1, 1\}\}$

When an election is held, the winning outcome is determined by majority rule.[9] The outcome function is, for any $s \in \{1\} \times [0, 1]$, and $v_i \in \{-1, 1\}$,

$$o(s, v_1, \ldots, v_n) = s_2 \text{ if } \sum_{i=1}^{n} v_i > 0, \text{ and}$$

$$= SQ \text{ if } \sum_{i=1}^{n} v_i \leq 0.$$

The outcome determines the payoffs to all players.[10]

I examine the case where all players have symmetric and single-peaked utility functions.[11] I define the *voter utility function* as: $U_i(x, T_i) = -(x - T_i)$. For voters, utility is solely a function of the distance between the location of the winning policy and their ideal point. I define the *setter policy utility function* to be $U_0(x, X) = -(x - X)$. The setter's policy utility is not affected by winning or losing the election except for the influence that the electoral outcome has on the position of the winning policy. Since the setter's incentives are also affected by the cost of entry, I define the setter's *utility function* as the setter's policy utility minus the cost of contesting the election: $\phi_0(s, v_1, \ldots, v_n|X, T_1, \ldots, T_n) = U_0(x, X) - [K \times s_1]$.

The Definition of Equilibrium Strategies

The equilibrium concept is constructed by starting with the Bayes–Nash concept and incorporating the assumptions of the model. (The formal statement and derivation are included as Appendix A.) I first incorporate the assumption that voters always vote as if they are the pivotal voter.[12] This strategy is weakly dominant with respect to the strategies of other voters. The incorporation of this assumption transforms the equilibrium concept into a variant of the *sequential equilibrium* concept of Kreps and Wilson (1982). I also incorporate the assumption, (essentially the same made by Kreps and Wilson) that voter beliefs are consistent. In short, players in this model choose strategies to maximize expected utility. These strategies are chosen with respect to the information and strategies of the other players. In equilibrium, each player's strategy is the best response possible to

the chosen strategies of others, given his or her information.

I now present two lemmas that help to characterize the setter's equilibrium strategies throughout the rest of the paper. Lemma 1 is an incentive compatibility condition that establishes that the setter contests an election if and only if the benefit from contesting the election is greater than the cost. Lemma 2 establishes an equilibrium location strategy for the setter. The proof of Lemma 2 is included as Appendix B.[13]

LEMMA 1. *The setter contests an election if and only if the benefit from contesting the election is greater than the cost.*

LEMMA 2. *A weakly dominant location strategy for the setter is* $s_2 = X$.

Lemma 2 shows that if voters are uncertain about the location of the alternative when it is time for them to vote, then a dominant strategy for the setter is to choose the (setter's) ideal point. In fact, the setter can, in equilibrium, choose any point for which $f > 0$; however, the ideal point provides the highest utility when s_2 is the electoral outcome. It follows that there exist profiles of voter preferences and beliefs that will enable *any policy in the space to be reached in a single vote.*

Contrast this finding to the agenda-setter's equilibrium location strategy in the (complete information) Romer–Rosenthal model, where the presence of completely informed voters forces the setter to propose an alternative that a majority of voters prefers to SQ in order to obtain a new outcome. This comparison shows how agenda-setter expectations about the level of voter information can affect the content of the electoral alternatives and suggests that the effects of information are not limited to changes in voting behavior or electoral outcomes. For notational convenience in describing equilibria, I will, henceforth, refer to the cumulative distribution function of alternatives as $F(X)$, and the corresponding density function as $f(X)$.

Evaluating the Effects of Information

Before proceeding, I would like to reintroduce the question that motivated this analysis: How does information affect electoral outcomes in the direct legislation environment? In many spatial models, the median voter's ideal point is used as a benchmark by which the correspondence between majority preferences and electoral outcomes is "measured." The generalizability of this model causes any description of the relationship between direct legislation outcomes and the median voter's ideal point to be relatively verbose. Therefore, I employ a surrogate benchmark called the complete information majority preferred alternative (CIMPA). The CIMPA is the outcome, among the (one or two) alternatives offered to voters in the direct legislation environment, that ex post provides a higher level of utility to the median voter.[14] When the setter contests an election, the CIMPA is either s_2 or SQ. (When voters are uncertain about s_2 then, by Lemma 2, the CIMPA is either X or SQ.) When the election is not contested, SQ is the only alternative and is, by definition, the CIMPA. Notice also that the "complete information *minority-preferred* alternative" is the direct legislation outcome if and only if a set of decisive voters makes the following ex post–mistaken inference:

$$\int_0^1 U_i(X, T_i)\, dF\,(X|M(s, X)) > U_i(SQ, T_i) > U_i(X, T_i);$$

that is, the *expected* utility of the setter's proposal is greater than the *known* utility from SQ, while, in fact, the *known* utility from SQ is greater than the *actual* utility a voter will receive from the true alternative. Throughout the analysis, our ability to describe the likelihood that voters make ex post mistaken inferences allows us to determine the likelihood that the CIMPA is the direct legislation outcome.

IDENTIFYING THE POWER OF INFORMATION

I now use different versions of the model to examine the effects of information asymmetries and communication opportunities on voting behavior, the setter's strategy, and electoral outcomes. To identify these effects, I first introduce a control case, a direct legislation environment where the transmission of information is not a factor and the actual interaction between the setter and the voters is negligible. All other game types will differ from the control case *only* in that specific types of communication are introduced.

In the control case the setter does not have to pay in order to contest the election, and there is no endorsement. Voters do not observe s_2 but do have prior beliefs about where it is likely to be located, $f(X)$.[15] In the control case equilibrium (the formal statement and proof of this equilibrium is included in Appendix C), the setter enters and chooses the personal ideal point as the location of s_2, while all voters condition their strategy exclusively on their prior information. The electoral outcome is determined by which of SQ and s_2 provide a greater expected utility to the median voter. Notice that this outcome depends only on voters' prior beliefs and *is not dependent* on the setter's actual choice of s_2.[16] Also, voters are more likely to make ex post mistaken inferences and the CIMPA is less likely to be the direct legislation outcome in the control case than in any of the other game types I introduce (holding other factors constant). Another interesting implication of this equilibrium is that the farther away SQ is from the median voter's ideal point, the more likely it is to lose, regardless of the actual location of s_2.

The usefulness of introducing the control case will now be revealed by comparing it to two game types that differ from it only in that a specific form of communication is introduced. The particular forms of

communication I introduce are the "observation of costly setter actions" and the "existence of perfectly reliable endorsements." If it is true that any voter can observe that an election is being held, can usually observe that a campaign is being run, and is sometimes supplied with endorsements, then the low-cost information cues that costly setter action and endorsements can provide allow me to develop a useful and relevant characterization of the voting behavior of those who do not take the time to obtain political information.

In addition, the two forms of communication I introduce are collectively exhaustive of how voters in this type of model can update their prior beliefs about the alternative. These forms of communication are descendants of the many examples in the economics literature that pertain to signaling.[17] All related models of direct legislation that choose to include the important role of information will have to utilize some combination or permutation of these two information types.

The Effect of Costly Action by an Agenda-Setter

I now examine the effect of both costly setter action and the information it provides on electoral outcomes. In many public decision-making institutions, a significant effort is required to get a particular alternative considered by a decision-making body. In California, for instance, the average successful initiative proponent spent $755,000 to qualify for the November 1988 ballot (Price 1988). In that same year, as I note elsewhere, proponents and opponents of five insurance reform initiatives spent over $82 million during the campaign season (Lupia 1991). Notice that only those individuals and groups most affected by a particular issue should expect to receive a positive return from expending the resources necessary to propose and support an alternative to SQ. Similarly, the finite length of legislative sessions suggests that marketlike forces may determine which alternatives are brought to a vote. The fact that a particular bill is presented to any legislative committee suggests that some expenditure of resources (effort) was engaged in by the bill's sponsor(s). Knowledge about the magnitude of this expenditure could provide a signal to an incompletely informed legislator (voter) about how much the bill differs from SQ.

In the *costly entry* game type, the setter must decide whether or not to spend K (> 0 and common knowledge) in order to contest the election. As before, voters do not directly observe s_2 but do have beliefs about it. Because the setter need not contest the election, the fact that the setter does so conveys information to the voters. The information provided to voters by this action is that the setter believes that he or she can recover (at least) the cost of contesting the election.[18] (Notice that the *only* difference between this game type and the control case is in the value of K.) For $K > 0$, the fact that the setter contests the election, along with the voters' knowledge of the setter's single-peaked utility function, implies that s_2 is not within a well-specified neighborhood of SQ, since electoral outcomes near SQ will not provide enough extra utility to make a setter's contesting the election a profitable endeavor.

Let $\epsilon(K)$ (hereafter ϵ) be a distance on the policy continuum that is an increasing function of K.[19] ϵ determines the range of alternatives within which it will never be profitable for the setter to contest an election. Since K and the shape of the setter's utility function is known, the correspondence between K and ϵ is common knowledge, and so is the distance from SQ within which it is impossible for the setter to recover the cost of contesting the election. Lemma 3 shows that for setters whose ideal points are located within the "range of unprofitable alternatives" $[SQ - \epsilon, SQ + \epsilon]$, there exist no policies which, given the cost of contesting the election K, will provide the setter with a higher level of utility than costlessly accepting SQ.

LEMMA 3. *If the setter's ideal point is located in the range of unprofitable alternatives, the setter should not contest the election.*

When $K > 0$ and the setter contests the election, voters know that $X \notin [SQ - \epsilon, SQ + \epsilon]$, and the voters' updated beliefs will have no support on this range. When the setter enters, voters observe the message (1, 0) and use Bayes Rule to incorporate this information into their beliefs about the location of s_2. This updating leads to a revised distribution of setter ideal points $F(X|(1, 0))$, which is related to $F(X)$ in the following way:

$$f(X|(1,0)) = \begin{cases} 0 & \text{if } X \in [SQ-\epsilon, SQ+\epsilon] \\ f(X)\times\dfrac{1}{1-F(SQ+\epsilon)+F(SQ-\epsilon)} & \text{if } X \in [0,SQ-\epsilon),(SQ+\epsilon,1]. \end{cases}$$

A voter given a single opportunity to guess the exact location of s_2 before and after observing "costly setter entry" would more likely guess correctly after the observation. The increased likelihood is due to the fact that there are fewer possible responses after the updating, and the responses that are eliminated by the updating would have all been incorrect guesses. Therefore, I claim that after observing costly setter entry, voters can make more accurate inferences about the location of s_2.

Besides allowing voters to update their beliefs in the manner just described, the size and location of the "range of unprofitable alternatives" will determine the number of voters that are members of one of two mutually exclusive and collectively exhaustive subsets of the electorate. The members of the first subset are called *centrist voters* $\{i|T_i \in [SQ - \frac{\epsilon}{2}, SQ + \frac{\epsilon}{2}]\}$ and the members of the second subset are called *noncentrist voters* $\{i|T_i \notin [SQ - \frac{\epsilon}{2}, SQ + \frac{\epsilon}{2}]\}$. The setter (being completely informed) knows the exact number of voters in each subset. Lemma 4 tells us that *centrist voters* can infer from the setter's entry, and the

TABLE 3.

Behavior and Outcomes from Experiments Based on the Model

TYPE OF DIRECT LEGISLATION ENVIRONMENT	CONTROL CASE	COSTLY ENTRY	COMPLETE INFORMATION
consistent votes (ex post)/total votes	164/246 (67)	212/289 (73)	233/246 (95)
CIMPA was the electoral outcome	22/40 (55)	69/80 (86)	37/40 (92.5)

Note: Percentages are in parentheses. CIMPA stands for Complete Information Majority Preferred Alternative.

common knowledge, that s_2 will provide them with lower utility than will SQ; that is, a voter who has an ideal point that is sufficiently close to SQ knows that anyone who pays a large amount in an attempt to defeat SQ must be proposing an alternative that is relatively far from his ideal point.

LEMMA 4. *"Vote for the status quo" is a dominant strategy for all centrist voters.*

In the equilibrium of the costly entry game type (the formal statement and proof of this equilibrium is included in Appendix C), the setter enters and chooses the personal ideal point as the location of s_2 if and only if the ideal point is not located within the range of unprofitable alternatives *and* updated beliefs will lead to a majority of voters to vote for s_2. Otherwise, she does not contest the election.[20] Centrist voters vote for SQ (and never make ex post mistaken inferences). Noncentrist voters maximize expected utility, where the expectation is conditional on their ideal point and their updated beliefs about s_2. From a computational perspective, it is interesting to note that s_2 is the electoral outcome only if the number of noncentrist voters who expect to receive a higher level of utility from s_2 than SQ make up a *majority of all voters.*

We can form several testable hypotheses about the effect of information asymmetries and signaling on the relationship between voter preferences and electoral outcomes by comparing the costly entry and control case equilibria. First, both centrist and noncentrist voters can use their observation of the agenda-setter's entry decision to make more accurate inferences about the location of s_2. Second, the introduction of this type of information should result in a greater (relative to the control case) likelihood that the CIMPA is the direct legislation outcome.[21]

I have elsewhere used a series of laboratory experiments to test whether the introduction of costly-entry-type information would (1) affect the probability that incompletely informed voters cast votes that were consistent with the votes they should have cast if they were "completely informed income maximizers" and/or (2) change the probability that the CIMPA was the direct legislation outcome (Lupia n.d.[a]). Some of these experiments were based on the control case and costly entry game types. In other experiments, all players were *completely informed.* Table 3 shows that the introduction of costly entry increased both of the aforementioned probabilities in these experiments. (Compare the control case experiments to the costly entry experiments.)

The effect of the type of information introduced in this game type has important substantive implications. To the extent that we are able to project the concept of costly entry on to a broader concept of "voter observations and beliefs about agenda-setter effort," we can use this model to better understand how voters can use commonly available, low-cost information cues in order to make decisions that are more likely to lead to the outcomes that would be chosen if the electorate had acquired more accurate information.

The Effect of Credible Endorsements

I now examine the effect of perfectly credible endorsements on voting behavior and electoral outcomes. The endorsement, $M_2(s_2) \in \{-1, 0, 1\}$, represents a costlessly verifiable, truthful opinion expressed by some individual or organization about the relative merits of SQ and s_2. In this model, it is assumed that voters know that the endorser possesses complete information and is a perfectly reliable source of information. The endorsement allows us to characterize the effect that the statements of well-known individuals, trusted friends, political parties, and opinion leaders have on individual-level voting behavior. The endorsement, as modeled here, is similar to the endorsement concept introduced by McKelvey and Ordeshook (1985). (In the description of this game type, $K = 0$ is assumed. Therefore, the only difference between this game type and the control case is the availability of the endorsement.)[22]

The content of the endorsement is either "s_2 is to the left of SQ," $M_2(s_2) = -1$; "s_2 is the same as SQ," $M_2(s_2) = 0$; or "s_2 is to the right of SQ," $M_2(s_2) = 1$. Since it is common knowledge that a perfectly credible endorsement will be provided, the setter knows that the choice of s determines which of three messages, $M(s, X) \in \{(1, -1), (1, 0), (1, 1)\}$, will be sent to voters. Voters use Bayes Rule to incorporate this information into their beliefs about the location of X. This updating leads to a revised distribution of setter ideal points, $F[X|(1, 1)]$ or $F[X|(1, -1)]$, which is related to prior beliefs $F(X)$ as follows:

If $M(s, X) = (1, 1)$, then

$$f(X|(1, 1)) = 0 \in [0, SQ)$$

$$= f(X) \times \frac{1}{1 - F(SQ)} \in (SQ, 1]$$

If $M(s, X) = (1, -1)$, then

$$f(X|(1, -1)) = F(X) \times \frac{1}{F(SQ)} \in [0, SQ)$$

$$= 0 \in (SQ, 1]$$

$$\text{and } F(X|(1, 0)) = SQ.$$

The updating in the game type allows all voters to make more accurate inferences about the location of s_2.

Besides allowing voters to update their beliefs in the manner just described, the endorsement serves to divide the electorate into two mutually exclusive and collectively exhaustive subsets. Let the voters whose ideal points are located in the range where $f(X|(1, 1)) = 0$ or $f(X|(1, -1)) = 0$ be called *opposite voters* ($\{i|T_i \in [0, SQ)$, if $M_2(s_2) = 1\}$, $\{i|T_i \in (SQ, 1]$, if $M_2(s_2) = -1\}$). Let all other voters be known as *nonopposite* voters. The setter, being completely informed, knows the exact number of voters in each subset. Lemma 5 tells us that *opposite voters* can infer from the endorsement and the common knowledge that X will provide them will lower utility than will SQ. For instance, a voter who knows that SQ is to the right of his or her ideal point, and whom an endorsement tells that s_2 is to the right of SQ, then the voter knows that SQ will provide higher utility than the realization of s_2.

LEMMA 5. *"Vote for the status quo" is a dominant strategy for all opposite voters.*

In the equilibrium of the *endorsement* game type (the formal statement and proof of this equilibrium is included in Appendix C), the setter always enters and chooses the personal ideal point as the location of s_2. Opposite voters vote for SQ and never make ex post mistaken inferences. Nonopposite voters maximize expected utility, where the expectation is conditional on their ideal points and their beliefs about s_2—which themselves are conditional on the endorsement. In general, all voters can use the endorsement to make more accurate inferences, which results in a greater likelihood (relative to the control case) that the CIMPA is the electoral outcome. From a computational perspective, s_2 is the electoral outcome only if the number of nonopposite voters whose updated expected level of utility from s_2 is higher than the certain level of utility that SQ provides make up a *majority of all voters*. In addition, if the setter's ideal point is on the opposite side of SQ from the median voter's ideal point, then the presence of a credible endorser prevents the setter from obtaining an outcome preferred to SQ.

When credible endorsements are available, the model suggests that the ability to answer questions about issue specifics is not a necessary condition for casting an "informed" vote. I have elsewhere attempted an empirical verification of this suggestion (Lupia 1991). In November 1988, I conducted an exit poll that was designed to elicit a measurement of what insurance reform voters in California knew about issue specifics, the "underlying" policy preferences of prominent endorsing groups, and the explicit preferences of the prominent endorsing groups over the alternatives that qualified for the ballot. One finding from the analysis of the poll is that respondents who possessed information about endorsers but could not answer questions about issue specifics cast votes very similar to those respondents who had both similar, relevant, personal characteristics and possessed enough information to answer issue-specific questions. This finding supports my model's claim that voters can use endorsements to make more accurate inferences (i.e., the types of inferences that "informed" respondents make) and increase the likelihood that they cast the same vote they would have cast if they had possessed better, or complete, information.

The effect of introducing costly setter action and perfectly credible endorsements suggest that the voters' access to simple and publicly available sources of information can help direct legislation voters make ex post more accurate inferences. The fact that we observe political elites making efforts to provide these types of cues suggests that these actors recognize and adapt to the information problems that are inherent in political decision making. If obtaining these forms of information requires less effort than obtaining information about the true location of an alternative, then the comparative statics suggest that when credible endorsements are available, a voter can cast an "informed" vote while possessing what might appear to be very limited information.

The Effect of Learning the Setter's Ideal Point

Finally, I consider the case where voters learn the location of the setter's ideal point. I include this game type to show that the setter's equilibrium location strategy, $s_2 = X$, holds even when voters are completely informed about the agenda-setter's preferences. In this game type, the perfectly reliable endorsement tells voters the location of the setter's ideal point, X, as opposed to the directional information it provided in the previous game type. That voters know X and do not observe s_2 when it is time for them to vote yields a game that is similar to the *one seller and sophisticated buyer* model of Milgrom and Roberts (1986).

In the equilibrium to the *learn setter's ideal point* game type, (the formal statement and proof of this equilibrium is included in Appendix C), the setter always enters and chooses $s_2 = X$. Voters never make ex post mistaken inferences because their knowledge of X allows them to infer precisely where s_2 will be. The electoral result is the setter's ideal point only

when the median voter prefers X to SQ. In addition, the CIMPA is always the electoral outcome.

The analysis of this game type is interesting since it provides an example of how even the smallest degree of voter uncertainty about the location of the alternative does not provide the setter with a sufficient incentive to consider voter preferences when making a location decision; that is, unless voters have full information in the direct legislation environment, the setter should choose his or her ideal point as the alternative to SQ. Intuition suggests that the voter's location strategy may be more responsive to voter preferences if either repeated play or increased electoral competition provided voters with opportunities to punish setters who propose "extreme" alternatives. The accuracy of this suggestion should be evaluated in future research.

CONCLUSION

This spatial model allows us to obtain a better understanding of the effect that different types of information have on political behavior and electoral outcomes. I use the model to provide a relatively dynamic description of how voters can use information cues to make more accurate inferences about electoral alternatives. More accurate voter inferences increase the probability that an incompletely informed voter casts the same vote he or she would have cast with better or complete information. When information cues increase the likelihood that incompletely informed voters emulate the behavior of informed voters, the probability that the complete information majority preferred alternative (CIMPA) is the direct legislation outcome increases.

I also use the model to show conditions under which information and communication opportunities affect the strategies of a monopoly agenda-setter. The most noteworthy characteristic of agenda-setter behavior in the model is that the agenda-setter should offer the personal ideal point as an alternative to the status quo when knowing that voters will be uncertain about the location (content) of the alternative. The fact that the content-determining component of the setter's strategy can be totally unresponsive to voter preferences demonstrates that the effects of political information are not necessarily limited to voter behavior or electoral outcomes. A comparison of setter behavior under different information conditions shows how the information that political elites have about the sources and content of election-day voter information might affect their preelection decisions about how to choose issues or frame campaign messages.

I now suggest two ways to enhance the applicability of this research and provide one piece of advice for those who wish for better informed voters. One direction for future research is to incorporate competition among political elites into this type of model. The presence of incompletely informed voters generates the potential effectiveness of information cues. If political elites expect cues to have an effect on voting behavior, we should not be surprised when they incorporate the provision of information cues into their electoral strategies. I expect that competition among potential agenda controllers and competition among information providers will affect the type and content of information available to voters. If competition affects information and information affects voting behavior, then research that incorporates both information and competition promises to teach us a great deal about political decision making.

I also believe that incorporating the notion of credibility into the study of political decision making is important. In most studies of politics, the role of information cues is ignored. In this model, information cues are perfectly credible. In reality, there exist many types of information cues; and they are often not perfectly credible. Those who have the resources to provide political information sometimes have an incentive to mislead voters. This possibility suggests that the credibility of the information provider could determine how certain types of information affect strategies and electoral outcomes. Credibility is a dynamic phenomenon and a complete description of its effects requires a distinct analysis. In a model related to the present one I feature credibility as a fundamental (and continuous) parameter for the analysis of elections; I find conditions under which an uninformed voter can use aspects of interaction with a possibly unreliable information provider to increase the likelihood of making the same decision the voter would have made if completely informed; and I show that the central results, derived herein, are robust to the introduction of credibility as a parameter (Lupia n.d.[b]).

I close by restating that my original motivation for the development of this model came from the debate about the use of the initiative and referendum in the determination of public policy. One of the criticisms of these institutions is that voters are not informed enough to make complex policy decisions. While educating voters about the intricacies of policy is one way to quell this type of criticism, I assert that many voters would not volunteer for such an education, since they have other things they want to do. Therefore, suppose that we can identify a class of situations where the existence of credible cues would help voters make more accurate inferences and that anticipation of the availability of these cues gives political elites greater incentives to be responsive to voter preferences. Then, those who want "more informed outcomes" might be better off ensuring that credible cues are provided to individuals who have an interest in electoral outcomes, the capacity for simple reasoning, and other things to do.

APPENDIX A: DERIVATION OF THE EQUILIBRIUM CONCEPT

I define the equilibrium by stating the Bayes–Nash equilibrium for this game and then incorporating the

assumptions of this model into the Bayes–Nash statement. Let $\bar{M}$ denote the message space which is $\{0, 1\}$ times the space defined in Table 2.

$\forall (k, j) \in \bar{M}$, let $A(k, j) = \{X: s_1 = k, \text{ and } M_2(s_2) = j\}$ be the set of all setter types that send message $M(s, X) = (k, j)$.

In a Bayes–Nash equilibrium, $\forall X \in [0, 1]$, the setter chooses $s \in \{0, 1\} \times [0, 1]$ to maximize

$$\phi_0(s, v_1, \ldots, v_n | X, T_1, \ldots, T_n),$$

and each voter $i \in N$ and $T_i \in [0, 1]$ chooses $v_i(T_i)$ to maximize

$$\int U_i[o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i]$$

$$dF(X)\, dG(T_{-i}).$$

The first difference between our equilibrium concept and Bayes–Nash is that we assume that voters always vote as if they are the pivotal voters (i.e., they adopt strategies that are weakly dominant with respect to the strategies of other voters); that is, $v_i^* \in S_i$ is weakly dominant if

$$\forall v_i \in S_i,\ v_{-i} \in S_{-i},\ s \in S_0,\ T_i \in [0, 1], \text{ and } T_{-i} \in [0, 1]^{n-1}$$

$$\int U_i[o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i]$$

$$\geq \int U_i[o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i],$$

with strict inequality for some T_i, T_{-i}, v_{-i}, and s. If v_i^* is dominant, it must be the case that

$$\int U_i(o(s_2, v_i(T_i, M(s, X)), v_{-i}(T_{-i}, M(s, X))), T_i)$$

$$dF(X) \geq \int U_i(o(s_2, v_i(T_i, M(s, X)),$$

$$v_{-i}(T_{-i}, M(s, X))), T_i)\, dF(X),$$

with sometimes strict inequality, which implies

$$\sum_{(j,k) \in M} \int_{A(j,k)} U_i(o(s_2, v_i(T_i, j, k), v_{-i}(T_{-i}, j, k)),$$

$$T_i)\, dF(X) \geq \sum_{(j,k) \in \bar{M}} \int_{A(j,k)} U_i(o(s_2, v_i(T_i, j, k), v_{-i}$$

$$(T_{-i}, j, k)), T_i)\, dF(X).$$

Note that for all $v_{-i} \in S_{-i}$ and $T_{-i} \in [0, 1]^{n-1}$, i can only affect the outcome of the election if

$$\sum_{i' \in (N - i)} v_{i'}(T_{i'}, (j, k)) \in \{0, 1\}.$$

In this case

$$o(s, v_i(T_i, M(j, k), v_{-i}(T_i, M(j, k))))$$

$$= \begin{cases} s_2 & \text{if } v_i = 1 \\ SQ & \text{if } v_i = -1. \end{cases}$$

Hence

$v_i =$

$$1 \text{ if } \int_{A(j,k)} U_i(s_2, T_i)\, dF(X) > \int_{A(j,k)} U_i(SQ), T_i)\, dF(X)$$

$$-1 \text{ if } \int_{A(j,k)} U_i(s_2, T_i)\, dF(X) \leq \int_{A(j,k)} U_i(SQ, T_i)\, dF(X)$$

is a dominant strategy for voter i. But v_i can be rewritten as

$v_i =$

$$\begin{cases} 1 & \text{if } \int_{A(j,k)} U_i(s, T_i)\, dF(X) > U_i(SQ, T_i) \int_{A(j,k)} dF(X) \\ -1 & \text{if } \int_{A(j,k)} U_i(s, T_i)\, dF(X) \leq U_i(SQ, T_i) \int_{A(j,k)} dF(X) \end{cases}$$

$$v_i = \begin{cases} 1 & \text{if } \int_0^1 U_i(s, T_i)\, dF(X|j, k) > U_i(SQ, T_i) \\ -1 & \text{if } \int_0^1 U_i(s, T_i)\, dF(X|j, k) \leq U_i(SQ, T_i), \end{cases}$$

where

$$f(X|k, j)) = \begin{cases} \dfrac{f(x)}{pr(k, j)} & \text{if } x \in A(k, j) \\ 0 & \text{otherwise.} \end{cases}$$

I can therefore restate the equilibrium concept for the direct legislation model (which is now more similar to the sequential equilibrium concept of Kreps and Wilson 1982 than Bayes–Nash) as a set of strategies $s \in S_0$, $v_i \in S_i$, and voter beliefs $f(X|k, j)$, such that for each $(k, j) \in \bar{M}$,

SETTER $\forall X$, $s = (s_1, s_2)$ satisfies $\max_{s \in \{0,1\} \times [0,1]}$

$$[U_0(o(s_2, v_1(T_1, M(s, X)), \ldots,$$

$$v_n(T_n, M(s, X)), X)) - (K \times s_1)].$$

VOTERS $\forall T_i$, $(i \in N)$, and $\forall (k, j) \in \bar{M}$, $v_i(T_i, k, j)$ satisfies

$$v_i = 1 \text{ if } \int_0^1 U_i(s_2, T_i)\, dF(X|k, j) > U_i(SQ, T_i)$$

$v_i = -1$ otherwise.

BELIEFS $\forall (k, j) \in \bar{M} : f(X|k, j) = \begin{cases} \dfrac{f(x)}{pr(k, j)} & \text{if } x \in A(k, j) \\ 0 & \text{otherwise,} \end{cases}$

where

$$pr(k, j) = \int_{A(k,j)} F(x)\, dx.$$

If we assume that the setter is completely informed, we can redefine and simplify the setter's optimization problem in the following manner. In a Bayes–Nash equilibrium, $\forall X \in [0, 1]$, the setter chooses $s \in \{0, 1\} \times [0, 1]$ to maximize

$$U_0(o(s_2, v_1(T_1, M(s, X)), \ldots, v_n(T_n, M(s, X)), X)) - (K \times s_1).$$

Now, $\forall M(s, X) \in \bar{M}$ and $s \in S_0$, let

$$C(SQ, M(s, X)) = \{|T_i|: U_i(s_2, T_i)\, dF\,(X|M(s, X)) \leq U_i(SQ, T_i)\},$$

and

$$C(s_2, M(s, X)) = N - C(SQ, M(s, X)).$$

$C[SQ, M(s, X)]$ is a count of the number of voters who receive expected utility from the SQ that is greater than or equal to the expected utility from the lottery of setter types sending message $M(s, X)$. $C[s_2, M(s, X)]$ is a count of the number of voters who receive higher expected utility from the lottery of setter types sending message $M(s, X)$ than from SQ. I can, for any $X \in [0, 1]$, redefine the setter's optimization problem as follows. *Choose* $s = (s_1, s_2)$ *to maximize*

$$\Phi_0(s, M(s, X)|X) = \begin{cases} U_0(SQ, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) \geq \dfrac{N}{2} \\ U_0(s_2, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) < \dfrac{N}{2}. \end{cases}$$

Thus, the equilibrium concept I have used is defined as a set of strategies, $s \in S_0$, $v_i \in S_i$, and voter beliefs, $f(X|k, j)$, such that for each $(k, j) \in \bar{M}$

SETTER $\forall X$, $s = (s_1, s_2)$ satisfies $\max_{s \in \{0,1\} \times [0,1]}$

$$\Phi_0(s, M(s, X)|X) = \begin{cases} U_0(SQ, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) \geq \dfrac{N}{2} \\ U_0(s_2, X) - (K \times s_1) & \text{if } C(SQ, M(s, X)) < \dfrac{N}{2}. \end{cases}$$

VOTERS $\forall T_i$, $(i \in N)$, and $\forall (k, j) \in \bar{M}$, $v_i(T_i, k, j)$ satisfies

$v_i = 1$ if $\int_0^1 U_i(s_2, T_i)\, dF\,(X|k, j) > U_i(SQ, T_i)$

$v_i = -1$ otherwise.

BELIEFS $\forall (k, j) \in \bar{M} : f(X|k, j)$

$$= \begin{cases} \dfrac{f(x)}{pr(k, j)} & \text{if } x \in A(k, j) \\ 0 & \text{otherwise,} \end{cases}$$

where

$$pr(k, j) = \int_{A(k,j)} F(x)\, dx.$$

APPENDIX B: PROOF OF LEMMA 2

Proof. The setter's choice of strategy relays a message to voters. Voters condition their choice on the message $M(s, X) = (s_1, M_2(s_2))$. I now establish the dominance of the strategy $s_2 = X$ for the four game types I introduce.

1. From the definition of the control case and Lemma 1, the setter sends only one type of message, $M(s, X) = (1, 0)$. (The setter always [costlessly] enters and there is no endorsement.) In this case, where $K = 0$, the setter chooses s to maximize

$$\Phi_0(s, M(s, X)|X) = \Phi_0((1, s_2), (1, 0)|X) = \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, 0) \geq \dfrac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, 0) < \dfrac{N}{2} \end{cases}$$

If $C(SQ, 1, 0) \geq N/2$, then $\Phi_0((1, s_2), (1, 0)|X) = U_0(SQ, X)$, independent of s_2. If $C(SQ, 1, 0) < N/2$, then $\Phi_0((1, s_2), (1, 0)|X)$ is maximized when $s_2 = X$. Therefore, since U_0 is maximized at $s_2 = X$ independent of the value of $C(SQ, 1, 0)$, this strategy is weakly dominant for the setter in the control case game.

2. In the *costly entry* case, the setter can send one of two messages, $M(s, X) \in \{(0, 0), (1, 0)\}$. (The setter either enters or does not. There is no endorsement.) If $M(s, X) = (1, 0)$, then $\Phi_0((1, s_2), (1, 0)|X)$ is maximized by $s_2 = X$, as was shown in the control case section of this proof. Note that the value of $\Phi_0((0, s_2), (0, 0)|X)$, the setter's utility when the election is not contested, is not affected by s_2: $\Phi_0((0, s_2), (0, 0)|X) = U_0(SQ, X) - (K \times 0)$. Therefore, $s_2 = X$ is weakly dominant for the setter in the costly entry case.

3. From the definition of the *endorsement* game type and Lemma 1, the setter can send one of three messages, $M(s, X) \in \{(1, -1), (1, 0), (1, 1)\}$. (The setter always [costlessly] enters and there is an endorsement.) The setter chooses s to maximize (where $K = 0$)

$$\Phi_0(s, M(s, X)|X) = \begin{cases} \Phi_0((1, s_2), (1, 1)|X) & \text{if } SQ < s_2 \\ \Phi_0((1, s_2), (1, 0)|X) & \text{if } SQ = s_2 \\ \Phi_0((1, s_2), (1, -1)|X) & \text{if } s_2 < SQ, \end{cases}$$

where

$$\Phi_0[(1, s_2), (1, 1)|X] = \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, 1) \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, 1) < \frac{N}{2} \end{cases}$$

$$\Phi_0[(1, s_2), (1, 0)|X] = U_0(SQ, X)$$

$$\Phi_0[(1, s_2), (1, -1)|X] = \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, 1) \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, -1) < \frac{N}{2}. \end{cases}$$

If $X = SQ$, then the proof is obvious. If $X \neq SQ$, then it remains to show that $s_2 = X$ is weakly dominant. Consider the following two subcases:

a. $s_2' \neq X$ and $s_2 = X$, where $s' = (s_1, s_2')$ and $s = (s_1, X)$, and $M(s', X) = M(s, X)$. If $s_2' \neq X$ then $U_0(X, X) > U_0(s_2', X)$ which implies $\Phi_0(s, M(s, X)|X) \geq \Phi_0(s', M(s_0', X)|X)$. (I.e., since $M_2(s_2 = X) = M_2(s_2 \neq X)$, voters cannot differentiate amongst the setter types that would send each message.) Thus, $s_2 = X$ is a weakly dominant setter location strategy in the endorsement game type when $M_2(s_2) = M_2(s_2)$.
b. $s_2 \neq X$ and $M_2(s_2) \neq M_2(X)$. Thus, $s_2 \neq SQ$. Let $s_2'(X) = (s_2 + SQ)/2$. $M_2(s_2) \neq M_2(X)$ implies that X and s_2 are on opposite sides of SQ. Thus, any $s_2'(X)$ that is closer to SQ provides a higher level of utility to the setter: $U_0[s_2'(X), X] > U_0(s_2, X)$. Hence, changing from s_2 to $s_2'(X)$ gives the setter a higher level of utility. Thus, s_2 cannot be an equilibrium partial strategy.

From Case A and Case B, I have established that $s_2 = X$ is a weakly dominant location strategy for the setter in the "Endorsement" game type.

4. In the *learn setter's ideal point* game type, the definition of the endorsement changes. $M_2(s_2) = X$ is the endorsement; $\bar{M} = \{0, 1\} \times [0, 1]$ is the message space; and $K = 0$.[23] Then, I can define the setter's objective function for this game type as

$$\Phi_0'(s, M(s, X)|X) = \begin{cases} U_0(SQ, X) & \text{if } C[SQ, M(s, X)] \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C[SQ, M(s, X)] < \frac{N}{2}. \end{cases}$$

The setter of type X can send one of two messages in this game type, $M' \in \{(0, X), (1, X)\}$. The setter's chooses s to maximize $\Phi_0'(s, M(s, X)|X)$, where

$$\Phi_0'[(0, s_2), (0, X)|X] = U_0(SQ, X).$$

$$\Phi_0'[(1, s_2), (1, X)|X] = \begin{cases} U_0(SQ, X) & \text{if } C(SQ, 1, X) \geq \frac{N}{2} \\ U_0(s_2, X) & \text{if } C(SQ, 1, X) < \frac{N}{2}. \end{cases}$$

Note, first, that the value of $\Phi_0'((0, s_2), (0, X)|X)$ is not affected by the setter's choice of s_2. If $M(s, X) = (1, X)$, then $\Phi_0((1, X)|X)$ is maximized by $s_2 = X$, as was shown in control case section of this proof. (The only difference between this objective function and that objective function is a constant.) Thus, $s_2 = X$ is a weakly dominant location strategy for the learn setter's ideal point game type.

APPENDIX C: FORMAL STATEMENT OF EQUILIBRIA

Control Case

PROPOSITION. *The equilibrium for the control case is*

$$\forall X \in [0, 1]: s = (1, X)$$

$$\forall T_i \in [0, 1]: v_i(T_i, M(s, X)) = \begin{cases} 1 \text{ if } \int_0^1 [U_i(X, T_i) f(X|M(s, X))] \\ \qquad > U_i(SQ, T_i) \text{ and } M_1(s_1) = 1 \\ -1 \quad \text{otherwise.} \end{cases}$$

Beliefs can be characterized by $f(X|M(s, X)) = f(X)$.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1. The setter's location strategy is proven by Lemma 2.

Costly Entry Game Type

PROPOSITION. *The equilibrium for the costly entry game type is*

$$\forall X \in [SQ - \epsilon, SQ + \epsilon]: s_1 = 0$$

$$\forall X \in [0, SQ - \epsilon), (SQ + \epsilon, 1]: s_1 = \begin{cases} 1 & \text{if } C(SQ, M(s, X)) < \frac{N}{2} \\ 0 & \text{otherwise} \end{cases}$$

$$\forall X \in [0, 1]: s_2 = X$$

$$\forall T_i \in [0, SQ - \tfrac{\epsilon}{2}), (SQ + \tfrac{\epsilon}{2}, 1]:$$

$$v_i(T_i, M(s, X)) =$$

$$1 \quad \text{if} \int_0^1 [U_i(X, T_i)f(X|1, 0))] > U_i(SQ, T_i) \text{ and } M_1(s_1)$$

$$= 1 - 1 \quad \text{otherwise}$$

Beliefs can be characterized by $f(X|(1, 0))$ or $f(X|(0, 0)) = SQ$, depending on the value of s_1.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1 and Lemma 3. The setter's location strategy is proven by Lemma 2. Centrist voter equilibrium strategies can be characterized by Lemma 4.

Endorsement Game Type

PROPOSITION. *The equilibrium for the endorsement game type is*

$$\forall\ X \in [0, 1]: s_1 = 1$$

$$\forall\ X \in [0, 1]: s_2 = X$$

$$\forall\ T_i \in [0, SQ): v_i[T_i, M(s, X)] =$$

$$\begin{cases} 1 \text{ if } M_2(s_2) = -1 \text{ and } \int_0^1 [U_i(X, T_i)f(X|(1, -1))] \\ \qquad > U_i(SQ, T_i) \text{ and } M_1(s_1) = 1 \\ -1 \text{ otherwise} \end{cases}$$

$$\forall\ T_i \in (SQ, 1]: v_i[T_i, M(s, X)] =$$

$$\begin{cases} 1 \text{ if } M_2(s_2) = 1 \text{ and } \int_0^1 [U_i(X, T_i)f(X|(1, 1))] \\ \qquad > U_i(SQ, T_i) \text{ and } M_1(s_1) = 1 \\ -1 \text{ otherwise.} \end{cases}$$

Beliefs can be characterized by $f(X|(1, -1))$ or $f(X|(1, 1))$, depending on the value of $M_2(s_2)$.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1. The setter's location strategy is proven by Lemma 2. The opposite voter equilibrium strategies can be characterized by Lemma 5.

Learn Setter's Ideal Point Game Type

Let $\bar{M} = \{0, 1\} \times [0, 1]$ be the message space. $M_2(s_2) = X$ is the endorsement, which reveals the setter's ideal point to voters (cf. Milgrom and Roberts 1986, prop. 1).

PROPOSITION. *The equilibrium for the "Learn Setter's Ideal Point" game type is:*

$$\forall\ X \in [0, 1]: s_1 = 1 \text{ if } C(SQ, M(s, X)) < \frac{N}{2}$$

$$\forall\ X \in [0, 1]: s_1 = 0 \text{ otherwise}$$

$$\forall\ X \in [0, 1]: s_2 = X$$

$$\forall\ T_i \in [0, 1]: v_i[T_i, M(s, X)]$$

$$= \begin{cases} 1 & \text{if } U_i(X, T_i) > U_i(SQ, T_i) \text{ and } M_1(s_1) = 1 \\ -1 & \text{if } U_i(X, T_i) < U_i(SQ, T_i). \end{cases}$$

Beliefs can be characterized by X.

Proof. The setter's equilibrium entry strategy is proven by Lemma 1. The setter's location strategy is proven by Lemma 2.

Notes

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1. The distinction between these two election types is that referenda originate from within the government and ballot initiatives originate from outside the government. The United State is one of the few democracies that does not use the initiative or referendum to determine national level policy, but their use is extensive at the state and local level. Recent important—but largely atheoretical—contributions to this debate include those of Magleby (1984) and Cronin (1989).

2. The setter model has been examined under different assumptions about the nature of incomplete information. In Romer and Rosenthal 1979, the setter model is examined under conditions of uncertain turnout. In Morton 1988, voters condition their actions in the present period on anticipated setter actions in future periods. Rosenthal (1990) discusses other elaborations of the "setter model."

3. The idea that the presence of low-cost information cues can influence policy outcomes is powerfully made by McCubbins and Schwartz (1984). They argue that Congress can efficiently narrow the divergence between legislative goals and bureaucratic actions by designing institutions that provide interested parties with incentives to obtain information (referred to as "fire alarms") about the correspondence between legislative goals and bureaucratic activities.

4. For simplicity, the one-dimensional case is discussed. Since there are, at most, two electoral alternatives in this model, the extension of the model to higher levels of dimensionality (making standard assumptions about voter preferences) is straightforward.

5. This representation of setter preferences allows a general representation of player prior beliefs. For example, when F is uniformly distributed, voters will assume that all possible setter ideal points can be drawn with equal probability. Alternatively, when F has all of its mass on one point, the voter's priors are completely informative.

6. A simple way to relax the assumption that the setter is completely informed is to allow N to be either infinite (or large and finite, a good approximation) and let the setter's information about the voter ideal points be the distribution G. A derivation of the model using the latter assumption is available upon request.

7. For notational convenience, I denote $s_2(X) = SQ$, when $s_1(X) = 0$ (i.e., the setter chooses to accept SQ).

8. Also, since $F(X)$ and $[0, 1]$ are continuous, $SQ = s_2$ occurs with near zero probability. If this event does occur, it follows trivially that SQ is the direct legislation outcome.

9. Supermajorities follow straightforwardly by changing the values of v_i. None of my results depend on the use of simple majority rule.

10. I have assumed that the SQ wins ties. This assumption is consistent with the tie-breaking rule used in many of direct legislation environments. Changing this assumption does not

alter the power of the results that follow, but it will influence boundary conditions in straightforward ways.

11. This restriction is made for notational and expositional simplicity. The assumptions of symmetry and linearity are not necessary for the proofs of any of my lemmas or propositions.

12. This assumption is common to most spatial election models.

13. The proofs of other lemmas follow straightforwardly from the assumptions of the model and are available upon request.

14. This statement seems to depend on the nonexistence of a preference profile that will sustain an enforceable system of side payments. In the one-dimensional case, the CIMPA always exists (and, in fact, is always the core or in the core).

15. In this game type, voters always receive the same message $M(s, X) = (1, 0)$, because (from Lemmas 1 and 2) we know that when $K = 0$, contesting the election is a weakly dominant strategy for the setter and there is no endorsement.

16. Except for "opposite" and "centrist" voters, the strategies of voters can be fully characterized using a single cut-point. (This concept is used in Alesina and Rosenthal 1989.) A proof of the uniqueness of the cut-point in this model is available upon request.

17. Spence 1973 is the seminal article on signaling; Milgrom and Roberts 1986 is also directly relevant.

18. By introducing the costly entry concept, I am not attempting to make any statement about the welfare properties associated with altering the magnitude of the barriers to ballot access. I introduce costly entry, since it is a type of information that is, to some extent, observable and is sometimes (as is the case with intense legislative wrangling or initiative campaign expenditures) widely publicized.

19. $d\epsilon/dK$ and $d^2\epsilon/dK^2$ are positive; $\epsilon(0) = 0$.

20. This implies that either all setter types whose ideal points lie outside of the "range of unprofitable alternatives" choose to contest the election or none do.

21. In addition, the introduction of costly entry increases the probability that SQ is the direct legislation outcome by narrowing the likelihood that a setter will challenge it. This type of result has been produced in a direct-legislation-type experimental environment by Herzberg and Wilson (1990).

22. The characteristics of the equilibrium of a game type that included both costly entry and endorsements follow straightforwardly from the equilibria presented.

23. The information provided by voters by $K > 0$ in the previous game types is superfluous in this game type.

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COVENANTS WITH AND WITHOUT A SWORD: SELF-GOVERNANCE IS POSSIBLE

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Contemporary political theory often assumes that individuals cannot make credible commitments where substantial temptations exist to break them unless such commitments are enforced by an external *agent. One such situation may occur in relation to* common pool resources, *which are natural or man-made resources whose yield is subtractable and whose exclusion is nontrivial (but not necessarily impossible). Examples include fisheries, forests, grazing ranges, irrigation systems, and groundwater basins. Empirical evidence, however, suggests that appropriators in common pool resources develop credible commitments in many cases without relying on external authorities. We present findings from a series of experiments exploring (1) covenants alone (both one-shot and repeated communication opportunities); (2) swords alone (repeated opportunities to sanction each other); and (3) covenants combined with an internal sword (one-shot communication followed by repeated opportunities to sanction each other).*

> And Covenants, without the sword, are but words, and of no strength to secure a man at all.
>
> —Hobbes, *Leviathan*

Contemporary political theories frequently presume that individuals cannot make credible ex ante commitments where substantial ex post temptations exist to break them unless such commitments are enforced by an external agent. Hobbes justified the necessity of Leviathan on the frailty of mere words. Hobbes calls a contract that involves a promise by at least one of the parties to perform in the future a *covenant* (1960, 87). When both parties promise future performance, it is a *covenant of mutual trust* (p. 89). A covenant of mutual trust in a state of nature is void in Hobbes's view if either has a reasonable suspicion that the other will not perform: "For he that performeth first, has no assurance the other will perform after; because the bonds of words are too weak to bridle men's ambition, avarice, anger, and other passions, without the fear of some coercive power; which in the condition of mere nature, where all men are equal, and judges of the justness of their own fears, cannot possibly be supposed" (pp. 89–90). On the other hand, a covenant made "where there is a power set up to constrain those that would otherwise violate their faith" is likely to be fulfilled (p. 90). Thus, Hobbes argued for the necessity of a "coercive power, to compel men equally to the performance of their covenants, by the terror of some punishment, greater than the benefit they expect by the breach of their covenant" (p. 94).

The weakness of mere words and the necessity of external agents to enforce contracts is also a foundation upon which the powerful edifice of noncooperative game theory has been constructed. John Nash (1950, 1951) distinguished between cooperative and noncooperative games. In cooperative games, players can communicate freely and make enforceable agreements; in noncooperative games, they can do neither. Some theorists particularly stress the inability to make enforceable agreements: "The decisive question is whether the players can make enforceable agreements, and it makes little difference whether they are allowed to talk to each other. Even if they are free to talk and to negotiate an agreement, this fact will be of no real help if the agreement has little chance of being kept. An ability to negotiate agreements is useful only if the rules of the game make such agreement binding and enforceable" (Harsanyi and Selten 1988, 3).[1] In other words, communication without a change in the payoff function does not eliminate a Nash equilibrium (the major solution concept used in noncooperative game theory). Not all Nash equilibria are efficient. Equilibrium payoffs are lower in a finitely repeated, full-information dilemma game than if the participants could make credible ex ante commitments. Hobbes's state of nature has frequently been represented as a social dilemma game.[2]

The assumption that what is called a *sword* wielded by an "external enforcer" is necessary before individuals can make credible ex ante commitments in social dilemma situations has important implications for common pool resources. Common pool resources (CPRs) are natural or man-made resources whose yield is subtractable and the exclusion from which is nontrivial (but not necessarily impossible).[3] Because CPRs can be represented as social dilemmas, particular policy recommendations follow almost immediately. Ophuls argued, for example, that "environmental problems cannot be solved through cooperation . . . and the rationale for government with major coercive powers is overwhelming" (1973, 228). His conclusion was that "even if we avoid the tragedy of the commons, it will *only* be by recourse to the tragic necessity of Leviathan" (p. 220).

Empirical evidence suggests, however, that individuals facing social dilemmas in many cases develop credible ex ante commitments without relying on external authorities. *Appropriators* from CPRs (e.g., fishers, irrigators, and herders) have repeatedly shown their capacity to organize themselves, establish credible commitments, monitor each others' behavior, and impose sanctions on those who break their commitments.[4] Self-organized CPR institutions have been devised without reference to central authorities and sustained over long periods of time without enforcement by external agents.[5] Experimental studies have repeatedly found that individuals placed in laboratory social dilemmas and allowed to communicate consistently achieve better outcomes than predicted by noncooperative game equilibrium. While findings concerning the positive effect of communication in laboratory settings (covenanting without a sword) lead to optimistic predictions about the capacities of individuals to solve social dilemmas, many cases of self-organized arrangements in field settings rely on internal sanctioning mechanisms (E. Ostrom 1990).

Past research has produced three anomalies from the standpoint of predictions derived from both Hobbes's state-of-nature theory and noncooperative game theory:

1. In one-shot social dilemma experiments, communication alone leads to substantial improvement in outcomes.[6]
2. In repeated social dilemma experiments, repeated communication alone leads to substantial improvements in joint outcomes.[7]
3. In field settings of repeated social dilemmas, participants invest substantial time and effort monitoring and imposing sanctions on one another.[8]

The results from field settings show that participants in social dilemmas do not rely entirely on communication. They monitor each other closely and impose sanctions on those who do not conform to the rules they have devised. If communication alone were a fully reliable mechanism to overcome the gap between ex post temptations and ex ante promises, then one should not see time and effort devoted to monitoring and sanctioning efforts in the field. Thus, prior research has generated interesting puzzles. Given that social dilemmas lie at the foundation of the theory of the state and the theory of collective action, it is important to explore the independent and interactive effects of (1) communicating (covenanting) alone, both one-shot and repeated; (2) sanctioning (the sword) alone; and (3) communicating with options to sanction (covenants with a sword). We shall manipulate experimental treatments to examine all three mechanisms. We shall construct a common constituent game as the basis for all these manipulations. This game, an n-person CPR appropriation game, will now be described and solved.

GAME-THEORETICAL PREDICTIONS

The Common Pool Resource Constituent Game

We will first specify the class of constituent CPR games from which we draw our designs. Assume a fixed number n of appropriators with access to the CPR. Each appropriator i has an endowment of resources e which can be invested either in the CPR or a safe, outside activity. The marginal payoff of the outside activity is normalized equal to w. The payoff to an individual appropriator from investing in the CPR depends on aggregate group investment in the CPR and on the appropriator investment as a percentage of the aggregate. Let x_i denote appropriator i's investment in the CPR, where $0 \leq x_i \leq e$. The group return to investment in the CPR is given by the production function $F(\Sigma x_i)$, where F is a concave function, with $F(0) = 0$, $F'(0) > w$, and $F'(ne) < 0$. Initially, investment in the CPR pays better than the opportunity cost of the forgone safe investment [$F'(0) > w$]; but if the appropriators invest all resources in the CPR the outcome is counterproductive [$F'(ne) < 0$]. Thus, the yield from the CPR reaches a *maximum net level* when individuals invest some, but not all, of their endowments in the CPR.[9] This environment is much richer and more complex than the ubiquitous repeated prisoner's dilemma game. While no formal game or laboratory experiment ever captures all the nuances of field settings, this n-person CPR game is a far more realistic environment in which to investigate the questions we have posed than many of the dilemma games previously explored.

Let $x = (x_1, \ldots, x_n)$ be a vector of individual appropriators' investments in the CPR. The payoff to an appropriator, $u_i(x)$, is given by

$$u_i(x) = we \text{ if } x_i = 0$$

$$= w(e - x_i) + (x_i/\Sigma x_i)F(\Sigma x_i) \text{ if } x_i > 0 . \quad (1)$$

Equation 1 reflects the fact that if appropriators invest all their endowments in the outside alternative, they get a sure payoff (we), whereas if they invest some of their endowments in the CPR, they get a sure payoff $w(e - x_i)$ plus a payoff from the CPR, which depends on the total investment in that resource $F(\Sigma x_i)$ multiplied by their share in the group investment $(x_i/\Sigma x_i)$.[10] Let the payoffs (represented in equation 1) be the payoff functions in a symmetric, noncooperative game. Since our experimental design is symmetric, there is a symmetric Nash equilibrium, with each player investing x_i^* in the CPR, where

$$-w + (1/n)F'(nx_i^*) + F(nx_i^*)[(n - 1)/\Sigma^* n^2] = 0 . \quad (2)$$

At the symmetric Nash equilibrium, group investment in the CPR is greater than optimal, but not all yield from the CPR is wasted.

Compare this deficient equilibrium to the optimal solution. Summing across individual payoffs $u_i(x)$ for all appropriators i, one has the group payoff function

$$u(x) = nwe - w\Sigma x_i + F(\Sigma x_i) ,$$

which is to be maximized subject to the constraints $0 \leq \Sigma x_i \leq ne$. Given the specified productivity conditions on F, the group maximization problem has a unique solution characterized by the condition:

$$-w + F'(\Sigma x_i) = 0. \quad (3)$$

According to equation 3, the marginal return from a CPR should equal the opportunity cost of the outside alternative for the last unit invested in the CPR.

It is worth noting that both the Nash equilibrium investment and the optimum group investment do not depend on the endowment parameter e as long as e is sufficiently large. Out of equilibrium, however, a larger e means players are capable of making larger mistakes. If the time required to converge to an equilibrium is large, then the Nash equilibrium may be an inappropriate behavioral theory for CPRs with high endowments.

Finite Repetition of a Common Pool Resource Constituent Game

Denote the constituent game by X and let X be played a finite number of times. Typically, a repeated game has many equilibria to choose from. Two equilibrium refinement principles are subgame perfection and subgame consistency. An equilibrium is subgame perfect if it prescribes equilibrium play on every subgame. An equilibrium is subgame consistent if it prescribes identical play on identical subgames. If the constituent game has a unique equilibrium, then the finitely repeated game has a unique subgame perfect and subgame consistent equilibrium (Selten 1971). Thus, equation 2 characterizes a finite sequence of equilibrium outcomes. This prediction is based on the assumption of a finite game of complete information. Our subjects know the game is finite.[11] Although we do not have complete control over our subjects' understanding of their decision task, we make all information readily available to them. Failure to induce complete information on the part of our subjects jeopardizes the uniqueness of this refined equilibrium (Kreps et al. 1982).

Communication and the Constituent Game

When the constituent game X has a unique equilibrium x^*, neither repetition nor communication creates new equilibrium outcomes. Let c denote a communication strategy, in the communication phase C, available to any player. As long as saying one thing and doing another has no payoff consequences, then any strategy of the form (c, x^*) is an equilibrium of the one-shot game (C, X), and finitely repeated x^* is a subgame consistent equilibrium outcome of one-shot communication (C, X, X, . . . , X) or repeated communication (C, X, C, X, . . . , C, X). In this situation, subgame consistency is deaf to covenants. However, as we shall show, communication makes a big difference in behavior.

Sanctioning and the Constituent Game

Our sanctioning institution is represented formally using the following construction. Let s be a matrix of zeros and ones, where $s_{ij} = 1$ means that player i has sanctioned player j, and $s_{ij} = 0$ means that i has not sanctioned j. Row i of the matrix s codes all of player i's sanctioning behavior. As before, let x be a vector of individual investments in the CPR and $u_i(x)$ be i's payoff function in the game without sanctioning. Player i's payoff function in the game with sanctioning, $u_i(x, s)$, is given by

$$u_i(x, s) = u_i(x) - f1 \sum_j s_{ij} - f2 \sum_j s_{ji}. \quad (4)$$

The parameters $f1$ and $f2$ represent the cost of fining and being fined, respectively.[12] The sum Σs_{ij} is the total number of fines j levied by player i, costing him $f1$ each; the sum Σs_{ji} is the total number of times player i is fined, costing him $f2$ each.

Adding this sanctioning mechanism to our constituent game X produces a game X–S with a unique subgame consistent equilibrium. In a one-shot game with a unique Nash equilibrium x*, any sanctioning activity is costly and cannot lead to higher payoffs. Thus, the equilibrium of the one-shot game with sanctioning is the pair (x^*, S*) = (x^*, 0); that is, the equilibrium sanctioning matrix is the zero matrix. At equilibrium, no one sanctions. Now suppose that the one-shot CPR game with sanctioning is to be repeated a finite number of times T. This finitely repeated game has a unique subgame consistent equilibrium given by strategy 1:

STRATEGY 1. *In every round, play (x^*, 0). In the event of any deviation from prescribed play, resume playing (x^*, 0) after the deviation.*

This equilibrium follows from backward induction. At the last round, T, no deviation is profitable. At the next to last round, $T - 1$, given that no deviations will occur in the last round, then no deviation is profitable—and so on. Repeating the game should not lead to sanctioning, either. There is, however, compelling evidence that backward induction must be learned through repeated play.

Besides the unique subgame consistent equilibrium, there are many imperfect equilibria as well. Let $z_i < x_i^*$ be the same for all i. Consider the repeated game strategy, strategy 2:

STRATEGY 2. *In every round except T, play (z, 0). In the event of any deviation, play ($x_i = e$, $s = 1$) for one round, then resume playing (z, 0). If no deviation took place in round $T - 1$, play (x^*, 0) in round T.*

This represents a trigger strategy. All players agree to invest less than they would according to strategy 1. If some player cheats, then every player dumps all his resources into the CPR ($x_i = e$) and every player issues one sanction for one round. Then play returns to normal. In the final round, everyone plays the one-shot Nash equilibrium. We claim that strategy 2 represents an imperfect equilibrium. To show this, it

suffices to show that no deviation from prescribed play pays. Let $F(ne)$ be a very large negative number. For $f1$ and $f2$ large enough, a player who deviated optimally for one round would gain some positive amount, depending on the level of z_i but in the next round would lose $(1/n)F(ne) + f1 + f2$ due to punishment from overinvestment and sanctions, as in strategy 2. This threat we call the *dire threat*, since it is the worst threat imaginable for one round in our design. Given such a threat, it does not pay to deviate, even for one round. Finally, if a punishment is not called for in the last round, the endgame equilibrium is played in that round. This shows that strategy 2 is an equilibrium. Its imperfection lies in the fact that the trigger punishment—dumping all tokens into the resource, everybody placing a fine—is too harsh to be credible at the end of the game.[13]

There is a large set of equilibria along the lines of strategy 2 involving variation of the length of punishment (1 or more rounds), the base level of investments z_i, and the direness of the one-period threat (dump not quite all tokens in the CPR, levy fines with some probability). In particular, by varying $f1$ and $f2$, we hoped to allow the subjects to find equilibria of the strategy 2 family involving punishments of the form (z_i, I) (i.e., reduced investment in the CPR) but sanctions for everyone if a deviation occurs (see Jankowski 1990).

Communication, the Constituent Game, and Sanctioning

We investigate the combination of communication with sanctioning in two ways. Our first design allows for a one-shot communication period, which is then followed by a sequence of constituent games with a sanctioning mechanism imposed. In our second design, we impose a one-shot communication period in conjunction with an opportunity for the subjects to choose whether or not they want a sanctioning mechanism. In both designs, the payoff functions are still given by equation 4, since communication per se has no payoff consequences and sanctioning does. Without loss of generality, let c be a communication strategy. Then appending c to strategy 1 yields a subgame consistent equilibrium, and every subgame consistent equilibrium has the same payoffs as strategy 1. In addition, as in repeated X–S, imperfect equilibria exist yielding higher payoffs than equilibria which are subgame consistent.

THE LABORATORY DECISION ENVIRONMENT

Design

In our experimental investigation we have operationalized this CPR environment with eight appropriators ($n = 8$) and quadratic production functions $F(\Sigma x_i)$, where

$$F(\Sigma x_i) = a\Sigma x_i - b(\Sigma x_i)^2,$$

with $F'(0) = a > w$ and $F'(nw) = a - 2bnw < 0$. For this quadratic specification, one has from equation 3 that the group optimal investment satisfies $\Sigma x_i = (a - w)/2b$. The CPR yields 0% on net when investment is twice as large as optimal, $\Sigma x_i = (a - w)/b$. Finally, solving equation 2, the symmetric Nash equilibrium group investment is given by

$$\Sigma x_i = [n/(n + 1)](a - w)/b.$$

This level of investment is between maximal net yield and zero net yield, approaching the latter as n gets large. One additional constraint that arises in a laboratory setting is that the x_i be integer-valued. This is accomplished by choosing the parameters a, b, d, and w in such a way that the predictions associated with Σx_i are all integer-valued.

In particular, we use the parameters shown in Table 1. These parameters lead to the predictions that 36 tokens invested in Market 2 yields the level of investment that maximizes group earnings and that each subject investing 8 tokens in Market 2 is a unique Nash equilibrium. At this equilibrium, subjects earn approximately 39% of maximum net yield from the CPR. Once again, note that the Nash equilibrium and optimal investment are not affected by the level of endowments.

Subjects and the Experimental Setting

Our experiments used subjects drawn from the undergraduate population at Indiana University. Students were volunteers recruited from principles-of-economics classes. Prior to recruitment, potential volunteers were given a brief explanation in which they were told only that they would be making decisions in an "economic choice" environment and that the money they earned would be dependent upon their own investment decisions and those of the others in their experimental group. In all experiments reported here, subjects were randomly recruited from a pool of subjects with prior experience in a CPR decision environment. All experiments were conducted on the NOVANET computer system at Indiana University. The computer facilitates the accounting procedures involved in the experiment, enhances across-experiment subject control, and allows for minimal experimenter involvement.

We shall represent the state of nature with the minimal institution baseline game (no covenants/no swords), then present findings from three environments with institutional configurations: (1) an imposed communication mechanism, (2) an imposed sanctioning mechanism, and (3) an imposed communication mechanism with either an imposed sanctioning mechanism or an opportunity to choose a sanctioning mechanism. Figure 1 gives an overview of the experimental design. Table 2 presents a summary of results across designs, focusing on the average net yield from the CPR (Market 2) as a percentage of maximum possible net yield.[14]

TABLE 1

Experimental Design Baseline Parameters for a Given Decision Period

EXPERIMENT CHARACTERISTICS	EXPERIMENT TYPE: LOW-ENDOWMENT	EXPERIMENT TYPE: HIGH-ENDOWMENT
Number of subjects	8	8
Individual token endowment	10	25
Production function, Market 2[a]	$23(\Sigma x_i) - .25\ (\Sigma x_i)^2$	$23(\Sigma x_i) - .25(\Sigma x_i)^2$
Market 2 return/unit of output	1¢	1¢
Market 1 return/unit of output	5¢	5¢
Earnings/subject at group max.[b]	91¢	83¢
Earnings/subject at Nash equil.	66¢	70¢
Earnings/subject at 0 net yield	50¢	63¢

[a]Σx_i = the total number of tokens invested by the group in Market 2. The production function shows the number of units of output produced in Market 2 for each level of tokens invested therein. Market 2 represents a CPR because the total output from Market 2 (and each subject's share of that output) is a function of the investment levels of all subjects. [b]In the high-endowment design, subjects were paid in cash one-half of their "computer" earnings. This maintained potential experimental profits at near equal levels across designs. Amounts shown are potential cash payoffs.

THE BASELINE GAME: NO COVENANTS AND NO SWORDS

For comparison with the designs discussed later, the baseline experiments can be represented as an iterated series (of 20 rounds) of the constituent game X. At the beginning of each experimental session, subjects were told that (1) they would make a series of investment decisions, (2) all individual investment decisions were anonymous to the group, and (3) they would be paid their individual earnings (privately and in cash) at the end of the experiment. Subjects then proceeded at their own pace through a set of instructions summarized as follows:

> Subjects faced a series of decision rounds in which they were endowed with a specified number of tokens, which they invested between two markets. Market 1 was described as an investment opportunity in which each token yielded a fixed (constant) rate of output and each unit of output yielded a fixed (constant) return. Market 2 (the CPR) was described as a market that yielded a rate of output per token dependent upon the total number of tokens invested by the entire group. The rate of output at each level of group investment was described in functional form, as well as tabular form. Subjects were informed that they would receive a level of output from Market 2 that was equivalent to the percentage of total group tokens they invested. Further, subjects knew that each unit of output from Market 2 yielded a fixed (constant) rate of return.[15]

Subjects knew with certainty the total number of decision makers in the group, total group tokens, and that endowments were identical. Subjects knew that the more the group invested in Market 2, the lower the average and marginal returns from Market 2 would be. They did not know the exact number of decision rounds.

In the baseline experiments, subjects participated in a series of 20 decision rounds. After each round, subjects were shown a display that recorded (1) their profits in each market for that round, (2) total group investment in Market 2, and (3) a tally of their cumulative profits for the experiment. During the experiment, subjects could request, through the com-

FIGURE 1.

Experimental design

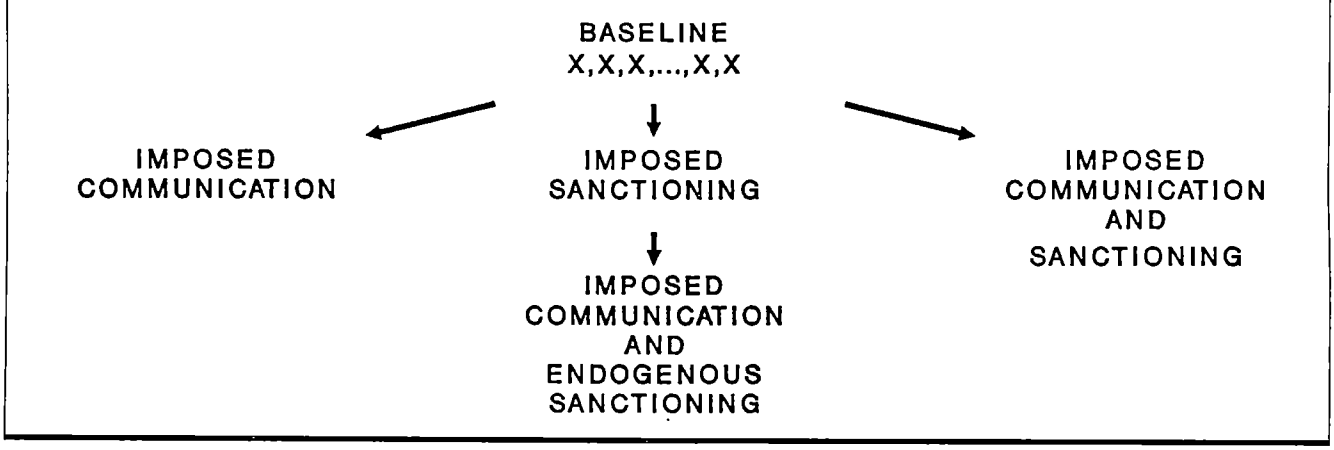


TABLE 2

Summary Results: Average Yield as a Percentage of Maximum

EXPERIMENTAL DESIGN	ROUND 1–5	6–10	11–15	16–20	21–25	26+
Baseline						
10TK[a]	51.5	34.7	34.4	35.6	37.1	29.6
25TK	−42.5	−12.4	10.3	32.0	—	—
One-shot communication 25TK	−40.9	−12.7	74.1	45.4	42.5	58.6
Repeated communication						
10TK	32.6	27.3	97.2	98.4	100.0	—
25TK	32.5	−14.4	74.1	75.0	68.9	—
Sanction 25TK	−35.7	−39.6	40.1	38.8	28.7	—
One-shot communication sanction 25TK	−.7	−27.0	86.8	86.3	82.5	77.8
One-shot communication 25TK						
No sanction chosen[b]	46.4	41.2	91.7	81.9	14.7	—
Sanction chosen[b]	−16.9	−5.1	92.5	91.6	89.9	93.8
Sanction chosen[c]	—	—	96.8	97.0	96.7	90.4

[a]TK corresponds to tokens per subject.
[b]Communication and sanctioning choice occurred after round 10.
[c]Communication and sanctioning choice occurred after round 1; the table displays this data beginning in round 11 for comparison purposes.

puter, this information for all previous rounds. Subjects received no information regarding other subjects' *individual* investment decisions or the number of rounds.

As shown in Table 2, in the first 10 rounds, the low-endowment treatment leads to an average net yield of 43%. In the high-endowment treatment, net yield averages −28%. This shows the importance of the endowment treatment out of equilibrium. In the last five rounds of the baseline experiments, the two treatments are very close—36% and 32%, respectively. These percentages are fairly close to the 38% predicted by the subgame consistent equilibrium, evidencing strategic learning on the part of the subjects.

Several characteristics of the individual experiments are important. Investments in Market 2 are characterized by a pulsing pattern: yield is reduced, investors reduce their investments in Market 2, and yields increase. For the high-endowment experiments, the low points in the pulsing pattern were at yields far below zero. There was some tendency for the variance in yields to decrease over the course of the experiments. At the aggregate level, the Nash prediction best describes our data. However, at the individual level we observed no experiment in which individual investments stabilized at the Nash equilibrium. In short, as predicted by both Hobbes's state-of-nature theory and subgame consistent equilibrium, individuals acting independently in a CPR game without communication or sanctioning do not solve the collective action problem they confront.

COVENANTS WITHOUT A SWORD: COMMUNICATION ALONE

Prior experimental evidence strongly supports the conclusion that communication in social dilemmas increases the frequency with which players choose and sustain joint income-maximizing strategies even when individual incentives conflict with such strategies. We shall examine the results from several types of communication experiments.

One-Shot Communication

Our first communication design was parallel to that of the high-endowment baseline game for the first 10 repetitions of the constituent game.[16] At the end of the tenth round, the subjects were informed that they would have a single opportunity of 10 minutes to discuss the decision problem. The instructions were:

> Some participants in experiments like this have found it useful to have the opportunity to discuss the decision problem you face. You will be given ten minutes to hold such a discussion. You may discuss anything you wish during your ten-minute discussion period, with the following restrictions: (1) you are not allowed to discuss side payments; (2) you are not allowed to make physical threats; (3) you are not allowed to see the private information on anyone's monitor.

After this opportunity to communicate, the subjects returned to the constituent game, which was then repeated up to 22 more times. The structure of the one-shot communication experiment is

$$X, X, X, \ldots, X, C, X, X, \ldots, X.$$

The subgame consistent and subgame perfect equilibrium outcome for the one-shot communication game is for each individual to invest 8 tokens in the CPR, the same as in the baseline. The maximum net yield is obtained if a total of 36 tokens are invested. Subjects are not allowed to invest fractional tokens; and the symmetric strategy to obtain the maximum return is halfway between everyone's investing 4 tokens and everyone's investing 5 tokens. If the subjects were to decide to invest either 4 or 5 tokens each, they would obtain 99% of maximum net yield in either case.

The transcripts of the discussion during the communication round reveal that subjects perceived their problem as involving two tasks: determining the maximal yield available and agreeing upon a strategy to achieve that yield. In all three groups, an agreement was reached; but no group found the optimal solution. Averaging over all experiments, yields were negative throughout the first 10 rounds (−27%). Once communication was allowed, net yields jumped to an average of 74% for the five rounds immediately following the communication time-out, then gradually decayed thereafter. The average net yield in the postcommunication phase was 55%. Defining a *defection* as an individual Market 2 investment greater than that agreed upon, we observe a defection rate of 25% during the decision rounds following communication. These results suggest that a single communication period enables participants to begin the process of adopting a joint strategy. However, the inability to communicate more often limits the durability of their agreements.

Repeated Communication

Our second design involves repeated communication in both the low- and high-endowment settings. At the outset, the constituent game was repeated for 10 rounds. After round 10, the subjects read an announcement similar to that for one-shot communication but informing them they would have an opportunity for discussion after *each* subsequent round. The subjects left their terminals and sat facing one another.[17] The structure of the repeated communication experiment is

$$X, X, \ldots, X, C\text{–}X, C\text{–}X, \ldots, C\text{–}X.$$

These experiments provide strong evidence for the power of repeated face-to-face communication. Subjects successfully used the opportunity to (1) calculate coordinated yield-improving strategies, (2) devise verbal agreements to implement these strategies, and (3) deal with nonconforming subjects through verbal statements. When allowed to communicate repeatedly, subjects greatly enhanced their joint yield and sustained this enhancement. In the *low*-endowment environment, net yields averaged 99% of optimum in the repeated communication phase with a 5% defection rate.[18] The *high*-endowment CPR game is a more challenging decision environment than the low-endowment one. While the subgame consistent equilibrium outcomes of the two games are identical, the disequilibrium implications of the 25-token (high-endowment) game change considerably. With 25 tokens, as few as three subjects investing all of their tokens can essentially ruin the CPR (bring returns below w), while with 10 tokens, it takes seven out of eight subjects to accomplish this much damage. Net yields in the repeated communication phase averaged only 73% of optimum, with a 13% defection rate.

Repeated communication enabled subjects to discuss defections and to cut the defection rate on agreements significantly. In all communication experiments, subjects offered and extracted promises of cooperation, thereby increasing their joint yield significantly above that obtained prior to communication. Discussions went well beyond discovering the level of investments that would generate maximum yields. A striking aspect of the discussion rounds was how rapidly the subjects, who had not had an opportunity to establish a well-defined community with strong internal norms, were able to devise their own agreements and verbal punishments for those who broke those agreements. These verbal sanctions had to be directed at unknown defectors, since subjects' decisions were anonymous. Subjects detected defection solely through aggregate investments. In many cases, statements like "Some scum-bucket is investing more than we agreed upon" were a sufficient reproach to change defectors' behavior. However, verbal sanctions were less effective in the 25-token environment.

The evidence from the one-shot and the high-endowment designs suggests why individuals in many field settings may not rely only on face-to-face communication. When repeated discourse is infeasible or when the actions of one or a few individuals can be a strong disequilibrating force, individuals who have the capacity to agree to sanction one another, as well as communicate with one another, might well want to reinforce their covenants with their own swords. While subgame consistency predicts that individuals in such settings would not sanction one another, endogenous sanctioning is frequently observed in field settings. We now turn to an examination of whether sanctioning behavior will occur in our laboratory environment and its effects on behavior.

SWORD WITHOUT COVENANTS: SANCTIONING ALONE

Experiments in this design began like high-endowment baseline experiments with the exception that after each round, subjects received *individual* data of all decisions.[19] This information was given by subject number, thus maintaining anonymity. Our sanctioning mechanism required that each subject incur a cost (a fee) in order to sanction another. In our first

sanctioning design, after round 10, subjects were given an announcement summarized as follows:

> Subjects were informed that in all remaining rounds each would be given the opportunity to place a fine. Each subject could levy one fine at a specified fee. The subject fined would pay a fine of a specified amount. It was possible for a single subject to be charged multiple fines. After each round, each subject filled out a fining form. These forms were collected and tallied by the experimenter, who then reported the results privately to each subject. Note that any subject who was fined did not know the identity of those who imposed the fine. At the end of the experiment, the experimenters subtracted from subjects' total profits the total of all fees and all fines.

The actual fees ranged from 5¢ to 40¢, the fines from 10¢ to 80¢ cents. The fee-to-fine ratio was either .25 or .50. After subjects read the announcement, we answered questions regarding the implementation of the procedure. No discussion was held on why the subjects might want to use the procedure or its possible consequences. This created an experimental setting as close as possible to the noncooperative assumptions of no communication and no capacity to engage in enforceable agreements. The structure of the imposed sanctioning design is

$$X, X, \ldots, X\text{–}S, X\text{–}S, X\text{–}S, \ldots, X\text{–}S, X.$$

The principle results from our sanctioning experiments are summarized as follows.[20] Across all eight experiments, net yield rose from −38% before the imposition of sanctioning to 37% after. When one subtracts the costs of fees and fines, however, average net yield increased to only 9%. Thus, sanctioning alone is not an efficient institution. Besides these quantitative results, we draw the following qualitative conclusions:

1. Significantly more sanctioning occurs than predicted by subgame consistency, and the frequency is inversely related to cost.
2. Sanctioning is primarily focused on heavy CPR investors.
3. There is a nontrivial amount of sanctioning that can be classified as error, lagged punishment, or "blind" revenge.

We observed 176 instances of sanctioning across the eight experiments. In no experiment did we observe fewer than 10 instances. The frequency of sanctions was inversely related to the cost of imposing the fine and dramatically increased with the stiffness of the fine. Further, our results, although reminiscent of strategy 2, did not strictly support the conclusion that subjects were playing an equilibrium of this form. Except for one experiment, where net yield was over 95%, yields were too low, and sanctioning levels too high, to be consistent with imperfect equilibria.

The second and third results relate to the reasons for sanctioning. From postexperiment interviews and personal observations, we offer four explanations for the higher-than-predicted level of sanctioning:

1. *One-round punishment.* The person fined was the highest, or one of the highest, investors in the previous round.
2. *Lagged punishment.* The person fined was one of the highest investors in the CPR in a round prior to the previous one.
3. *Blind revenge.* The person fined was a low CPR investor and was fined by a person fined in a previous round.
4. *Error.* No obvious explanation can be given for the action (trembling hand).

In summary, 77% of all sanctioning was aimed at investors who in the previous round were above-average investors in the CPR. An additional 7% was aimed at subjects who had been heavy investors in the CPR in earlier (but not the most recent) rounds. We classify 5% as blind revenge and the remaining 11% as errors.[21]

The evidence from these experiments suggests why individuals in field settings might not want to rely on sanctioning alone. Individuals who have the capacity to sanction one another without the ability to communicate about joint strategies and the use of sanctions face an insuperable handicap to increasing efficiency. Finally, we return to the question whether communication and sanctioning together foster sustainable high yields.

COVENANTS WITH A SWORD: COMMUNICATION AND SANCTIONING

Our last two decision environments investigate the consequences of combining a one-shot opportunity to communicate with either an experimenter-imposed sanctioning mechanism or an opportunity to decide whether or not to adopt a sanctioning mechanism endogenously. These experiments began like those in the design with sanctioning alone. After round 10, subjects were given an announcement that they would have a single 10-minute discussion period. In experiments with an imposed-sanctioning mechanism, subjects were also given an announcement (prior to discussion) similar to that of the sanctioning-alone environment. In experiments where subjects had an opportunity to choose a sanctioning mechanism, the announcement informed them that at the end of 10 minutes they would vote on whether to institute a sanctioning mechanism and if so, what the level of fines would be. The only restriction on the sanctioning mechanism was that the fee-to-fine ratio was ½. The voting rule was strict majority, with the status quo a repeated baseline experiment without a sanctioning mechanism.

Imposed Communication and Sanctioning

The structure of the three experiments in this design is

X, X, . . . , X, C, X–S, X–S, . . . X–S, X.

In the first experiment, the participants rapidly focused on the problem of deciding upon a joint investment strategy. They spent most of their 10 minutes calculating various options to ensure that they had discovered an optimal strategy. They decided to invest 4 tokens each in Market 2 and the remaining 21 tokens in Market 1. Further, they agreed to fine one another if anyone put more than 4 tokens in Market 2. One subject said, "If everyone puts in 4 tokens, we are going to be making 42 cents more money in the individual accounts. This is the highest." Another said, "Does everyone agree to this? OK, now we have agreed that everyone will put 4 tokens in and if anyone puts any more in, we are all going to fine them. Is that all agreed, now?" With this specific agreement to which everyone nodded assent, the subjects returned to their terminals and made investments for 16 more rounds without a single defection nor any use of the sanctioning mechanism. They obtained 98% of maximum net yield from the CPR and did not waste resources on fees or fines.

In the second experiment, the subjects did not find the optimal strategy but devised a complex rotation system to ensure that they all received what they thought would be maximal returns. They decided that each subject would invest 6 tokens in Market 2 and that the right to invest 2 more tokens each round would be rotated first to Subject 1, then to Subject 2, on through Subject 8. One subject suggested that they not fine at all, but another argued: "No, let's fine anyone who breaks our rules. If they break our rules, then we should fine 'em!" After further discussion, the subjects agreed that they would use the sanctioning mechanism to fine anyone who deviated from "their rules." For two rounds they kept their agreement. On round 13, one subject invested 7, rather than 6, and was immediately fined by one of the other subjects. On round 19, two subjects invested one more token than agreed upon and each was immediately sanctioned by another subject. No more defections were attempted and no more fees were paid to assess fines. In this experiment, the subjects achieved 86% of maximum net yield (since they had miscalculated the optimum). Their net return dropped to 79% when fees and fines were deducted from their earnings.

In the third experiment, the subjects never discussed the possibility of devising a joint strategy even though they mentioned how the overinvestment of some of the subjects during the first 10 rounds had made it difficult for the rest of them. The closest they got to an agreement was to discuss fining those who were obviously overinvesting, for example, "those who invested over 21 tokens in Market 2." A considerable amount of their discussion time was wasted in awkward silence. They finally asked whether they had to sit there the entire 10 minutes. After verifying that no subject wanted to use the remaining 2 minutes of their time for further discussion, the experimenters let the subjects return to their terminals. Following the communication period, the subjects achieved an average of 70% of net yield from the CPR, up from −14% in prediscussion rounds. A total of twenty 40¢ fees were paid to impose the same number of 80¢ fines on other subjects. The fines were directed toward subjects who had invested heavily in Market 2 in the prior round. Net yield fell to 24% with fees and fines deducted from earnings.

Imposed Communication and Endogenous Sanctioning

In order to make the choice of a sanctioning mechanism meaningful, subjects in this decision environment were randomly drawn from the pool of subjects from our imposed sanctioning design. The structure of the four experiments in this design is

X, X, . . . , X, C, X, X, . . . , X or

X, X, . . . , X, C, X–S, X–S , . . . , X–S, X.

Subjects decided upon a joint investment strategy and established a sanctioning mechanism in only two of the four experiments. In one they chose a fee-to-fine ratio of 10¢/20¢ and in the other a ratio of 20¢/40¢. Net yield averaged 91% of maximum following the communication round. The level of defection from their agreements was very low. Net yield fell slightly to 86% when fees and fines were deducted.

In one of the experiments where subjects rejected the adoption of a sanctioning mechanism, they did adopt an agreed-upon joint strategy to invest four tokens in the CPR (a joint strategy very close to optimal). In the first few rounds following discussion, they sustained their agreement; but by round 16, four subjects invested more than their agreement. From then on, the level of defections steadily rose. Overall, they had an average net yield of 36% and a defection rate of 42%, representing the highest defection rate of any communication experiment where an agreement was reached. The other experiment in which a sanctioning mechanism was not chosen is an outlier. Across all of our CPR experiments with the baseline institution, this is the only experiment where yields in the first 10 rounds were essentially maximal. When given the opportunity to discuss the decision problem and choose a sanctioning mechanism, the group agreed that they did not need a mechanism and that no one should try to get "greedy," that is, invest too much in the CPR. The group held together for a few rounds, after which yields began a gradual decline. This decline was due primarily to a gradual increase in Market 2 investments by two subjects. By round 25, net yield had dropped to 56% of optimum. Net yield for all rounds following the discussion session averaged 76%, compared to 90% prior to round 10.

We have traced back to the specific sanctioning/no communication experiment in which each of these subjects participated. Of the 32 subjects in these four experiments, 18 voted for, and 14 voted against, the

implementation of a sanctioning mechanism. Of the 14 who voted *no*, 11 had previously participated in a sanctioning experiment with a fee-to-fine ratio of 20¢/80¢. Of the 18 who voted *yes*, only 3 had been in a 20¢/80¢ design. We infer from this result that the high level of sanctioning activity in the 20¢/80¢ design, the lack of overall efficiency gains and the presence of blind revenge combined to impede the willingness of participants to choose a sanctioning mechanism.

It is possible that the experience of the first 10 rounds of the constituent game had an effect on mechanism choice. To examine this possible hysteresis effect, two additional experiments were conducted. In these two experiments, the opportunity to communicate and to adopt a sanctioning mechanism was available at the outset. The structure of these two experiments is

C, X, X, . . . , X or C, X–S, X–S, . . . , X–S, X.

In both of these experiments, the subjects quickly agreed to an investment strategy and a sanctioning mechanism to punish defectors. Across the two experiments, net yields averaged 95%—94% with fees and fines included.

The payoff consequences of selecting or not selecting a sanctioning mechanism were very different across the experiments in this design. The groups choosing some form of sanctioning institution earned average yields of 93% in the postdiscussion phase. Indeed, yields this high suggest that this set of institutions, endogenously chosen, approximate the conditions necessary for a cooperative game. The groups not choosing some form of sanctioning institution earned average yields of only 56%, with serious decay.

The results from this set of communication and sanctioning experiments suggest that some subjects can find yield-improving joint strategies, design a sanctioning mechanism, use the sanctioning mechanism, and achieve a high rate of conformance to their joint strategies. On the other hand, prior negative experience with institutions that individuals view as punitive and inefficient is conducive neither to the design of better institutions nor to a willingness to use them.

CONCLUSIONS: SELF-GOVERNANCE IS POSSIBLE

The inconsistency between predicted results and observed behavior in prior research stimulated this research. In one-shot and finitely repeated games, communication alone is not predicted to have an effect on behavior. Earlier experimental studies of social dilemmas have, however, shown that communication alone leads to more efficient outcomes. We confirm these results in complex CPR environments.

With regard to communication alone, we obtain the following results (summarized in Table 3):

1. In the low-endowment CPR environment, average net yield increased from 35% (when no communication was allowed) to 99% (when communication was allowed on a repeated basis).
2. In the high-endowment CPR environment, average net yield increased from 21% (when no communication was allowed) to 55% (when communication was allowed only once) to 73% (when communication was allowed on a repeated basis).

These results raise several puzzles. Because the subgame consistent equilibrium prediction for games with communication (one-shot or repeated) is the same as that for games without communication, opportunities for "mere jawboning" should make no difference; yet they do. Further, the equilibrium prediction is the same for both low- and high-endowment environments. The high-endowment environment, however, exhibits lower net yield and fosters less effective communication. The finding that CPR appropriators in the field invest substantial resources in sanctioning activities stimulated our exploration of sanctioning behavior in the laboratory. With an imposed sanctioning institution and no communication we find the following:

3. Subjects are willing to pay a fee to place a fine on another subject far more than was predicted.
4. In the high-endowment environment, average net yield increases from 21% with no sanctioning to 37% with sanctioning. When the costs of fees and fines are subtracted from average net yield, however, net yield drops to 9%.

Thus, subjects overuse the sanctioning mechanism, and sanctioning without communication reduces net yield. The finding that CPR appropriators in the field invest substantial resources in both communication and sanctioning activities stimulated our exploration of these joint behaviors in the laboratory. Examining only the high-endowment environment, we find the following:

5. With an imposed sanctioning mechanism and a single opportunity to communicate, subjects achieve an average net yield of 85%. When the costs of fees and fines are subtracted, average net yield is still 67%. These represent substantial gains over the baseline, where the net yield averaged 21%.
6. With the right to choose a sanctioning mechanism and a single opportunity to communicate, subjects who adopt a sanctioning mechanism achieve an average net yield of 93%. When the costs of fees and fines are subtracted, average net yield is still 90%. In addition, the defection rate from agreements is only 4%.
7. With the right to choose a sanctioning mechanism and a single opportunity to communicate, subjects who do not adopt a sanctioning mechanism achieve an average net yield of only 56%. In addition, the defection rate from agreements is 42%.

Thus, subjects who use the opportunity to communicate to agree to a joint investment strategy and

TABLE 3

Aggregate Results All Designs (%)

EXPERIMENTAL DESIGN	AVERAGE NET YIELD CPR	AVERAGE NET YIELD CPR (MINUS FEES AND FINES)	DEFECTION RATE
Baseline			
10TK[a]	34	—	—
25TK	21	—	—
One-shot communication 25TK	55	—	25
Repeated communication			
10TK	99	—	5
25TK	73	—	13
Sanction 25TK	37	9	—
One-shot communication sanction 25TK	85	67	1
One-shot communication 25TK			
No sanction chosen	56	—	42
Sanction chosen	93	90	4

Note: All computations are for periods in which the treatment was in effect. Nash equilibrium for all designs is 39% net CPR yield.
[a]TK corresponds to tokens per subject.

choose their own sanctioning mechanism achieve close-to-optimal results. This is especially impressive in the high-endowment environment, where defection by a few subjects is very disruptive. For those who predict cooperation in repeated settings based on trigger strategies, our findings are not supportive. In no experiment where one or more subjects deviated from an agreed-upon joint strategy did the subjects then follow a trigger strategy of substantially increasing their investments in the CPR.[22] In fact, in some experiments where one or more subjects deviated from an agreed-upon joint strategy, some subjects subsequently *reduced* their investments in the CPR. When subjects discussed the problem of how to respond to one or more free-riders, they overtly rejected the idea of dumping all of their tokens into the CPR.[23] To return to our starting point, namely, Hobbes's assertion, these experiments suggest that covenants, even without a sword, have some force, while swords without a covenant may be worse than the state of nature. Best of all the conditions we examined are covenants with an *internal* sword, freely chosen or made available as an institutional option.[24]

Two major implications follow from the results. The first relates to policy analysis. Policymakers responsible for the governance and management of small-scale, CPRs should *not* presume that the individuals involved are caught in an inexorable tragedy from which there is no escape. Individuals may be able to arrive at joint strategies to manage these resources more efficiently. To accomplish this task, they must have sufficient information to pose and solve the allocation problems they face. They must also have an arena where they can discuss joint strategies and perhaps implement monitoring and sanctioning. In other words, when individuals are given an opportunity to restructure their own situation, they frequently—but not always—use this opportunity to make credible commitments and achieve higher joint outcomes without an external enforcer. We cannot replace the determinate prediction of *no cooperation* with a determinate prediction of *always cooperate*. Our findings challenge the Hobbesian conclusion that the constitution of order is only possible by creating sovereigns who then must govern by being above subjects, by monitoring them, and by imposing sanctions on all who would otherwise not comply.[25]

The second major implication relates to behavioral theory. In finitely repeated social dilemma experiments, a wide variety of treatments that do not change the theoretically predicted subgame consistent equilibrium outcomes do change subjects' behavior. This raises a *substantial* question whether our subjects conceptualize their decision task in the way theorists do. For instance, if subjects believe the game is being repeated according to some exogenous probabilistic mechanism, then there are equilibria supporting more cooperative behavior if the subjective continuation probability is not too low. Or it may be that subjects are acting as boundedly rational players in the sense of Selten, Mitzkewitz, and Uhlich (1988). In this case, the observed improvement in yield could be the result of boundedly rational equilibrium, as Selten, Mitzkewitz, and Uhlich observe in a duopoly context. We intend to explore these intriguing possibilities in future research.

Notes

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1. Harsanyi and Selten add that in real life, "agreements may be enforced externally by courts of law, government agencies, or pressure from public opinion; they may be enforced internally by the fact that the players are simply unwilling to violate agreements on moral grounds and know that this is the case" (1988, 3). To model self-commitment using noncooperative game theory, the ability to break the commitment is removed by trimming the branches that emanate from a self-commitment move to remove any alternative contrary to that which has been committed. In a lab setting, this would mean changing the structure of the alternatives made available to subjects after an agreement, which was not done.

2. See, e.g., Campbell 1985; Hardin 1971, 1982; McLean 1981; Moore 1987; Taylor 1987. The term *social dilemma* is introduced by Dawes 1975, 1980.

3. See Bianco and Bates 1990 for a theoretical analysis of the capabilities and limits of assigning leaders strong sanctioning powers and Samuelson et al. 1986 for an experimental investigation of the choice of a Leviathan-like mechanism to solve social dilemmas.

4. See Gardner, Ostrom, and Walker 1990 and E. Ostrom, Gardner, and Walker n.d. for a detailed exposition of the terms we use and the models we have developed.

5. See Berkes 1989; National Research Council 1986; E. Ostrom 1990; Pinkerton 1989; and Wade 1988 for a discussion of successful, failed, and fragile efforts to self-organize and govern, small-scale CPRs. Michael Hechter's (1987) recent synthesis of self-organizing capabilities within many social groups provides evidence across other domains (see also Williamson 1975, 1985). See Kreps 1990 for a useful synthesis of literature relevant to endogenous commitments within firms.

6. The extensive literature on one-shot communication in dilemma games includes Bornstein et al. 1989; Braver and Wilson 1984, 1986; Caldwell 1976; Dawes, McTavish, and Shaklee 1977; Dawes, Orbell, and van de Kragt 1984; Dawes, van de Kragt, and Orbell 1988; Edney and Harper 1978; Jerdee and Rosen 1974; Kramer and Brewer 1986; Orbell, Dawes, and van de Kragt 1990; Orbell, van de Kragt, and Dawes 1988, 1991; van de Kragt et al. 1983, 1986.

7. For a discussion of relevant literature, see Isaac and Walker 1988, 1991; E. Ostrom and Walker 1991.

8. See n. 5.

9. Investment in the CPR beyond the maximum net level is termed *rent dissipation* in the literature of resource economics. One can interpret this environment as a limited access CPR (see, e.g., Clark 1980; Cornes and Sandler 1986; Negri 1989).

10. This specification actually has a number of other possible interpretations. For instance, if one defines $F(\Sigma x_i)/\Sigma x_i = y$ and y to be a public good, then one has the payoff functions for a voluntary contribution mechanism as in Isaac and Walker 1988. Alternatively, one can define y in the same expression to be an externality, in which case one has payoff functions for Plott's experiments on externalities in product markets (Plott 1983). For further details, see Ledyard 1991.

11. During recruitment, subjects are told they will participate in a one-to-two hour decision-making experiment. Although the exact endpoint is not revealed, it is explicitly bounded from above. Further, all subjects have experienced the boundedness of an experiment that lasted between 10 and 30 rounds.

12. We use the word *fine* not in the context of redistribution. What is crucial here is that real resources are used up, not just redistributed, by efforts to sanction.

13. Besides the symmetric imperfect equilibria we have given, there are many asymmetric equilibria. Take any permutation of the identity matrix, e.g., 1 sanctions 2, 2 sanctions 3, etc. Then these permuted sanctions also support the same outcome as strategy 2 does. Notice that if mistakes are what cause deviations, then an equilibrium like strategy 2 will generate n fines every time a mistake takes place—considerably more than the zero fines generated by the subgame consistent equilibrium strategy 1.

14. Net yield accrued as a percentage of maximum = (return from Market 2 − opportunity costs of tokens invested therein)/(return from Market 2 at the optimum − opportunity costs of tokens invested therein). Opportunity costs equal the potential return that could have been earned by investing the tokens in Market 1.

15. A complete set of instructions is available upon request. In high-endowment experiments, subjects were informed that their cash payoff would be one-half of the "lab dollars" earned in the experiment. This was done so that total payments in high- and low-endowment treatments would be approximately equal. At the end of all experiments, subjects were paid privately (in cash) their individual earnings. All subjects had participated previously in an experiment using the constituent game environment. The number of rounds in earlier experiments had varied from 10 to 20. Subjects were recruited randomly from this pool of experienced subjects to ensure that no prior experimental group was brought back intact. We provide a detailed account of behavior in the constituent game environment in Walker, Gardner, and Ostrom 1990, 1991.

16. In the one-shot communication experiments, subjects received information on individual decisions after each round to facilitate our comparing results in designs with sanctioning either alone or in conjunction with one-shot communication added. Information was given by subject number, thereby preserving anonymity. This added information had no significant impact on observed yields.

17. As in the one-shot communication setting, each person was identified with a badge that was unrelated to their player number. This facilitated player identification in our transcripts. If unanimous, players could forgo discussion.

18. We also conducted a series of costly communication experiments, in which subjects had to pay in advance for opportunity to communicate. See E. Ostrom and Walker 1991 for a detailed discussion. See also Isaac and Walker 1988, 1991.

19. Earlier experiments focusing on sanctioning mechanisms without communication include Yamagishi 1986, 1988.

20. A comparison of the initial 10 rounds to the initial 10 rounds of baseline suggests that the addition of anonymous information about individual decisions had no impact on investments.

21. A second set of sanctioning experiments was conducted as a check on the robustness of our original design. Readers of our earlier results conjectured that the lack of a significant improvement in net yield with the introduction of a sanctioning mechanism in our initial design could be due to a hysteresis tied to the decisions in the first 10 periods, periods in which there was no sanctioning mechanism. In our second design, three new experiments were conducted, with the sanctioning mechanism introduced prior to the first decision period. The fee-to-fine ratio was 40¢:80¢. Subjects used fines repeatedly in all three experiments. The results from our second set of experiments are consistent with those from our first design. There was no persistent yield-improving behavior that could be tied to the introduction of the sanctioning mechanism. In fact, when costs of fees and fines are incorporated we found a negative impact on net benefits.

22. It is well known that inducing trigger strategy behavior in subjects is extremely difficult. For a recent attempt, see Sell and Wilson 1991.

23. One player resisted the suggestion made by another

player to dump all of the tokens into the CPR by stating, "We screw ourselves, too."

24. We did not explore the effect of an external agent assigned responsibility to monitor and sanction behavior because in this research we were interested in the feasibility of internal enforcement.

25. See V. Ostrom 1987, 1989, 1991 for an elucidation of an alternative theory to Hobbes's.

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RATIONALITY, MORALITY, AND EXIT

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The morality and rationality issue is explored from an Axelrod-type perspective; that is, it is discussed in terms of recurrent-prisoner's-dilemma-type games and behavioral strategies or programs for playing them. We argue that intuitive notions of rationality and morality can be shown to be mutually compatible if two assumptions are made: (1) that morality is specified as a general behavioral disposition or program whose rationality is to be determined in comparison to alternative behavioral programs and (2) that the recurrent game is specified as a prisoner's dilemma game with an exit option. The results of a simulation experiment are presented, showing that a "moral program" (specified as one that never defects, but exits in response to an opponents defection) is successful in competition with a variety of alternative programs, including Tit for Tat.

> The reconciliation of morality with rationality is the central problem of modern moral philosophy. It is easy to achieve this reconciliation by fiat, to select a conception of rationality and a conception of morality so that the two fit. What is of greater interest and value is to attempt this reconciliation beginning with our intuitive conceptions of rationality and morality.
>
> —David Gauthier

The discussion on how rationality and morality can be systematically related to each other has a long tradition in moral philosophy. More recently efforts have been made to bring a new perspective to this issue by recasting it in terms of the prisoner's dilemma setting of game theory. The most elaborate contribution of this kind is probably David Gauthier's *Morals by Agreement* (1986), which is based on the presumption that the structure of moral problems is "represented most clearly by the Prisoner's Dilemma" and that "whatever force moral claims have derives, and derives entirely, from their role in overcoming the structural problem of interaction represented by the Prisoner's Dilemma" (Gauthier 1988, 176).

Casting the *morality and rationality issue* in terms of the prisoner's dilemma has helped to shift a debate that has often been rather elusive to a more operational level, since it requires that such attributes as morality and rationality be translated into behavioral strategies or programs (Vanberg 1988; Vanberg and Buchanan 1990). Another contribution (though not directly addressing the morality and rationality issue) of particular significance in this context is Robert Axelrod's *Evolution of Cooperation* (1984), the core part of which is a computer tournament among alternative strategies/programs for playing iterated prisoner's dilemmas.

We shall approach morality and rationality from an Axelrod-type perspective; that is, we shall discuss this issue in terms of recurrent games and behavioral strategies/programs for playing these games. More specifically, we shall argue that our intuitive notions of rationality and morality can be shown to be mutually compatible *if* appropriately specified. Two assumptions are at the core of our argument. First, we presuppose that the *rationality of morality issue* is appropriately discussed at the level of general behavioral dispositions or programs, as opposed to the level of particular actions. Second, we assume that an important aspect of what we mean by *morality* can be more adequately discussed in the context of a recurrent game that includes an *exit option*, rather than in the context of a recurrent prisoner's dilemma game that, in its pure form, rules out the possibility of exit from some or all rounds of the game.

Though the focus of this paper is on the prisoner's dilemma notion of morality, it should be noted that this interpretation is, in certain respects, both wider and narrower than our intuitive understanding of morality. As Ullmann-Margalit and Buchanan have pointed out in their respective comments on Gauthier's argument, it is wider insofar as the set of prisoner's-dilemma-type problems includes situations that we may not consider as moral problems, for example, the compliance problem faced by members of an industrial cartel (Buchanan 1988, 75–79; Ullmann-Margalit 1977, 41–42). The prisoner's dilemma notion of morality is narrower than our intuitive understanding of moral problems, since the latter includes situations that are not characterized by the strategic interdependence of the prisoner's dilemma. In other words, what we shall discuss as moral problems lies in the intersection of two sets: our intuitive notion of morality and prisoner's dilemma problems. All we need claim for our purpose is that the prisoner's dilemma notion captures a relevant aspect of morality as commonly understood.[2]

We discuss morality as a behavioral disposition and the concept of rationality as it applies to dispositions as opposed to particular acts. We look at the implications of adding an exit option to the cooperate and defect options that constitute the prisoner's dilemma and propose a definition of morality as a behavioral strategy/program, based on the two previously discussed assumptions. We present the results of a series of simulation experiments that measure the

performance of a moral program in competition with a limited set of alternative programs and conclude with a brief discussion on the scope and the limits of the exit option in social interaction.

RATIONALITY AND BEHAVIORAL DISPOSITIONS

One aspect of Axelrod's tournament approach to the repeated prisoner's dilemma is its implicit shift of perspective from the level of situational choice to the level of programmed choice. By comparing alternative behavioral strategies/programs according to their relative performance over a sequence of encounters, one moves the question, What is rational? from the level of single, particular choices to the level of *programs* that provide generalized instructions for how to act in particular instances. The significance of this shift of focus from single choices to behavioral programs lies in the fact that a program may be rational in the sense of generating better overall payoffs than any feasible alternative program but may call for single actions that if considered separately, would appear irrational.

The relevant distinction here is analogous to the distinction between constitutional and subconstitutional choices in a collective choice setting. Constitutional choices are choices *among* rules/constraints; subconstitutional choices are choices *within* these rules/constraints. In a similar sense we may draw a distinction between an individual's *personal constitution* (the amalgam of that person's general dispositions and behavioral rules, or, in ordinary language, *character*) and choice behavior in particular instances. To the extent that such a distinction can be plausibly drawn, we can meaningfully separate two levels for which the question of rationality can be raised: the level of specific situational choices and the personal constitutional level. At the personal constitutional level, the relevant alternatives are not particular actions but, instead, general dispositions or behavioral programs.

Leaving aside the issue of how an individual's personal constitution is actually formed (esp. whether and to what extent it is subject to explicit choice, as opposed to unreflected behavioral conditioning), we want to suppose that our intuitive notions of morality typically refer to attributes of dispositions, rather than of particular choices. In other words, we contend that the ordinary meaning of morality is best modeled at the personal constitutional level, rather than at the level of particular situational choices.[3] We shall argue that our intuitive notion of morality can be translated into behavioral strategies that can be shown to be successful in competition with a set of alternative programs. Successful programs do not necessarily perform best in every situation, but, rather, are such that no feasible alternative program generates systematically greater payoffs. Such programs may be rationally chosen.

Readers familiar with David Gauthier's *Morals by Agreement* will notice that the purpose of our enterprise is basically the same as his. Fundamental to Gauthier's argument is the distinction between what he calls *straightforward* and *constrained* maximization—which correspond to our distinction between situational rationality and personal constitutional rationality. Straightforward maximization, in Gauthier's terminology, means to maximize (i.e., choose the most preferred alternative) at the level of particular choices (1986, 15). Constrained maximization, by contrast, means to maximize at the personal constitutional level (i.e., choose the most advantageous behavioral disposition from among potential alternative dispositions).[4]

In Gauthier's account, the morality issue is clearly a matter of general dispositions, not of single choices. Accordingly, whether morality—understood as a disposition to accept moral constraints (1986, 15)—is rational has to be judged at the level of behavioral dispositions, not at the level of particular actions (p. 182). A morally disposed person may well—and typically will—choose nonoptimally on particular occasions in the sense of not choosing what, under separate consideration of the specific case, would be the maximizing alternative. Yet this does not make the moral disposition irrational so long as no alternative disposition (even the disposition to act as a straightforward maximizer) could be expected to render systematically higher payoffs over a sequence of actions.[5]

PRISONER'S DILEMMA AND THE EXIT OPTION

In Axelrod's simulation experiments, and generally in discussions of strategy choice in recurrent prisoner's dilemma games, it is implied that the players *must play*. Players simply cannot avoid playing the game. This assumption is, of course, part of the original prisoner's dilemma story: the two prisoners can only choose between *confess* and *not confess*; they cannot exit from the situation. The restriction of choice to the alternatives, *cooperate* and *defect*, is inherent to the structure of a prisoner's dilemma game; and it is, as such, also an essential element of the structural definition of a recurrent prisoner's dilemma game. However, in the wake of its generalized reception in the social sciences, the notion of the iterated prisoner's dilemma has come to be applied to a wide range of social settings and interaction problems for which the *no exit* assumption is clearly not appropriate. And we suppose that many of the social interaction problems (to which the notion of morality is commonly applied) arise in contexts where the persons involved actually have viable exit options in the sense that they can refuse to continue to interact with their respective counterparts. We shall work out some of the implications that follow if

the presence of an exit option is systematically taken into account.

To be sure, allowing players to choose whether or not to play a particular prisoner's dilemma game changes the nature of the game. In the prisoner's dilemma game with an exit option the payoff structure for the intersections of the players' cooperate and defect options remain the same. But the availability of an exit option changes the overall structure in a way that may eliminate the characteristic features of the prisoner's dilemma setting. For this very reason, the introduction of an exit option would be illegitimate if proposed as a contribution to the technical debate on the iterated prisoner's dilemma.[6] However, our principal interest is not the formal characteristics of the repeated prisoner's dilemma but the substantive relationship between rationality and morality. Our analysis is centered on the recognition that the prisoner's dilemma notion captures an essential aspect of moral problems but that the genuine repeated prisoner's dilemma, as technically defined, may not be the most appropriate model for the problem we seek to understand.[7]

The principal effect of adding the option to *refuse to play* to the strategies available in the original prisoner's dilemma is that the defect strategy may no longer be strictly dominant. What is particularly important for our purpose, is that adopting a disposition not to defect is no longer equivalent to becoming an unconditional cooperator (Figure 1).

With the inclusion of an exit option, the payoffs of exit become an important determinant of the viability of game strategies. In practice, the net benefits of exit depend on the availability of alternatives (or, more specifically, on the expected payoffs from those alternatives), whether such alternatives exist in the form of potential interactions with other players or in solitary activity.[8] In a competitive market context, for instance, the easy accessibility of alternative options decreases the opportunity cost of exit and reduces a player's dependency on any particular counterpart. In fact, one virtue of market competition lies in its tendency to reduce the cost of exit from, and consequently the dependency on, transactions with particular players. Conversely, where competition and the accessibility of alternatives is restricted, the costs of exit tend to be increased.

Figure 1 embodies the assumption that the costs and benefits of exit are such that the expected payoffs from choosing the *do not play* alternative are higher than those resulting under mutual defection but lower than those expected under mutual cooperation. The main thrust of our argument rests on this ordinal ranking of expected payoffs from mutual cooperation, mutual defection, and exit. There are, to be sure, circumstances conceivable under which the ranking of the payoffs from mutual defection and exit would be reversed. However, the potential occurrence of such circumstances does not invalidate the fundamental logic of our argument. We shall return to this issue in the concluding section.

MORALITY AS A BEHAVIORAL PROGRAM

In a short paragraph of his *Evolution of Cooperation*, Axelrod raises the question of whether Tit for Tat, in addition to being "an effective strategy for an egoist to use," is also "a moral strategy for a person . . . to follow" (1984, 136). His answer is that at least if measured against our everyday intuitions, "reciprocity does not seem to be the height of morality" (p. 137). He suggests, however, that Tit for Tat may well be the best we can hope for in moral affairs. Principles like the Golden Rule or the Kantian categorical imperative (which are widely regarded as exemplary moral standards) seem to require us to adopt the nonviable strategy of unconditional cooperation. Such a strategy not only is detrimental to the person adopting it but also "tends to spoil the other player" (p. 136) and thus allows noncooperative, exploitative programs to flourish. A disposition to cooperate unconditionally is not protected against exploitative programs and will tend to be selected against in a competitive, evolutionary environment. Furthermore, it not only promotes its own elimination but is also detrimental to other "friendly" programs because it nourishes exploitative programs. By contrast, Tit for Tat does well for itself and is actively supportive of a moral environment. It rewards cooperation and punishes defection. Therefore, Axelrod concludes, although its eye-for-an-eye message gives reciprocity a "slightly unsavory taste," "a moral person couldn't do much better" (p. 137).

If, as Axelrod suspects, Tit for Tat seems somewhat at odds with our intuitive notion of morality, the reason probably lies in the fact that *defection* is included in its behavioral repertoire. When we think of defection as lying, not keeping promises, defrauding, stealing, exploiting, and so on, it seems, indeed, difficult to attach the attribute *moral* to a behavioral program that includes such options even if they are exhibited only in response to an opponent's prior defection.[9] One obvious problem with such responses in kind is that they send potentially ambiguous signals to opponents and (perhaps more importantly) to third parties. They send ambiguous signals in all cases where it may not be totally obvious to opponents and onlookers whether the defection is a retaliatory act of an otherwise moral program or simply defection of an exploitative program. Since behavioral programs or dispositions are not directly observable but can only be inferred from observed responses in particular situations, any use of defection options like the aforementioned examples raises the information costs for deciphering what kind of program is behind a person's observed actions. This, in turn, may invite attempts to camouflage as retaliatory sanctions what are, in fact, exploitative defections.

If morality as a behavioral program is not to be doomed from the outset, it can only be a program of *conditional* cooperation, that is, a program that dic-

FIGURE 1.

Payoffs for a Prisoner's-Dilemma-with-Exit Game

Individual 1 \ Individual 2	Cooperate	Defect	Withdraw
Cooperate	R, R	S, T	N, N
Defect	T, S	P, P	N, N
Withdraw	N, N	N, N	N, N

$T > R > N > P > S$

tates cooperation with cooperators but at the same time includes protection against exploiters. The willingness to cooperate can be specified as a disposition never to be the first to defect, while the ability to protect against exploiters can be seen to rest in a disposition not to continue to cooperate when an opponent defects. There are three varieties of conditional cooperative programs: Tit for Tat, *prudent morality, retributive morality*. The latter two differ from Tit for Tat in that they exclude the defection option. This feature, we suppose, is an essential ingredient of our common notions of morality.

Retributive morality is a program that excludes the defect option from its behavioral repertoire and, rather than responding in kind (like Tit for Tat), responds to an opponent's defection by some explicit punishment that typically involves some costs to the actor. The cost feature tends to eliminate the ambiguity attached to Tit for Tat's punishment by defection. Because of its less ambiguous nature, retributive morality is potentially more effective than Tit for Tat in supporting "general" morality in a community. It shares, however, to some extent, the problematic feature of Tit for Tat that it can get caught in a vicious cycle of mutual retaliation once a feud gets started (Axelrod 1984, 138).

We shall concentrate attention on prudent morality. Under a program of prudent morality, an individual cooperates whenever he chooses to play the game but refuses to play with individuals who have defected in the past. Prudent morality is similar to retributive morality in that it excludes the defect option from its behavioral repertoire. Its difference from the latter lies in its more defensive and less corrective nature. Retributive morality requires the actor to take action explicitly to punish a defector, whereas prudent morality simply dictates refusal to continue to deal with the opponent.

Prudent morality contributes principally to one of the two practices that support the moral order of a social community. It dictates strict compliance with moral rules; but because it fails to impose explicit punishments on defectors, it only indirectly deters others from defecting. The refusal to continue to interact with a defector is, of course, a punishment of sorts; but it tends to be less deterring than the retaliatory responses of Tit for Tat or retributive morality.[10] Yet even if its laxity in punishing defectors may be seen as a flaw, prudent morality seems to us to be, among the strategies of conditional cooperation considered here, overall, the least controversial candidate for a program representation of what is commonly understood as morality.[11] The fact that it is defined by what it *does not do* (i.e., defect) also seems in line with the *thou shalt not* terms in which moral principles are often stated. We, therefore, include prudent morality as the *moral program* in the simulation experiment we shall discuss.

Our discussion now allows us to restate the issue of the relation between rationality and morality in a fashion to satisfy David Gauthier's demands. If we specify the concept of morality suggested here and we define rationality as choosing from among a set of alternatives the one that generates the highest pay-

offs (or, at least, payoffs not systematically below those of any feasible alternative), we not only use these concepts compatibly with their common understanding, but we also use them in a sense that makes them definitionally independent. Whether rationality and morality, so defined, are compatible is a *factual*, not a definitional, matter. Whether morality is compatible with rationality has to be answered in the light of a comparison between the overall payoffs that actors can expect from potential alternative dispositions or behavioral programs. To show the rationality of morality would mean to demonstrate that in competition with alternative behavioral programs, the moral program is more (or, at least, no less) successful than any other and, consequently, a program that can be rationally chosen.[12]

THE VIABILITY OF ALTERNATIVE DECISION RULES

The rationality to an individual player of alternative dispositions is essentially a matter of the cumulative payoffs generated by such programs in the community where an individual is located. Since, as Axelrod (1984) has shown, cumulative payoffs vary with the community of players with whom an individual player (or program) interacts, individuals are likely to find different strategic programs rational in different settings. To demonstrate that a particular program has general appeal, it is necessary to show that it performs, on average, no worse than any other. We consider two programmatic implementions of prudent morality—one intolerant, one more tolerant. The difference between the intolerant and the tolerant version is that the former terminates all interaction after a fellow player's first defection while the latter gives a defecting opponent a second chance after withdrawing for one round. The intolerant program is examined first in as much as it is the simpler program and has properties identical to the tolerant program in the first several tournaments analyzed.

All of the tournaments use the same payoff matrix for each prisoner's-dilemma-with-exit encounter between individuals in the communities of interest. The payoffs are (1,1) for the *cooperate, cooperate* cell and (−1,−1) for the *defect, defect* cell. If either player chooses not to play a particular game, the payoffs are (0,0). In the exploitative off-diagonal case, the player who cooperates receives −2 while the player who defects receives 2. The same payoff structure has been used for illustrative purposes by Tullock (1985).[13]

Elementary Strategies with Exogenous Community Populations

We shall report simulation results from two series of tournaments involving four decision rules in an environment where players confront prisoner's dilemma games with an exit option (PDE), then analyze settings where the population of players is endogenously determined by payoffs in previous rounds of the game. In the first series of tournaments examined, the number of PDE pairings among individuals is varied within an exogenously determined population of four elementary strategies. We find that the prudent moral strategy does best in an environment where many pairings occur. In the second series of tournaments, both the number of PDE pairings and the composition of the relevant group is varied. This series of tournaments demonstrates that advantage of prudent morality is greatest in relatively harsh settings. The prudent strategy is more robust than the other four programs in the sense that its performance is relatively more independent of the strategic programs used by other players in the community.

In each pairing with another player, individuals are assumed to use a particular strategic program that determines whether or not they participate in the prisoner's dilemma game before them and, if so, whether they cooperate with the other player or not. Individuals are characterized by their strategic programs. In the first round of play, individuals know nothing about their counterparts. In subsequent pairings, they remember whether particular players defected in previous rounds of play or not. Of course, only the adaptive strategies make use of this information. The payoffs for each encounter are those we have specified.

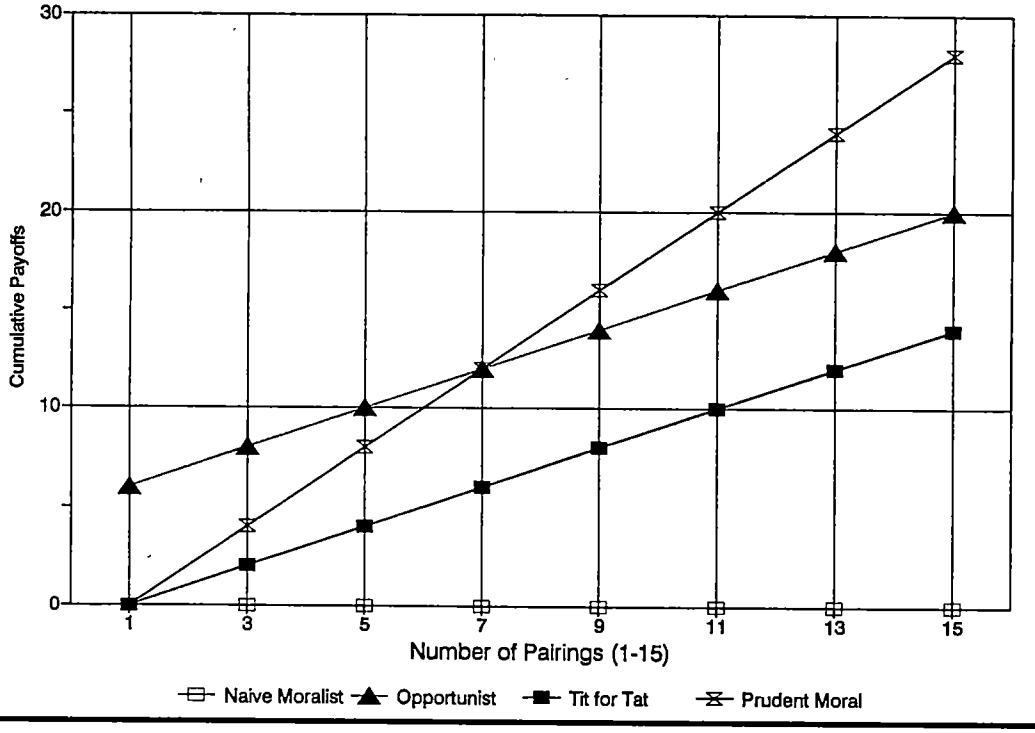
To make the results comparable to Axelrod's tournaments, we assume that several of the players have adopted dispositions that always require playing the game at hand. This allows the advantage of strategies that make use of an exit option to be readily demonstrated. Initially, our attention is focused on four strategic programs: (1) always play and always defect, the *Opportunistic* strategy; (2) always play and always cooperate, the *Naive Moralist's* strategy; (3) always play and use the strategy that the other player used the last time they met, *Tit for Tat*; and (4) play only with those who have never previously defected but always cooperate if one chooses to play, the *Intolerant Prudent Moral* strategy. All but the opportunistic strategies are "nice" strategies, in that they play and cooperate in the first round of play with a stranger. The first two programs are implemented as if they are dominant pure strategies, inasmuch as the course of play is not affected by past rounds of the game. The last three programs are adaptive strategies in that previous rounds of play affect their current behavior vis-à-vis their counterparts.

As in Axelrod's work, our simulations clearly reveal that the viability of a particular strategy varies with the strategies used by other players and with the number of times players confront each other in PDE games. However, in contrast to Axelrod's work, we find the Tit for Tat strategy to be less robust than the selective strategy of the prudent moralist.

The first series of tournaments shows that the viability of alternative dispositions varies with the

FIGURE 2.

1–15 Pairings, Cumulative Payoffs



number of times that individuals confront each other in PDE settings. Figure 2 summarizes the cumulative scores realized in several series of pairings between four individuals, each playing one of the four strategies described. They do not meet themselves. At the left-hand side of the chart, players meet each other a single time in the PDE setting. In this case, the opportunistic strategy is substantially more viable than any of the others. It yields a payoff of 2 in each of the pairings with the other players and thus a total score of 6. The other strategies generate a score of 0—a payoff of −2 in their pairing with the opportunist and payoffs of 1 with each of the other two players. Note that as the number of pairings increases, the viability of both Tit for Tat and the prudent moral strategy increases. Both adaptive strategies reduce losses from subsequent encounters with opportunistic players. Tit for Tat reduces them from −2 to −1. The prudent moral strategy reduces them from −2 to 0. Because of this greater reduction in losses from opportunistic players, prudent morality dominates Tit for Tat in two or more encounter games. Moreover, it outscores the opportunistic strategy in settings where players meet each other in PDE settings seven or more times. Tit for Tat never outscores the opportunistic strategy in this population of players.

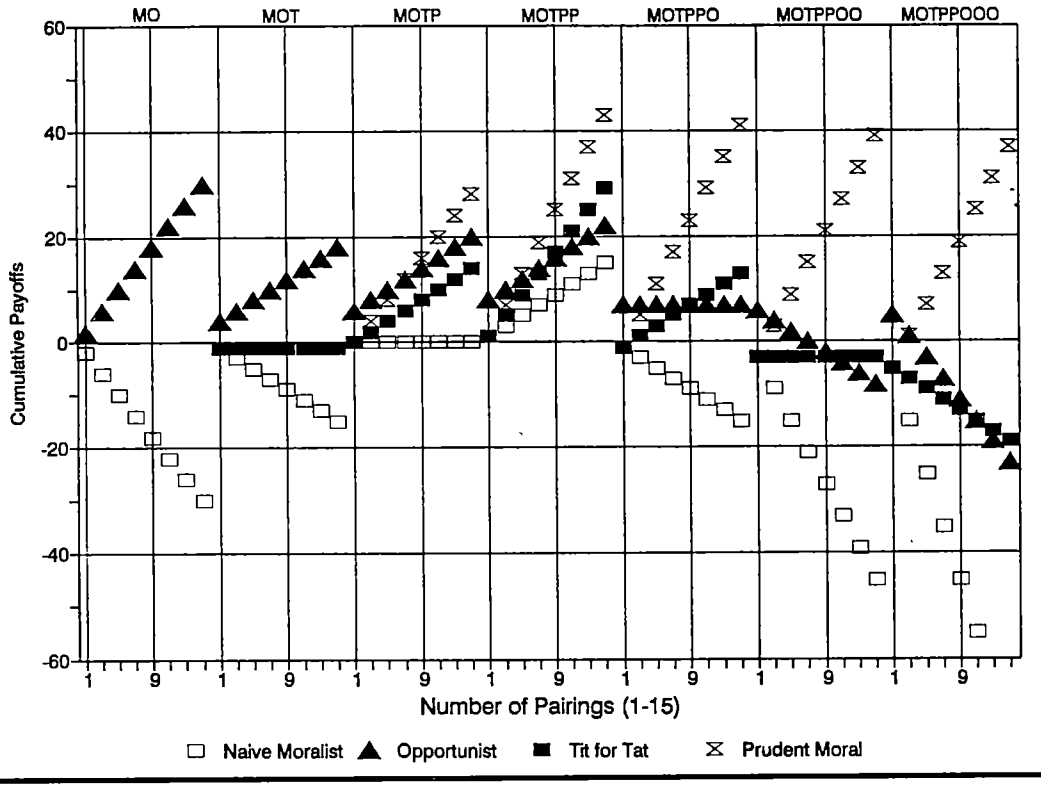
The second series of tournaments analyzes the extent to which the four strategic programs are affected by changes in the population of players confronted. The results clearly demonstrate that the attractiveness of a particular strategic program varies with the population of players faced. However, the results indicate that prudent morality is the least sensitive to community composition and scores uniformly better than the other programs if there are fairly numerous pairings with other players.

Figure 3 summarizes the cumulative scores realized by individuals playing the four strategies in seven different communities composed of different numbers of players. In each community, a series of tournaments similar to those reported in Figure 2 is conducted, in which pairings between players vary from 1 to 15. Figure 3 thus summarizes the results of 105 different simulations. Community composition is identified by a series of letters along the top of the figure. The various numbers of pairings between players within each community are written along the bottom of the table. The vertical dimension represents cumulative payoffs. Each symbol (triangle, rectangle, etc.) characterizes the payoff of a particular strategy. The location of each symbol represents the cumulative score that an individual playing that strategy has realized within the relevant community after a particular number of pairings with each of the other players.

The first community, at the far left of the diagram, consists of a single naive moralist (M) and an opportunist (O) and is labelled MO. In this setting the opportunistic strategy easily dominates the moralist strategy for all ranges of encounters. The second community adds a Tit for Tat player (T) to the previous community and is characterized by the subinterval labeled MOT. Note that the addition of

FIGURE 3.

Payoffs in Alternative Populations



the Tit for Tat player enhances the welfare of the naive moralist, while it reduces the score of the opportunist at all levels of encounters. However, the opportunistic strategy continues to be the most viable. The third community is identical to the one analyzed in Figure 2; it adds a prudent moralist (P) to the previous group and is labeled MOTP. Note that inclusion of a prudent moralist improves the lot of all the other players at all encounter levels.

The fourth community adds another prudent moralist to the community (MOTPP). Note that the addition of a second prudent moralist again increases the welfare of all other community members and makes the prudent moral strategy even more viable over a wider range of encounters. It now dominates the opportunistic strategy in settings where at least five PDE games are played between each pairing of the five community residents. In this environment, Tit for Tat emerges as the second most viable strategy in settings where PDE games are relatively numerous. Even the naive moral strategy is viable in this setting in the sense that it yields, for the first time, a positive net score over the entire range of possible encounters. The viability of the naive moral strategy requires a relatively large number of other benign players.

Communities 5, 6, and 7 show the effects of adding one, two, and three additional opportunistic players to the fourth community (yielding MOTPPO, MOTPPOO, and MOTPPOOO). Note that the addition of other opportunistic players worsens the lot of all other players, including other opportunists. The scores of the prudent moral strategy are the least affected and now exceed the opportunistic strategy over a wider range than in the earlier communities. In the rightmost community (MOTPPOOO), the prudent moral strategy scores better than any other if there are two or more PDE pairings with other community residents. It is the only viable strategy if there are four or more encounters in the sense that only the prudent moral strategies yield positive scores in this range. The naive moralist's strategy is by far the worst strategy in this relatively nasty community. Although gains are made from dealings with the two prudent moralists and the Tit for Tat player, losses generated by interactions with opportunistic players far outweigh those gains.

The second tournament demonstrates that the prudent moral strategy is highly viable in many settings and that communities with a relatively large number of such players tend to be relatively more successful than communities composed of a relatively large number of opportunistic players. Indeed in relatively inhospitable communities like community 7, the prudent moral strategy was the *only* viable strategy if PDE pairings are frequent.

All this suggests that individuals who adopt a prudent moral strategy will be relatively more successful than those who adopt other programs of similar complexity, within this range of environ-

ments. On the other hand, each of the other programs is viable in the proper context. To explore whether the prudent moralist's strategy has an evolutionary advantage, another series of tournaments is analyzed, in which community composition is determined endogenously.

Tournaments with Endogenous Population Mixes

In the third series of tournaments, community composition is endogenously determined by past cumulative payoffs. Here we find that prudent morality is generally the most successful of the strategies in the tournament, insofar as it scores well and therefore tends to be more than proportionately represented in viable communities. We examine two types of evolutionary simulations but report in detail only the more interesting one. Simple population growth models yield relatively unsurprising results. In the case where community size is allowed to grow and the number of PDE encounters is greater than five, individuals who adopt the prudent moral strategy rapidly displace all other player types. In settings with a large number of pairings, the prudent moral strategy yields the largest cumulative payoffs and grows most rapidly. This is evident from the results reported in Figure 3. In cases where the number of encounters is small, opportunistic players tend to displace other types of players. However, the resulting community disappears because a community of opportunists is not viable. In this sense, natural selection might be said to favor players who adopt the prudent moral strategy.

A somewhat more rigorous test of viability of alternative dispositions involves competition within a population of fixed size. These simulations reveal that in cases where the community size is held constant but its composition is allowed to vary according to past cumulative scores, community composition fluctuates in an irregular cyclic manner. This fluctuation occurs in part because we allow strategic programs to regain popularity if the social environment changes so as to increase the viability of those programs. Insofar as moral decision rules reflect social, rather than genetic, conditions, total elimination of a particular rule is a less plausible evolutionary result than selective transmission and rebirth. Rather than disappear forever, unsuccessful decision rules may be expected to reappear if social circumstances change in a way that increases their viability.

The tournaments involve communities in which 25 PDE games are played with players other than themselves. Initially, the dispositions of players are equally distributed throughout the community; but in succeeding periods, they are proportional to positive scores generated in the previous rounds. Programs that generate negative or zero scores in a particular generation of the community disappear for at least one generation. Programs *reappear* if the community's composition becomes more favorable at some point in the future. To allow this possibility, the score that missing strategies would have generated had they been present is used to determine its relative size or population share in the next generation. In the first evolutionary tournament, the population shares sum to four in each period, reflecting the constant population assumed for this series of simulations.[14]

Figure 4 reports the results of such an evolutionary tournament between the four dispositions used in the other series of tournaments. Population shares are plotted in a stacked diagram. Vertical distances between lines represent the relative size of the various decision rules. The shaded areas highlight the time path of population shares of specific strategies. The average width gives an indication of the relative success of the various strategies over the time period of interest. Note that the time paths of population shares for both nonadaptive strategies are highly cyclic in nature. Both the naive moralist and opportunistic strategies occasionally disappear entirely. For example, the naive moralist strategy disappears in the second generation. This, in turn, makes the opportunistic strategy less viable, since it does poorly in a community composed of other opportunists, Tit for Tat players, and prudent moralists. With the disappearance of the opportunists, the naive moralist strategy returns to viability and reappears in the third generation. This, of course, makes opportunism a more viable strategy—and so on. The population share of Tit for Tat is relatively stable, ranging between .86 and 1.33. The population share of players using the prudent moralist strategy is also cyclic but never falls below 1 and exceeds 2 at some point during each cycle. If population share is used as an index of viability, the prudent moral strategy is the most viable of the four programs in this tournament. As indicated by the previous series of tournaments, the prudent moral strategy does well in most communities.

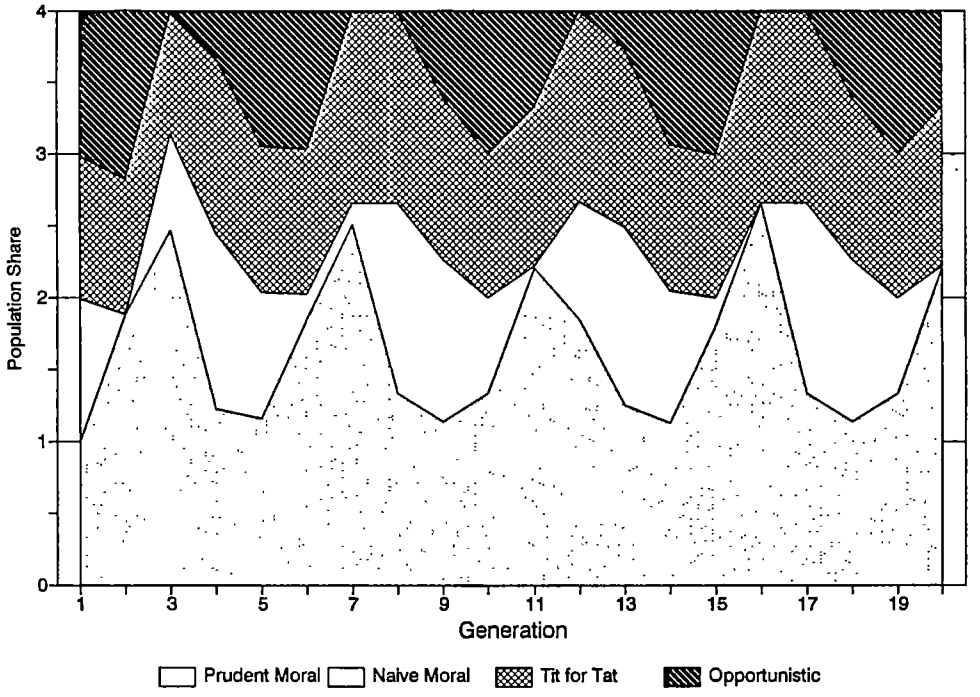
Evolutionary Tournaments with More Sophisticated Strategies

In the last series of tournaments, several more sophisticated strategies are added to the repertoire of possible programs. We first consider a fifth strategy, *Hit-and-Run*, a prudent form of opportunism. Hit-and-run is added to determine whether the exit option itself, rather than the moral program, was the source of prudent morality's superior performance. The Hit-and-Run strategy is the mirror image of the Prudent moral strategy in that it calls for playing only with individuals who did not defect in the previous period. However, whenever the game is played, the Hit-and-Run player defects.

We follow the analysis of Hit-and-Run with consideration of sixth and seventh strategies, *Tester* and *Tolerant Morality*. The Tester strategy tests to see whether or not other groups of players are exploitable naive moral types who always cooperate. It defects in the first meeting with each other player and

FIGURE 4.

Evolutionary Success (25 Pairings)



continues to defect as long as the other player cooperates. If the other player does not cooperate, Tester cooperates twice and thereafter plays tit for tat. Tester is designed to make Tit for Tat score relatively better by responding to nonexploitability with two cooperative rounds (rather than one).[15] Tester may be considered a trainable program insofar as retribution via defection or exit transforms tester from an exploitative strategy into a well-behaved Tit for Tat player.

The Tolerant Moral strategy shares with the intolerant Prudent Moral program the property that it never defects. It is more tolerant in that it does not give up on an opponent after a single defection. Instead, it responds to defection with a one-period exit followed by cooperation. If a second defection is encountered, it withdraws from all future PDE games with such players. More sophisticated versions of Tester and Tolerant Morality are possible, but these programs capture essential elements of a wide array of reactant and tolerant strategies.

In the first of the tournaments including more sophisticated strategies, we explore whether or not the advantage of the prudent moral strategy is simply a result of the fact that it is the *only* program using the exit option. To shed light on this question, the Hit-and-Run program is added to the previous set. Its advantage over the nonadaptive opportunistic strategy is that it avoids long-term unproductive engagements with Tit for Tat players and other opportunistic programs.

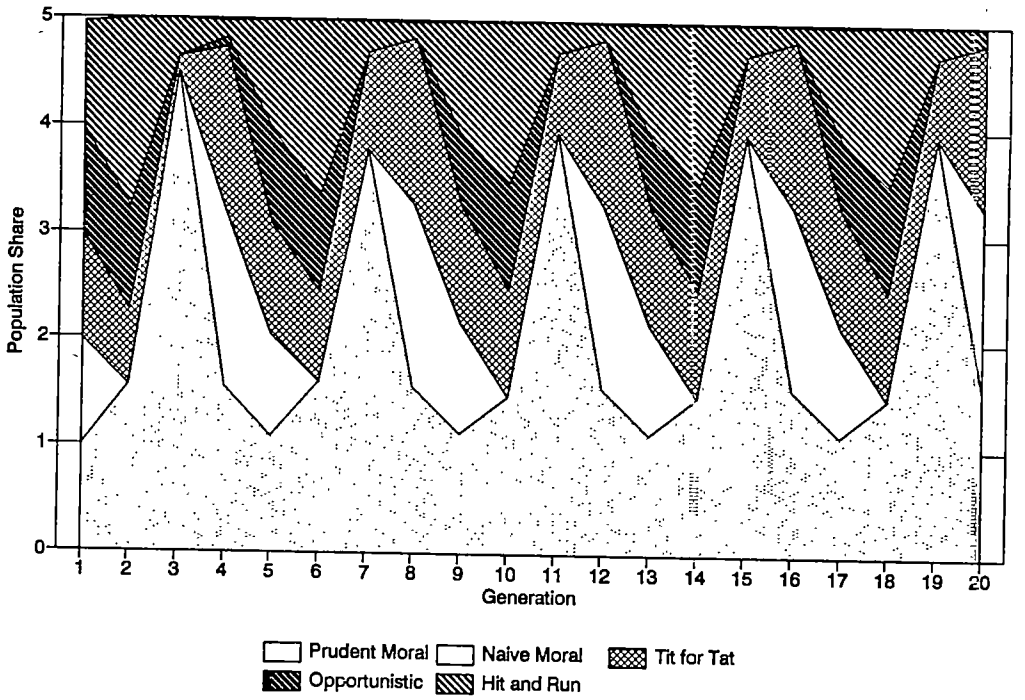
Figure 5 reports the results of an evolutionary tournament for the resulting population of five strategic programs. The results are similar to those of the previous tournament between four programs. Again, the cyclic pattern occurs; and again, the Prudent Moral program proves to be on average the most successful. The Hit-and-Run strategy is more viable than the two naive strategies in that it never completely disappears. However, its population share is never larger than the Prudent Moral strategy and is often smaller than that associated with Tit for Tat.

In the last series of tournaments reported here, the Tester and Tolerant Morality programs are added to the previous population of five strategies. Tester is an opportunistic, but trainable, version of Tit for Tat. The Tolerant Moral program is similar to the Prudent Moral program except that it checks to see whether the behavior of players who have transgressed (defected) in a previous round of the game might be altered by exit. If a second defection occurs, it ceases to interact with those using the offending strategy.

The results of this tournament are reported numerically in Table 1. The numbers represent population shares in a community of seven strategies and are similar to those plotted for the previous evolutionary tournaments. The results are broadly similar to those of the previous tournaments in that there are cycles and the moral strategies do well. However, there are several new results of interest, the first of which is that with the addition of two more retributive strategies, the nasty strategies do significantly worse in this tournament than in the previous ones. The simple opportunistic strategy disappears in round 2

FIGURE 5.

Evolution (5 Strategies, 25 Pairings)



and never returns, while the Hit-and-Run strategy shrinks to a range between .1 and .3 reflecting variation in the small pool of naive moralists available to exploit. Interactions with Tit for Tat and Tester, together with shunning by the two moral programs, combine to make the simple opportunistic strategy inviable in this setting. Hit-and-Run's participation in games with programs other than the naive moralist cease after the second meeting with other programs.

The four most viable strategies in this setting are Tit for Tat, the original (intolerant) Prudent Moral program, Tester, and the Tolerant Moral program. Tester and the original Prudent Morality program do significantly less well than Tit for Tat and the Tolerant Moral programs. After the first round of the game, Prudent Morality ceases to interact with Tester, which reduces the cumulative scores of each. Note that in the first round of the game, where there are still relatively large numbers of individuals using the Opportunistic and Hit-and-Run strategies, the two moral strategies are more successful than either Tester or Tit for Tat. In relatively nasty environments, the opportunity cost of exit is low—in fact, negative.[16] With the disappearance of the simple Opportunistic program in the second round, the relative performance of the original intolerant program suffers, and Tit for Tat gradually converges to about the same population share as the Tolerant Moral program. In this part of the game, interactions with Tester (or the lack thereof) are crucial. Both the Tit for Tat player and the Tolerant Moralist are able to "persuade" Tester to give up his exploitative behavior with respect to themselves, although not for others. This provides them with another useful partner in PDE settings. Here, the opportunity costs of exit are significant and reduce the long-term performance of the intolerant form of Prudent Morality. The original intolerant Prudent Moralist does not check to see whether defectors are like Tester, who might be "taught a lesson," and thereby misses profitable interactions with Tester. On the other hand, in more dangerous settings like those of the first round, where a large number of unteachable defectors exist, not giving a defector a second chance is the second most viable strategy.

All in all, the simulation results suggest that a strategy of cooperating with those who have cooperated in the past and avoiding those who have defected in the past is eminently viable and is robust over a wide range of community compositions. The advantage of tolerant versions of this strategy depends on the population mix. Tolerance has advantages if there are other programs in the community whose behavior can be changed by imposing modest sanctions.[17] Overall, the prudent moral strategies are viable moralities. Moreover, the results are only somewhat sensitive to the magnitude of the payoffs. For example, a doubling of the off-diagonal payoffs has little effect on the evolutionary simulations, although it does increase the scores of the opportunistic strategy in settings with relatively few encounters.[18]

Table 1.

Evolution within a Fixed Population (7 Initial Programs, 20 Generations)

GENERATION	NAIVE MORAL	OPPORTUNIST	TIT FOR TAT	INTOL. MORAL	HIT-AND-RUN	TESTER	TOL. MORAL
1	1	1	1	1	1	1	1
2	0	.27	1.292	1.331	1.022	1.35	1.736
3	0	0	1.94	1.493	.11	1.384	2.072
4	.775	0	1.741	1.351	.106	1.284	1.743
5	.52	0	1.611	1.277	.526	1.454	1.613
6	.021	0	1.803	1.345	.451	1.568	1.811
7	0	0	1.97	1.435	.123	1.496	1.977
8	.677	0	1.775	1.342	.103	1.325	1.777
9	.496	0	1.641	1.289	.48	1.453	1.642
10	.069	0	1.798	1.345	.431	1.551	1.805
11	.019	0	1.947	1.426	.157	1.497	1.954
12	.346	0	1.863	1.406	.12	1.399	1.865
13	.445	0	1.74	1.345	.311	1.418	1.742
14	.264	0	1.762	1.342	.382	1.485	1.766
15	.138	0	1.85	1.381	.282	1.495	1.855
16	.229	0	1.862	1.396	.198	1.45	1.866
17	.344	0	1.801	1.371	.25	1.431	1.804
18	.31	0	1.778	1.355	.319	1.457	1.781
19	.225	0	1.811	1.367	.305	1.477	1.815
20	.222	0	1.838	1.381	.253	1.465	1.842

Note. We report population shares of alternative behavioral programs in successive generations. Scores are calculated for 25 rounds of play between each pairing of rules. These pair scores are then weighted by population shares and summed to give generational payoffs for each program. Population weights are determined by the relative payoffs of program interactions in the previous generation.

SCOPE AND LIMITS OF EXIT

A venerated tradition in philosophy centers around the axiomatic assumption that morality, by necessity, means self-sacrifice and that consequently, the teaching of morality is essentially the teaching of self-denial.[19] For this tradition, self-sacrifice is by definition an attribute of morality; and it follows as an implication from such a definition that morality cannot be a rational disposition in the sense of furthering the well-being of the person adopting it. The results of our discussion support the equally long-standing tradition of critical arguments against such understanding of morality. It shows that the basic thrust of our common understanding of morality and rationality can be specified in a fashion that makes the two definitionally independent; and it also shows that the two independently defined notions are, in fact, compatible. In other words, it shows that there is no reason to assume that morality is necessarily self-sacrificial unless we insist on defining morality in a way that rules out the possibility of morality's ever being rational. Those who insist on the self-sacrifice notion of morality may rightfully have in mind that a moral disposition implies the adoption of constraints on one's own choice of behavioral strategies. What they fail to recognize is that such constraints need not be in conflict with self-interest. As David Gauthier puts it, constraints on the pursuit of one's interest can well "have themselves a foundation in the interest they constrain" and can "make possible the more effective realization of one's interests" (1986, 2, 103).

To be sure, the assumption that an exit option is readily available (i.e., that the costs of exit are relatively low) plays a critical role in our argument. In cases where exit costs are low, the prisoner's-dilemma nature of the "iterated prisoner's dilemma" essentially disappears. Where player A has access to alternatives that promise payoffs close to what he could expect from mutual cooperation in an ongoing game with player B (as in a competitive market), exit is essentially costless. Interaction with defectors can be easily avoided, and defectors will find that they are avoided.[20]

With a low-cost exit, since a defecting counterpart can be easily avoided, there is little reason for a player to make efforts to change the opponent's behavior. To the extent, however, that attractive alternatives are not easily available, efforts to "correct" a defecting opponent's behavior may become preferable to leaving the interaction. Where the payoffs that player A can expect from mutual cooperation with player B are significantly higher than what A expects from the next best alternative, A will have an interest in undertaking efforts to "correct" a defecting B's behavior. One can apply A. O. Hirschman's (1970) well-known distinction between *exit* and *voice*. The higher the costs of exit from a particular counterpart, the more important is the use of voice, in the sense of efforts to improve the relationship.

It can be concluded from this argument that prudent morality (the disposition never to defect and to restrict one's choice options to the alternatives *cooperate* or *do not play*) becomes an increasingly more viable program as the costs of exit from a given setting decrease. With decreasing exit costs, the pros-

pects for future gains become less and less reliant on efforts to change a defecting opponent's behavior. It is under such conditions that the two other strategies of conditional cooperation, Tit for Tat and retributive morality, become less attractive candidates for a viable morality. On the other hand, as the costs of exit increase, the role of voice, (i.e., efforts to influence a particular counterpart's behavior) gains in importance, compared to exit. We cannot easily exit from our neighborhood; the investments that we have made in friendship relations rule out easy substitutability. In relations like marriage we deliberately raise exit costs in order to make commitments credible so as to generate rewards that otherwise would be unattainable. This suggests that in such relations, Tit for Tat and retribution may play a much more significant role than in market relations.[21]

If this argument on the relation between exit costs and the adequacy of the different versions of a conditionally cooperative strategy is correct, one should expect this to be reflected in our common notions of morality. And there seems to be, indeed, some evidence suggesting that our intuitions of what is moral vary with relevant differences in exit costs. At the high-cost end of the spectrum, moral justification for Tit for Tat and retributive behavior seem to be fairly common, whereas Prudent Morality gains in importance as we move to the low-cost end. Another implication is that contrary to a common perception, markets are not parasitic on moral attitudes that have to rely, for their nourishment, on other social settings. Instead, the more the essential feature of markets (coordination by voluntary contract) is applicable and secured, the more markets reinforce the virtues of honest behavior that in turn, make them work effectively. In fact, one of the major defects of socialism may be seen in the fact that it is unable to create and nourish the moral attitudes that it would need to be workable.

Notes

We are indebted to Geoffrey Brennan, James M. Buchanan, Mark Crain, and Hartmut Kliemt for helpful comments and suggestions on earlier versions. A previous draft of the paper was presented at the Public Choice Society meetings, New Orleans, 1991.

1. To be sure, one could also argue that moral problems as such are defined relative to some community and that, in this sense, for the community of cartel members, the problem is a moral one. Such a broader interpretation would seem to be implied, for instance, in D. T. Campbell's interpretation of moral norms as "our individually rational preferences as to how others behave" (1986, 171).

2. It should be noted that our discussion is also limited in the sense that it deals only with moral problems that are (or can be disaggregated into) two-person prisoner's dilemmas. The relevance of the distinction between two-person prisoner's dilemmas and N-person prisoner's dilemmas for the study of moral rules is discussed in Vanberg and Buchanan 1990.

3. In his 1965 essay, "Ethical Rules, Expected Values, and Large Numbers," J. M. Buchanan has argued along similar lines: "Note that the choice to be analyzed is that between separate *rules* for behavior, not that between separate *acts* in particular circumstances. . . . The individual is presumed to be facing the following question: What ethical rule shall I adopt as a guide to my behavior in subsequent actions? There are two alternatives before him. He can adopt a rule which we shall call the moral law, or he can adopt a rule which, loosely, we shall call the private maxim. By selecting the first, the individual commits himself to act in subsequent situations on the basis of something like the generalization principle. . . . By selecting the second instead, he commits himself in advance to no particular principle of behavior. He retains full freedom to act on the basis of expedient consideration in each particular situation that arises" (1977).

4. David Gauthier writes, "A disposition is rational if and only if an actor holding it can expect his choices to yield no less utility than the choices he would make were he to hold any alternative disposition" (1986, 182–83).

5. This is not to argue that moral rules necessarily replicate the series of actions that would have occurred in a perfectly informed maximizing strategy. If the disposition always—in each and every instance—to maximize perfectly were a feasible program, then the person, for obvious reasons, could not do better than adopt this program. However, for imperfect agents (i.e., agents who make mistakes), reliable long-term case-by-case maximization is simply not a feasible program (Heiner 1990). Our analysis shows that relatively simple moral rules perform at least as well as comparable pragmatic rules.

6. It has been occasionally observed in discussions on the iterated prisoner's dilemma that the presence of an exit option makes a critical difference (see, e.g., McLean, Orbell, and Daws n.d., Orbell, Schwartz-Shea, and Simmons 1984; Schuessler 1989; Witt 1986). Indeed, the inclusion of an exit option may change the nature of the game such that we are, in effect, no longer dealing with a genuine iterated prisoner's dilemma. To be sure, one could argue that the iterated prisoner's dilemma itself is also no longer a true prisoner's dilemma. However, while in an ordinary iterated prisoner's dilemma, the single constituent games are prisoner's dilemmas if considered separately, this is no longer true for the prisoner's dilemma with exit.

7. In his article "Adam Smith and the Prisoner's Dilemma," Gordon Tullock (1985) noted that interaction between firms and consumers in ordinary competitive markets is typically not analogous to an iterated prisoner's dilemma because actors are free to "select their opposite numbers." He suggests that such market interaction can be more appropriately illustrated in a setting where *self-selected* pairs of players can play an iterated prisoner's dilemma (on computer terminals), with each player retaining the option at any time to terminate an ongoing game (or exit the booth) and seek out some other willing partner, so that, in effect, "the people who play the games do so in voluntarily chosen pairs" (p. 1074). In such a setting, Tullock argues, "the prisoners' dilemma vanishes because the individual players have a strong desire to establish credibility so that they can play in future games. The basic reason is simply that people voluntarily choose their own partners" (ibid.).

8. We shall specify further on what, exactly, is meant by *exit*—in particular, whether it means a *definite* or a *temporary* refusal to interact with a defector.

9. When interacting with its own kind or with unconditional cooperation, Tit for Tat is, in fact, indistinguishable from unconditional cooperation: it always cooperates and never defects. In this sense, the success of Tit for Tat in Axelrod's simulation experiments shows that a rational egoist, under appropriate conditions (i.e., when interacting with suitable opponents), will behave morally. It does not show that morality is rational.

10. To be sure, a total termination of interaction may be more damaging than a single retaliatory defection.

11. It is useful to take a brief look at the relation between our concept of prudent morality and David Gauthier's notion of *constrained maximization*. To our knowledge, Gauthier nowhere really specifies constrained maximization in terms of

an explicit behavioral program. It is obvious that he wants it to be different from unconditional cooperation. "A constrained maximizer," he argues, "makes reasonably certain that she is among like-disposed persons before she actually constrains her direct pursuit of maximum utility" (1986, 169). And he emphasizes that "persons disposed to co-operation only act co-operatively with those whom they suppose to be similarly disposed" (p. 172). But it is less clear whether he wants generally to exclude the defect option from a constrained maximizer's behavioral repertoire or not; nor is it quite clear which role, if any, he sees for retribution (see Buchanan 1988, 81–82). We think, however, that an attempt to translate the thrust of Gauthier's argument into a behavioral program should generate something similar to our notion of prudent morality. If, indeed, there is a systematic difference between his notion and ours, it probably lies in the fact that he seeks to specify morality in terms of a *disposition to cooperate*, while our emphasis is on morality as a *disposition not to defect*. We suppose that the latter notion lends itself to a more straightforward and unambiguous specification as a behavioral program.

12. It should be noted, however, that such rationality of morality does not eliminate the problem of *temptation* at the moment of choice. The issue (briefly mentioned earlier), of how behavioral dispositions or programs are formed or adopted, is of particular relevance in this context. Situational temptation is apparently less of a problem for tacitly conditioned dispositions than for deliberately and rationally adopted programs. In fact, whether (from the perspective of a strict rational choice model) an actor can be assumed able effectively to commit to a rule is an interesting issue.

13. In terms of the letters used in Figure 1, the payoffs are: $T = 2$, $R = 1$, $N = 0$, $P = -1$, $S = -2$.

14. The share of the ith strategy going to population W_i is determined by the formula

$$W_{it} = 4S_{it-1} / \sum_{j=1}^{N} S_{jt-1},$$

where S_{it-1} is the cumulative score realized in the previous round of the game by strategy i. The population shares sum to N, the number of programs considered. The use of fractional population shares allow the results to extend to arbitrarily large, stable populations. Since a program cannot have a negative population share, programs that generated a negative score in the previous round are given a population share of zero and excluded from the denominator of the weighting formula. If a strategy was dropped from the previous round, next period's population share is determined by the cumulative payoff that the strategy program would have realized in the previous round of play had it been included. The lack of an inertia factor makes the time path more jagged than it would have been and is best interpreted as movement from one long-run equilibrium to another. This is a fairly severe test for a strategy, since its gains can be lost overnight, as the population of strategy programs changes. The cumulative score of strategy i in generation t is calculated by determining the score that such a strategy realizes with play against other strategies, S_{ij}, weighted by the other strategy's population weight, W_{jt}. These weighted scores are added up across the strategies in the game:

$$S_{it} = \sum_{j=1}^{N} W_{jt} S_{ij},$$

In effect, this gives the long-run average payoff that would have occurred had the generation of pairings been generated randomly with probabilities equal to the population shares divided by number of strategies. Players are never paired with themselves unless population shares are greater than one. The weight used for meetings between players using the same strategy is $W_{it} - 1$ for $W_{it} > 1$ and 0 otherwise. This sampling-without-replacement weighting scheme somewhat reduces the scores of nice strategies and improves the scores of nasty strategies; but it is the most reasonable method of representing interaction in small stable communities.

15. Had Tester followed an opponent's defection with only a single cooperative move, the relative performance of the Tolerant Moral program would have been improved compared to both Tit for Tat and Tester. Tit for Tat would get into a sequence of mutual defection with this version of Tester. By contrast, the Tolerant Moral program would obtain cooperation from such a Tester. A more recalcitrant version of Tester would worsen the score of Tolerant Morality.

16. By *opportunity cost of exit* we mean the sequence of payoffs that a player gives up by ending interaction with another particular player.

17. More generally, tolerance is advantageous if either behavior of occasional defectors can be changed by exit or defections are sufficiently rare that overall cumulated payoffs are positive. With the game payoffs used here, gains from interacting with a random strategy with a probability of more than .66 cooperations in games played would be to the benefit of tolerant strategies. Random strategies generally do not do very well in games with Tit for Tat players, however.

18. A change in the rank order of the payoffs can affect the results. In some settings, the exit option can generate a payoff smaller than the *mutual defect* result. For example, individuals might provide suboptimal levels of a public good that yields benefits, rather than costs, for fellow players. In this case, the exit option generates a smaller level of the public good, hence smaller payoffs. However, it is not clear whether what we have called a prudent moral strategy requires withdrawal from all such games. Exercise of the exit option (the one used in the simulations) requires exit only in cases where the previous round of play made one worse off than one would have been by not playing the game. Moreover, it bears noting that solutions to public goods problems often require commitments to joint production that leave cooperating players worse off if others do not adhere to their commitments.

19. R. D. Alexander's critical assessment of this tradition is summarized in the statement, "Assuming that morality has to be self-sacrificing is what has led moral philosophers into their worst intellectual quagmires" (1957, 151).

20. Gorden Tullock writes: "Where there are a number of potential players available, the dilemma is proportionally weakened. Indeed, here we have what amounts to a mapping of the usual economic distinction between a monopoly on the one side and competition on the other. . . . Where the market is broad and there are many alternatives, you had better cooperate. If you choose the noncooperative solution, you may find you have no one to noncooperate with" (1985, 1081).

21. It also explains why the incidence of feuds (vicious circles of mutual retaliation) seems to be higher for relations where exit is essentially ruled out.

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ETHNIC DEMOCRACY AND THE LEGAL CONSTRUCTION OF CITIZENSHIP: ARAB CITIZENS OF THE JEWISH STATE

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The citizenship status of its Arab citizens is the key to Israel's ability to function as an ethnic democracy, *that is, a political system combining democratic institutions with the dominance of one ethnic group. The confluence of republicanism and ethnonationalism with liberalism, as principles of legitimation, has resulted in two types of citizenship: republican for Jews and liberal for Arabs. Thus, Arab citizens enjoy civil and political rights but are barred from attending to the common good.*

The Arab citizenship status, while much more restricted than the Jewish, has both induced and enabled Arabs to conduct their political struggles within the framework of the law, in sharp contrast to the noncitizen Arabs of the occupied territories. It may thus serve as a model for other dominant ethnic groups seeking to maintain both their dominance and a democratic system of government.

The problem of maintaining viable democratic regimes in ethnically divided societies promises to be one of the more salient political issues of the new world order. Contrary to the expectations of modernization theorists in the 1950s and 1960s, "nation building" has not only failed in the Third World but is crumbling in the Second and is showing growing signs of strain even in the First. As Western liberal democracies are increasingly confronted with the reality of ethnic diversity and as multiethnic societies in Eastern Europe and elsewhere are struggling with the process of democratization, the efforts to reconcile competing ethnic claims with democratic values and institutions are bound to be more and more difficult to sustain.

One successful example of a democratic yet deeply divided society is Israel. A Jewish state by self-definition, Israel has a Palestinian Arab minority of about 15%, and its Jewish population is divided almost equally between Jews of European and Asian–African origin. Still, since its establishment in 1948, the country has maintained a stable democratic regime that has withstood five major wars, 12 national elections, massive immigration waves, and at least one serious economic depression. Moreover, the political stability within Israel itself has been highlighted, since December 1987, by the national uprising of the Palestinian Arabs in the territories occupied by Israel since 1967 (Lustick 1980; Smooha 1978, 1990).

An important reason behind Israel's ability to manage ethnic conflicts within the framework of the law is the particular character of its political system. This has recently been described as an *ethnic democracy* as distinguished from a majoritarian, consociational, or *Herrenvolk* democracy. An ethnic democracy is a polity combining "the extension of political and civil rights to individuals and certain collective rights to minorities with institutionalized dominance over the state by one of the ethnic groups" (Smooha 1990, 391).

Israeli ethnic democracy has been constituted, according to Smooha, by the tension between two political commitments: one to the Jewish character of the state and the other to a democratic form of government (Smooha 1990, 391–95). Other social scientists have discussed this tension in terms of particularism versus universalism, collectivism versus liberalism, primordialism versus civic culture. Whatever the designation, the current scholarly consensus is that the ethnic Jewish element in Israel's political culture has been on the ascendance since 1967 and that the universal, democratic element has been on the retreat (E. Cohen 1983, 1989a; Kimmerling 1985; Medding 1990, 233; Weissbrod 1983).

I shall argue that the portrayal of Israel's political culture as constituted by these two conflicting principles cannot adequately explain one of the key features of Israeli democracy, namely, the citizenship status of its Arab citizens. In particular, I will argue, this dichotomous view would render a contradictory account of the way in which Arab citizenship has been codified and made explicit by a series of legislative and judicial acts during the 1980s.

To account for these developments, it would be more useful to understand Israeli political culture as comprised not of two constitutional principles—liberalism and ethno-nationalism—but of three—republicanism, liberalism and ethnicity (see Smith 1988). Of the three, the republican principle has been easily dominant; but it has increasingly been challenged in recent years in the name of the other two.

The confluence of these three principles of legitimation has resulted, I will show, in two types of citizenship: republican for Jews and liberal for Arabs. Thus, while Jews and Arabs formally enjoy equal citizenship rights, only Jews can exercise their citizenship as practice, by attending to the common good. Nevertheless, the citizenship rights enjoyed by Israeli Arabs are not inconsequential. Since they were first laid down in 1948, these rights have become the foundation of a civic status that both induces and

enables Israel's Arab citizens to confine their political struggles within the boundaries of the law.

My aim here, however, is not only to analyze a set of constitutional arrangements that have enabled a stable democratic regime to be sustained in a deeply divided society. I also hope to contribute to the extension of the current discussion of citizenship into the sphere of ethnic relations. The need to incorporate the problematic of ethnicity into the analysis of citizenship has frequently been pointed out. Thus, Turner has argued that "any further development of the theory of citizenship will have to deal more fundamentally with . . . the conditions under which citizenship can be formed in societies which are, as it were, constituted by the problems of ethnic complexity" (Turner 1990, 212–13). So far, however, with the exception of the issues of immigration and civil rights—which have mainly to do with admission to, rather than the definition of, citizenship (Brubaker 1989; Gibney 1988; Shklar 1991)—the field of ethnic relations has remained largely untouched by the citizenship debate. This in sharp contrast to the important ways in which the conventional views of citizenship, both liberal and republican, have been challenged by, for example, feminist writers (Jones 1990).

THE CONCEPTUAL BACKGROUND: LIBERAL VERSUS REPUBLICAN CITIZENSHIP

In the *liberal* tradition citizenship has been understood as "a set of normative expectations specifying the relationship between the nation-state and its *individual* members which procedurally establish the rights and obligations of members and a set of practices by which these expectations are realized" (Waters 1989, 160; emphasis mine).

The essence of the liberal conception of citizenship lies in its individual, universal, and equal character. As formulated by Roche, a community of citizens must possess at least three characteristics:

> Firstly, . . . all [members] have a presumptively equal status, equal rights and duties, etc. Principles of inequality deriving from gender, ethnic, class or other contexts are not supposed to be of relevance to the status of citizenship as such. Secondly, citizens accept that in principle and in fact they are and will remain strangers to each other. . . . They are not complete strangers, however. . . . They have impersonal but fairly complex and occasionally modifiable sets of assumptions and expectations about each other. . . . Thirdly, the presumptive equality between these fellow strangers is assumed to normatively govern, and to be enforceable in, their interactions with each other. (1987, 376–77)

The abstraction of the self from all particularity as a condition of citizenship in liberal democratic theory has been criticized by the proponents of republicanism. This "unencumbered self," whose sole characteristics are its subjectivity and its possession of natural rights, is denied, according to Sandel (1984), the possibility of membership in a moral community; for a moral community presupposes a notion of the common good antecedent to the individual citizen's choice. This contradicts the most basic liberal tenet of the priority of right over good (pp. 85–91).

To be able to participate in a moral community, Sandel has argued, one needs "character"—which the abstract, a-historical, unencumbered, liberal self does not possess. Having character means knowing "that I move in a history I neither summon nor command, which carries consequences nonetheless for my choices and conduct. It draws me closer to some and more distant from others; it makes some aims more appropriate, other less so" (Sandel 1984, 90–91). A shared moral purpose, from which originate both a notion of civic virtue and criteria for membership in the community, is thus at the heart of the republican ideal of citizenship (Geise 1984, 27).

Unlike the liberal citizen, who is the passive bearer of *status* (a mere bundle of rights), the republican citizen experiences citizenship as *practice* (active participation in the determination, protection, and promotion of the common good). The will and capacity to participate constitute one's civic virtue and are not only an expression, but also a condition, of citizenship (Oldfield 1990, 1–11). This raises the question how the republican community is constituted and what qualities are required for active participation in it.

Historically, the question of membership in the republican community was unproblematic, since membership was determined largely by birth. In the current revival of republicanism in the United States, however, two distinct notions of community may be discerned: a *weak* community, constituted by deliberation, membership in which is essentially voluntary and a *strong*, historical community that is discovered, not formed by its members (Friedman 1989, 283–84; Sandel 1982, 150; Sunstein 1988, 1541–42, 1571; Taylor 1989, 170).

The civic virtue appropriate to the weak community is no more than the willingness to be public-spirited—to place the general interest ahead of one's own in deliberating the common good (Geise 1984, 27). In the strong community, however, the possession of civic virtue requires much more than that. Since it is a historically constituted entity, members of this community must be committed to its ongoing existence as an important value in and of itself. As Sandel has put it, "[To] family or community or nation or people . . . I owe more than justice requires *or even permits*, not by reason of agreements I have made but instead in virtue of those more or less enduring attachments and commitments which taken together partly define the person I am" (1982, 179; emphasis mine; see also Taylor 1989, 182).

While the weak community is inclusive and open (at least in principle), people who are formally members can still exclude themselves from full citizenship practice by shying away from active participation in politics (Oldfield 1990, 160–61). In the strong community the problem is much more severe; for this type of

community is necessarily exclusive (as many critics have charged: I. Shapiro 1990, 198–99; Smith 1988; Sunstein 1988, 1539–40). Moreover, the basis for exclusion from the strong community is at least partly ascriptive, since membership in Sandel's "enduring attachments" cannot be entirely a matter of choice. Since republican communities could not exist as islands unto themselves even in antiquity—much less today—the question arises, What of members of society who cannot or would not become full members of the community?

While millions of people officially defined as guest workers, undocumented aliens, and so on, live as noncitizens on the margins of many democratic societies, a permanent status equivalent to the metics of the ancient polis is no longer acceptable to modern democratic theory (Dahl 1989, 119–31; Walzer 1983, 53–61). The only solution, therefore, for people who cannot acquire full republican citizenship is to grant them a residual, truncated status, similar to the liberal notion of citizenship as a bundle of rights. Bearers of this citizenship as status do not share in attending to the common good but are secure in their possession of what we consider essential human and civil rights (Oldfield 1990, 160–61). Precisely this type of citizenship is possessed by Israel's Arab citizens.

ISRAEL'S POLITICAL CULTURE

Israel's political culture and its constitutional arrangements are rooted in the experiences of the Yishuv, the Jewish community in Palestine under the British Mandate (1922–48). Jewish settlement in Palestine, which began in the 1880s, was conducted through most of its prestatehood period under the ideological banner of Labor Zionism. The leading ethos of that period was *chalutziyut* (pioneering)—the mutual redemption of the Jewish people and their land through physical labor, agricultural settlement, and military defense.

As a political community the Yishuv was semivoluntary in nature under the aegis of the British Mandate. *Chalutziyut* served as its civic virtue, the criterion by which (in theory at least) civic recognition was accorded to individuals and social groups. Thus, a distinction was made not only between Jews and Arabs but also between the (mostly European) Jews who came to Palestine to "build and be built" there and the (mostly non-European) Jews who were regarded simply as immigrants (E. Cohen 1983, 115–16; idem 1989a, 150; Eisenstadt 1948; idem 1950, 200–203; idem 1985, 102–24, 297–300; Horowitz and Lissak 1978, 120–56; Shafir 1989, 91–122; Y. Shapiro 1977; idem 1984).

As the "civil religion" of the Yishuv (Liebman and Don-Yehiya 1983, chap. 2), *chalutziyut* had a distinctly strong republican character. Like most nationalist ideologies, it embodied a collective sense of mission for an ethnically defined community and expressed it in a powerful founding myth. Israel's Declaration of Independence, which concluded the Yishuv period, proclaimed:

> The Land of Israel was the birthplace of the Jewish people. Here their spiritual, religious and national identity was formed. Here they achieved independence and created a culture of national and universal significance. Here they wrote and gave the Bible to the world. . . . Impelled by this historic association, Jews . . . in recent decades . . . returned in their masses. They reclaimed the wilderness, revived their language, built cities and villages, and established a vigorous and ever-growing community. (quoted in Medding 1990, 237; see also Y. Shapiro 1977, 10)

Two other strong republican elements in the Yishuv's political culture were a powerful connection between military and civic virtue and a collectivist economic philosophy that assigned great, if not overwhelming, significance to the achievement of national economic goals through the public sector. While individual rights and the procedural rules of democracy were widely respected, they were clearly seen, in line with republican thinking, as secondary in value to the collective Zionist mission. Thus, the liberal notion of the priority of right over good had a very weak grounding in Israeli political culture (Aharoni 1991; Y. Shapiro 1976, 2–4; idem 1977, 19, 29–30, 37, 41; Sternhel 1991; see also Kerber 1988, 1665; Sunstein 1988, 1551, 1564–65).[1]

The ethos designed to legitimate the transition to statehood in 1948 was termed *mamlachtiyut*.[2] It emphasized the shift from sectoral interests to the general interest, from semivoluntarism to binding obligation, from foreign rule to political sovereignty. Equal application of the law was of paramount importance if the state was to assert its authority over the various Jewish social sectors, which had enjoyed a large degree of autonomy in the Yishuv. These sectors (identified by their brand of Zionism—Labor, bourgeois, or religious) had maintained their own educational networks, health care systems, labor exchanges, and even military forces. They all had to be brought now under the jurisdiction of the state.

Israel's first premier, David Ben Gurion, was the great preacher of *mamlachtiyut* and its key element, the uniform rule of law:

> Only in a state in which everyone—citizen, soldier, official, minister, legislator, judge and policeman—is subject to the law and acts according to the law; only in a state in which there is no arbitrariness, neither of ministers or rulers, nor of representatives of the people, and also not of individuals and political leaders—only in a state such as this is freedom guaranteed to the individual and to the many, to the person and to the people. (quoted in Medding 1990, 138)

The state envisioned by Ben Gurion was not, however, a neutral,[3] nightwatchman, liberal state. It was to continue to be committed to the values of *chalutziyut* and to demand such commitment from its citizens:

> Even if in their private lives they act as *chalutzim*, both the individual, and the organizations of individuals, will fail if they do not put their *chalutzic* activity in the service

of the state, and if the state's financial, organizational and legislative power is not committed to the *chalutzic* tasks that are thrust upon us." (quoted in Medding 1990, 136)

Mamlachtiyut, then, was not meant to displace *chalutziyut* as the legitimating ethos (or "civil religion") of society but, rather, to subsume it within the organizational framework of the state. And individuals and social groups were to continue to be evaluated on the basis of their contributions to the common good as defined by the Zionist project (M. Cohen 1987, 201–59; Horowitz and Lissak 1978, 189–95; Kimmerling 1985, 272; Liebman and Don-Yehiya 1983, chap. 4; Medding 1990, 134–77).

The most important statutory manifestation of Israel's commitment to its Jewish moral purpose is the Law of Return. Enacted in 1950, this law guarantees immediate right of entry to every Jew who comes to Israel and expresses a wish to settle there (with the exception of those who might pose a danger to public health or security). The Nationality Law grants every person admitted under the Law of Return Israeli citizenship from the day of arrival in the country (Kraines 1976, 95–99; Kretzmer 1990, 18, 31n., 36–40).

When Ben Gurion submitted the Law of Return to the Knesset for its approval, he argued that the preferential treatment accorded to Jews in the proposed statute did not affect the equal citizenship status of Jews and non-Jews, because the rights granted to Jews—and only to Jews—in the Law of Return were not *given* to them by the state. These rights predated the state, and Jews had possessed them by virtue of being Jews. Moreover, the State of Israel itself came into being through the right Jews had always had to the Land of Israel and in order to enable them to fulfill that right. Therefore, Israel could neither grant nor deny Diaspora Jews the right to settle in the country. In its positive acts, Ben Gurion vouched, the state would always maintain strict equality between its Jewish and non-Jewish citizens (Israel, Knesset 1950, 2035–37).

Ben Gurion's argument that the state was committed to the fulfillment of a primordial Jewish right to the land—a right from which the state derived its very purpose—was a clear affirmation of republican, as opposed to liberal, principles. Liberals have recognized the right of "communal self-determination," that is, the right of a community to shape its own cultural character or provide refuge to its ethnic kith and kin (Walzer 1983, 35–51). Otherwise, few immigration laws would be legitimate from a liberal point of view. However, Ben Gurion's claim that the state was morally bound to promote some good that was antecedent to it and that, moreover, this justified making distinctions between the rights of different groups of citizens (or potential citizens) on the basis of ethnic affiliation is anathema to liberal political theory. It is perfectly acceptable, however, from a strong republican point of view (see Smith 1988).[4]

To sum up, then, the dominant strain in Israel's political culture may be termed *ethnorepublicanism*. Jewish ethnicity is a necessary condition for membership in the political community, while the contribution to the process of Jewish national redemption is a measure of one's civic virtue. This conception necessarily excludes the Arabs. As non-Jews, they cannot belong to the ethnically defined community; as those from whom the land is to be redeemed they cannot partake of Zionist civic virtue. I shall now show how, starting with this a priori exclusion, a liberal citizenship status has been constructed for Israel's Arab citizens.[5]

THE FOUNDATIONS OF ARAB CITIZENSHIP

At the conclusion of the 1947–49 Arab–Israeli war, 63,000 of the 160,000 Arabs who had remained in the territory of the State of Israel were granted immediate Israeli citizenship. The others could apply for citizenship under the Nationality Law promulgated in 1950 (Moshe Shapira in Israel, Knesset 1950, 2134–35).[6]

This granting of Israeli citizenship to the country's Arab residents constituted a renunciation of the ethnonational principle, which would have mandated the institution of a South Africa–style *Herrenvolk* democracy for Jews only. That the *Herrenvolk* democracy option was not even raised by any serious political force in the Yishuv attests to the weakness of ethnonationalism as a legitimating ideology among Jews in Palestine at the time. Similarly, ethnically blind liberal citizenship could not have become the norm, either; for this would have meant forfeiting the most essential goals of Zionism. Ethnorepublicanism, on the other hand, was not only a natural continuation of the tradition of the Yishuv but also the most attractive option available to Israel's founders; for ethnorepublican citizenship could reconcile the uniform rule of law with the assignment to Jews of "more than justice permits."

The extension of citizenship rights to Arabs did not stem, however, solely from Israel's commitment to equality before the law. Attenuating circumstances made that decision both easier and more necessary than it might have been. The Arab population of Israel had been reduced by the war from a 2:1 majority (in the area of Mandatory Palestine) to a small minority of about 12.5% of the population of the State of Israel (Lustick 1980, 48–49; Morris 1987; Kamen 1987–88). Moreover, the Jewish leadership believed "that the minority problem had been all but eliminated by the mass Arab exodus—that the number of Arabs left in the state was insignificant" (Lustick 1980, 53). This made it easier to include the remaining Arabs in the body politic of the state.

In addition, the international situation had to be considered, as well. In the deliberations leading to the partition of Palestine in 1947, the Zionist representatives had emphasized that "when we speak of a Jewish State we do not have in mind any racial state or any theocratic state, but one which will be based upon full equality and rights for all inhabitants with-

out distinction of religion or race, and without domination or subjugation" (Robinson 1947, 208). The UN Partition Resolution itself, which had legitimated Israel's founding in international law, required that elections to the legislative organ of the state be conducted on the basis of "universal suffrage" and that the state guarantee "all persons equal and nondiscriminatory rights in civil, political, economic and religious matters and the enjoyment of human rights and fundamental freedoms, including freedom of religion, language, speech and publication, education, assembly and association" (quoted in Medding 1990, 11–12).

Arab delegations at the United Nations complained about the treatment of the Arabs in Israel, and Israel's first application for membership in that body was refused in December 1948 (Lustick 1980, 61). Furthermore, the government sought to avoid the possibility that the remaining Arabs, who were concentrated in clearly demarcated geographical areas, would demand secession from Israel on the basis of their right of national self-determination—as has, indeed, happened in the territories occupied in 1967, whose residents have not been granted Israeli citizenship (ibid., 61–63).

After some debate in Ben Gurion's inner circle, Arab citizens were allowed to participate in the first general elections held in January 1949—at a time when the war had barely ended and prior to Israel's second, and successful, application for membership in the United Nations. Ben Gurion's reason for allowing the Arabs to vote was instructive: "We must not begin with national discrimination" (quoted in Medding 1990, 26). Significantly, however, over 60% of the Arab voters supported Ben Gurion's ruling party, Mapai, or its Arab affiliates, in these first general elections. This electoral pattern was maintained until 1965 (Rouhana 1986, 136).

The gap between the Arabs' formal citizenship status and their actual treatment (in this early period) as occupied enemy population was bridged by emergency decrees and administrative regulations. Most conspicuous of these was the military administration instituted in the areas of Arab concentration, "which imposed severe restrictions on their freedom of movement and economic opportunities, and placed them under surveillance and military law" (Medding 1990, 25; see also Dowty 1988; Jiryis 1976, 15–55; Kretzmer 1990; Lustick 1980).

Several Jewish political parties opposed the military administration, either because it violated the Arabs' citizenship rights or because it was seen as an instrument in the hands of the ruling party to secure for itself the lion's share of the Arab vote. In response to this pressure, many of the restrictive regulations against the Arabs had been gradually relaxed, beginning in the early 1960s. The final abolition of the military administration came, however, only in December 1966, following Ben Gurion's resignation from the government and the development of shortages in the (Jewish) labor market (Jiryis 1976, 31–55; Shalev 1989, 108; Smooha 1989, 15, 207).

One important *obligation* still denied to most Israeli Arabs is military service, which is mandatory for most Jewish citizens. This exemption is not a matter of law but of administrative practice and is probably unavoidable as long as the primary task of the Israeli military is to fight Arabs. Still, many social rights in Israel have been tied to the performance of military service, so that most Arab citizens either do not enjoy them or are entitled to smaller benefits (Kretzmer 1990, 98–107).

Moreover, the possibility of instituting some form of alternative national service for Arabs (which has been done in the case of religious Jewish women) has not been seriously considered by either the state or the Arabs themselves. Since any notion of citizenship entails the equality of rights and obligations—and especially in view of the extraordinary importance accorded national service in the republican view of citizenship (Oldfield 1990, 166–67)—this omission is highly symbolic of the Arabs' exclusion from attending to the common good. It has also become a major issue in the debate over the rights of Arab citizens. Thus, in the general elections of 1988 three right-wing Jewish political parties demanded that national service for Arab citizens become a condition for their enjoyment of full citizenship rights (Horowitz and Lissak 1989, 39–40; Nakdimon et al. 1988, 75, 133, 142; Smooha 1989, 96, 224).[7]

THE CODIFICATION OF ARAB CITIZENSHIP

The Challenge of al-Ard

The first major constitutional challenge to the citizenship status of Israeli Arabs came in 1965, an election year. Israel's electoral system is a proportional system, in which candidate lists submitted by political parties compete in a single national constituency for a share of the Knesset's 120 seats. These candidate lists have to be ratified beforehand by the Central Elections Commission (CEC), a semijudicial body made up of representatives of all parties currently in the Knesset and presided over by a Supreme Court justice.

Until 1965, the only Arab parties that participated in national elections were those affiliated with Zionist Jewish parties (except for the Israeli Communist party, which had been taking on more and more the character of an Arab party). In 1965, however, one of the lists of candidates submitted to the commission was the Socialist list organized by al-Ard (the Land), a small group of nationalist Arab intellectuals that had been previously declared an unlawful association by the minister of defense (Jiryis 1976, 187–96). The commission decided to disqualify the Socialist list on the grounds that it was "an unlawful association, because its promoters deny the [territorial] integrity of the state of Israel and its very existence" (Kretzmer 1990, 24). The commission's decision was appealed to the Supreme Court, which had to face the full force of

the contradiction between the different constitutional principles governing the citizenship status of Jewish and Arab citizens.[8]

By the legal situation that existed in 1965, the Central Elections Commission did not have the authority to turn down a list of candidates if that list had met all the technical criteria stipulated in the Elections Law (such as a certain number of signatories and candidates' ages, citizenship status, etc.). In other words, the commission had no statutory authority to pass judgment on the list's political goals or those of its members. Strict adherence to the existing law would have mandated the Court to uphold the appeal of the Socialist list—as was, indeed, recommended by one of the three justices. The other two justices, one of whom was the president of the Supreme Court, decided to deny the appeal on the grounds that the gravity of the issue placed before the commission justified its departure from the strict letter of the law (Gavison 1986; Kretzmer 1990, 22–26; *Yardor* v. *CEC* 1965; see also Smith 1988, 243–44).

There was some unclarity in the Court's written decision, however, as to what precisely was the issue before the commission: Was it that the al-Ard group objected to the Jewish character of the state or to its very existence? But if the main opinion for the majority, written by President Agranat, is read in its entirety, it becomes quite clear that the two issues were one and the same in his mind (*Yardor* v. *CEC* 1965, 384–89; see also Gavison 1986, 179, 181). Thus, Gavison has interpreted Agranat's opinion to mean that

> Israel's being a Jewish state established as the fulfillment of the Jewish people's dream of self-determination justifies, as a necessary condition, the disqualification of a list which does not accept the Jewish people's right to a state of its own (as distinguished from a list which accepts this right but objects to some of its political and legal manifestations or questions the conditions under which a Jewish state should continue to be maintained in Israel). (1986, 159)

Evidently, the Socialist list was judged to deny the Jewish people's right to a state of its own.[9]

Between 1965 and 1984 no Arab party had attempted to field a list of candidates in Knesset elections. (In 1980 a public meeting called by Arab organizations to discuss the possibility of forming an all-Arab political party was banned by the government.) Instead, Arab voters had been shifting their votes from Mapai and its successor, the Labor party, to the Israeli Communist party, whose following has become overwhelmingly Arab. Thus, the communists gained 51% of the Arab vote in 1977 and have been receiving about a third of the Arab vote since then (Rouhana 1986).

During the 1970s, following the abolition of the military administration in 1966 and the Six Day War of 1967, Israeli Arabs were becoming increasingly assertive of their rights and had shown growing willingness to use the political system for promoting those rights. A number of countrywide representative organizations had emerged in that period, such as the Committee of Heads of Arab Local Councils and the Committee to Defend the Lands (established to fight the expropriation of Arab-owned land by the state). These organizations have gained the grudging de facto, if not de jure, recognition of state authorities (al-Haj and Rosenfeld 1989, 1990; Lustick 1980, 246–49; Smooha 1989, 211–12).

1984

The ethnorepublican principle governing the status of Israel's Arab citizens was next seriously challenged before the general elections of 1984. This time ethnorepublicanism was contested by two political parties: a joint Arab–Jewish party, the Progressive List for Peace (PLP), headed by a former member of al-Ard and Rabbi Meir Kahane's ultranationalist Jewish party, Kach. The two parties couched their arguments in the language of liberalism and Jewish ethnonationalism, respectively (on the PLP see Lustick 1990; Rouhana 1986; Smooha 1990; on Kach see Peled 1990a, 1990b; Shafir and Peled 1986 Sprinzak 1986).

The joint premise shared by the PLP and Kach was that because Israel had a sizable Arab minority, the claim that it could be both Jewish and democratic at the same time was a charade. A state defined as Jewish could not treat its non-Jewish citizens as equals. A democratic state (by any acceptable sense of the term) could not give preference to any particular group. While agreeing on the need to choose between these two constitutional principles, the two parties differed diametrically in the choices they recommended. The PLP advanced the liberal argument that all discriminatory laws and practices favoring Jews should cease to exist and that the territories occupied by Israel since 1967 should be turned over to their Palestinian inhabitants. Kahane's party argued, in the name of Jewish ethnonationalism, that all Arabs, citizens and noncitizens alike, must be expelled from Israel and the occupied territories, and that the territories should be incorporated into Israel.

The Central Elections Commission decided to disqualify the PLP list because "this list believes in principles that endanger the [territorial] integrity and existence of the State of Israel, and [the] preservation of its distinctiveness as a Jewish state in accordance with the foundations of the state as expressed in the Declaration of Independence and the Law of Return" (Kretzmer 1990, 27; *Neiman* v. *Chairman of the CEC* 1985, 238). The Kach list was disqualified, as well, because the party was found to

> uphold racist and anti-democratic principles which contradict the Declaration of Independence of the State of Israel, openly supports acts of terror, tries to incite hatred and hostility between different sectors of the population in Israel, aims at offending the religious feelings and values of some of the citizens of the state and negates in its goals the foundations of democratic government in Israel (*Neiman* v. *Chairman of the CEC* 1985, 238).

Both parties appealed the commission's decisions to the Supreme Court and were reinstated into the

race. The Court ruled, on the basis of the 1965 al-Ard precedent, that in the absence of specific legislation empowering it to do so, the commission could not disqualify the Kach list for the racist and anti-democratic nature of its political platform. In the case of the PLP, the court decided that the commission acted within its authority in disqualifying the list for seeking to negate the existence of the state but that sufficient evidence had not been presented to the commission to merit such a determination. A number of the justices called upon the Knesset to enact legislation that would broaden the authority of the commission to disqualify candidate lists (Gavison 1986, 184–95; Kretzmer 1990, 27; *Neiman* v. *Chairman of the CEC* 1985, 226–33).

While in the case of Kach the implication of the Court's decision was quite clear, namely, that there was no legal way to bar from the electoral process a party seeking to expel the Arab citizens from the country, the implications of the PLP decision depended on an interpretation of the 1965 al-Ard case. If the Court is understood, in the earlier case, to have equated negating the Jewish nature of the state with a negation of its very existence, then one would have to conclude that the 1984 PLP decision meant that "a list cannot be permitted to stand for election if it denies the Jewish character of the state" (Klein 1985, 414; see also Gavison 1986, 159, 179, 181).

If a more restrictive interpretation of the 1965 decision is adopted, however—as was evidently done by the Court in 1984 (Justice Moshe Beiski and Justice Menachem Elon in *Ben-Shalom* v. *CEC* 1988, 242, 257; Gavison 1986, 180–81, 184–85), then the existence of the state and the preservation of its Jewish character must be seen as two separate issues. In that case the al-Ard precedent would apply only to candidate lists that seek to negate the very existence of the state. As Kretzmer has put it, "What of a list that explicitly wishes to repeal the Law of Return, but is sincerely committed to achieving this by the legislative process alone? The statement of the judges in the *Neiman I* [v. *Chairman of the CEC* 1985] case would seem to imply that such a list, which is not committed to physical destruction of the State, may not be disqualified under the *Yardor* [al-Ard] precedent" (1990, 27). This question, which bears directly on the ability of Israel's Arab citizens to have their own vision of the common good represented in the political process, was to be substantially clarified (and, in the case of Kach, completely transformed) by a series of actions that followed the 1984 elections.

1988

Both Kach and the PLP succeeded in electing representatives to the Knesset in 1984, with Kach winning one seat (for Rabbi Kahane himself) and the PLP two. This outcome—especially the election of Kahane, a vociferous racist demagogue—prompted the government to introduce legislation that would bar such parties from contesting future elections. The statute, an amendment to Basic Law—the Knesset adopted on July 31, 1985, reads:

> A list of candidates shall not participate in elections to the Knesset if its goals, explicitly or implicitly, or its actions include one of the following:
> (1) Negation of the existence of the State of Israel as the state of the Jewish people;
> (2) Negation of the democratic character of the State;
> (3) Incitement of racism. (Israel, Knesset 1985, 3951).

It should be noted that this amendment states the particularistic norm of Israel's being the state of the Jewish people explicitly, while the universal norm of equality before the law appears only implicitly, under the more general term *democratic*. During the debate, two changes proposed by members of Knesset belonging to the Israeli Communist party and the PLP (both predominantly Arab parties) were voted down. Their proposals would have removed or mitigated the particularistic content of article 1 by having it close with either "the State of Israel" or "the State of Israel as the state of the Jewish people and its Arab citizens" (Gavison 1986, 198; Kretzmer 1990, 29).

In 1988, prior to the next Knesset elections, the Central Elections Commission was presented with petitions to disqualify, on the basis of the amended Basic Law, the candidate lists of both Kach and the PLP, as well as almost every other party planning to participate in the elections. Kach, for example, requested that all other parties then represented in the Knesset be disqualified, since they all violated either article 1 or article 2 of the amendment. This, of course, was in line with its position that the Jewishness of Israel and its democratic system of government were mutually exclusive.

The commission deliberated seriously the cases of only three parties: the PLP, Kach and Moledet (Homeland), a new, extreme, right-wing party, which, like Kach called for the "transfer" of Arabs from the Israeli "control system" (Kimmerling 1989) but, unlike Kach, limited its demand to the noncitizen Arabs of the occupied territories (Nakdimon et al. 1988, 142). The commission approved the request to disqualify Kach, rejected the request to disqualify Moledet, and was tied (along strict party lines) in its vote on the PLP. The tie was broken by the commission chair, a Supreme Court justice, who decided that the PLP would be allowed to run (Israel, CEC 1988a–d).

Two of the commission's decisions (those relating to Kach and the PLP) were appealed to the Supreme Court. The Court rejected both appeals—Kach unanimously and the PLP by a 3:2 majority (*Ben-Shalom* v. *CEC* 1988; *Neiman* v. *Chairman of the CEC* 1988; Kretzmer 1990, 29). The appeals provided an opportunity for the constitutional implications of the amended Basic Law to be examined by the country's highest judicial authority. While the Court's decisions—against Kach and in favor of the PLP—seemed to uphold the liberal, as against the ethnonational, principle in the constitutional makeup of the state, a closer reading of the decisions reveals a much more complicated picture.

The fundamental constitutional, philosophical, and ideological issues involved in the decision to reject the appeal against the PLP were discussed thoroughly only in the opinions of the two justices who were in favor of upholding it. The three majority justices, including the president of the Supreme Court, rejected the appeal, like the Court did in 1984, for lack of sufficient evidence against the PLP. President Shamgar referred approvingly to the opinions of the two dissenting justices (*Ben-Shalom* v. *CEC* 1988, 246), who also concurred with each other (pp. 232, 257). The two other majority justices did not even refer to the opinions of their dissenting colleagues.

The opinions of the dissenting justices, buttressed by lengthy and profound analyses of the problem, were unequivocal. In the words of Deputy President Elon:

> The principle that the State of Israel is the state of the Jewish people is Israel's foundation and mission [*yessoda vi-yeuda*], and the principle of the equality of rights and obligations of all citizens of the State of Israel is of the State's essence and character [*mahuta ve-ofya*]. The latter principle comes only to add to the former, not to modify it; [there is nothing in] the principle of the equality of civil rights and obligations to modify the principle that the State of Israel is the state of the Jewish people, *and only the Jewish people*. (*Ben-Shalom* v. *CEC* 1988, 272; emphasis mine)

Moreover, according to Elon, the State of Israel was established for "the entire Jewish people, with all its individuals and with all of its communities, . . . both those living in the state and those residing in [the] diasporas and exiles" (ibid.). To claim that the State of Israel should be regarded as the state of its Jewish and Arab citizens, even if this claim is not accompanied by an explicit demand to abrogate the Law of Return,[10] is to call for turning Israel from a Jewish to a Jewish–Arab state. This is tantamount to negating the character of Israel as the state of the Jewish people. Since the PLP demanded complete equality between Jews and Arabs in Israel and extended this demand to the national character of the state, as well, it should have been excluded from the elections. The same argument was developed, at great length, by the other dissenting justice, Dov Levin (ibid., 234–46, 275–76; see also Kretzmer 1990, 30).[11]

Implications

Taken as a whole, the Supreme Court's decision in the 1988 PLP case meant that participation in Knesset elections could be legally denied to a list of candidates "that reject[ed] the particularistic definition of Israel as the state of the Jewish people, even if the list [was] committed to achieving a change in this constitutional fundamental through the parliamentary process alone." Moreover, in Kretzmer's view, the decision also implied that

> on the decidedly fundamental level of identification and belonging there cannot be total equality between Arab and Jew in Israel. The state is the state of the Jews, both those presently resident in the country as well as those resident abroad. Even if the Arabs have equal rights on all other levels the implication is abundantly clear: Israel is not *their* state." (Kretzmer 1990, 31; emphasis original)

This view is shared by Smooha as well: "From the Israeli–Arabs' viewpoint, the provision that Israel is the land of Jews all over the world, but not necessarily of its citizens, degrades them to a status of invisible outsiders, as if Israel were not their own state" (Smooha 1990, 402).

In the opinion of these two Israeli scholars, then, the 1988 PLP decision explicitly excluded Israel's Arab citizens from full membership in the community. Thus, the Court seemed to uphold the ethnonational principle of legitimation over the liberal–democratic one. However, to appreciate the full import of the PLP decision, it must be seen in the context of two other acts connected with the 1988 elections: the exclusion of Kach and inclusion of Moledet.

In the deliberations of the Central Elections Commission on the issue of Moledet, it was quite clear that at least in the minds of the commission members, there was a world of difference between advocating the "transfer" of both citizens and noncitizens (Kach) and calling for the expulsion of noncitizens only (Moledet).[12] Much of the debate in the commission turned on the question of whether or not Moledet was really interested in expelling noncitizen Arabs only or whether this limitation was a mere tactical ruse designed to evade the racism and antidemocracy clauses of the law (Israel, CEC 1988d). It was obvious to Moledet's opponents on the commission that they had to prove that its "transfer" plans included citizen, as well as noncitizen, Arabs. Since they failed to prove this to the satisfaction of their colleagues, the party was not disqualified. Nor was this decision even appealed to the Supreme Court, as the case of the PLP had been.

The decision to exclude Kach but not Moledet thus reaffirmed the distinction between citizen and noncitizen Arabs. It buttressed, in other words, the citizenship rights of Israel's Arab citizens. If viewed through the lens of ethnicity versus democracy, therefore, the decisions in the matters of the PLP, Kach, and Moledet together seem to point simultaneously in two different directions: they both limited and reinforced the Arabs' democratic rights.

I suggest, however, that the three decisions are perfectly consistent with one another. They constitute an affirmation of the ethnorepublican principle governing the Arabs' citizenship status over both the liberal and the ethnonational challenges mounted against it. Thus, a protective wall (so to speak) has been placed around Israel's Arab citizens—a wall that separates them from both the Jewish citizens, who can attend to the common good, and the noncitizen Arabs of the occupied territories, who are Israel's metics. Within the area confined by this wall, Arab citizens can securely enjoy (formally at least) the rights of liberal citizenship. They must not, however, challenge the existence of the wall itself.

CONCLUSION

Israel's Arab citizens are by no means satisfied with the place accorded them in Israel's political system (Smooha 1990). However, the limited, liberal citizenship status they enjoy has endowed them with sufficient rights and privileges to both enable and induce them to conduct their struggle within the constitutional framework of the state, rather than against it. This has been a key factor in allowing Israel to maintain a stable democratic regime in the context of an acute ethnic conflict.

The Intifada (Palestinian national uprising), which has been raging in the occupied territories since December 1987, has strained, but also underscored, the commitment of Israeli Arab citizens to abide by the law of the land. The Intifada greatly increased the momentum of Arab protest activity inside Israel (strikes, rallies, demonstrations, petitions to the government), which had been on the rise since the mid-1970s. Unlike the uprising in the occupied territories, however, these activities, with only marginal exceptions, have remained nonviolent and have been carried out within the framework of the law. The reason for this has been clearly articulated by Israeli Arab leaders. As Rouhana has put it: "the Arab leadership made clear that the Arabs in Israel would act only within the law. It was argued that their status is different from that of other Palestinians and therefore, that their efforts would be expressed differently" (1989, 47). Furthermore, the Israeli Arabs' increasing political confidence is fed, according to Rouhana, "by the sense of security that their status as Israeli citizens provides, as well as their formal equality before the Israeli law" (ibid., 54; see also E. Cohen 1989b, 85).

On a more fundamental level, their "Israeliness" is an important component of the Arabs' sense of collective identity, in spite of their growing self-identification as Palestinians. As Smooha has argued,

> The acquisition of various Palestinian identifications is manifested mostly in feelings of solidarity but not in a desire to dissociate oneself from Israel and to join any Palestinian entity. The overwhelming majority of the Arabs would be relieved by a settlement of the Palestinian problem, and in particular, 80% would feel better as Israeli citizens if a Palestinian state were to be established in the West Bank and Gaza Strip. Yet, only 8% of them . . . are willing to consider a move to such a state. (1989, 92–93)

Still, if my analysis is correct, there is a limit beyond which the Arabs' exercise of their citizenship rights will not be able to proceed. This limit is located at the transition point between struggling to have their liberal rights respected in the conduct of official policy and attempting to challenge the prevailing notion of the common good.[13]

As dominant ethnic groups in existing and aspiring democracies try to maintain both their dominance and a democratic system of government, they will very likely experiment with different varieties of two-tiered democracy. Thus, the constitutional arrangements that have made Israel into an ethnic democracy (especially the combination of republican and liberal citizenship) may become more widespread in the future (see Ginsburgs 1990).

The overt use of ethnicity (as of other particularistic markers, e.g., gender, class, or religion) to distinguish between different categories of citizenship is offensive to anyone committed to liberal values. It may serve us well, however, to remember that discrimination on the basis of ethnicity, race, religion, gender, and sexual preferences has not been unknown in liberal democracies. Moreover the very definition of citizenship in liberal political theory has had a particular bias built into it (as feminist writers have repeatedly pointed out):

> A citizen is one whose membership is contingent upon the ultimate subordination of the specific bonds of gender, race, and class—indeed, all particularized identities—in favor, most often, of a national identity and loyalty to the state. To become a citizen is to trade one's particular identity for an abstract public self. But the identification of citizens and the definition of citizenship is derived from the representation of the behavior of a group with particular race, gender, and class characteristics (white, male elites) as the model of citizenship for all individuals. (Jones 1990, 784; see also Hirsch 1986; Turner 1986, 46–47)

Thus, in the real world of democracy, the choice very often may not be between the faithful implementation of "All persons are created equal" and blatant group discrimination. It may be a choice between de facto discrimination under the guise of liberal constitutional principles and something resembling two-tiered democracy. Given this choice, an ethnorepublican arrangement whereby a subordinate ethnic group is confined to, but also made secure in the possession of, liberal citizenship rights and is allowed the political space to struggle for the consolidation of these rights may not be the worst possible outcome.

Notes

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1. It has been suggested that proportional representation is another characteristic of a republican polity (Sunstein 1988, 1585–89). Israel's electoral system is, of course, proportional in the extreme. It would be interesting to explore the connection between republicanism and proportional representation, both theoretically and empirically.

2. The conventional translation of *mamlachtiyut*, "statism," is unfortunate because of its protofascist connotations. Although *mamlacha* literally means "kingdom," *mamlachtiyut* conveys a sense of a common purpose, as determined by an all-encompassing social–political community and expressed

in the rule of law. Thus, the concept borders on Rousseau's *general will*.

3. Medding describes the Israeli state structures as "neutral," in the sense of being free of "partisan criteria" (1990, 9, 11). He also emphasizes that he does not mean to suggest that they are neutral in the class struggle (p. 28, n. 3). However, it would be exceedingly difficult to sustain the argument that the Israeli state is neutral as between Jews and Arabs (Smooha 1990, 393) or in the liberal sense of lacking a moral purpose.

4. Israeli jurists have tended to use liberal arguments in defense of the Law of Return. They have argued, for example, that as a law of immigration and naturalization and as a law that does not discriminate *against* any particular national or ethnic group, it does not violate the norms of international law (Smooha 1978, 399, n. 24). Ben Gurion, however, explicitly stated that the Law of Return was *not* an immigration law (Israel, Knesset 1950, 2037). Ironically, the fact that a similar law exists in the Federal Republic of Germany has also been adduced in support of the Law of Return (Kretzmer 1990, 45, n. 6). The Arab view of the Law of Return is that it gives the right to "return" to Jews who have never set foot in the country, while hundreds of thousands of Palestinian refugees are denied that right (see Smooha 1978, 197; idem 1990, 396). Another important, though largely symbolic, manifestation of Israel's moral purpose is the Nazi and Nazi Collaborators (Punishment) Law of 1950. In this law, Israel undertook to punish Nazi criminals for crimes committed before the state existed, outside of its territory, and in which neither the victims nor the perpetrators were its own citizens. The most celebrated case adjudicated under this law was that of Adolph Eichmann (Papadatos 1964, 32).

5. This sketchy description of Israeli political culture does not take account of much of its complexity or of changes that have occurred in it over time. The most comprehensive—though still very partial—account is Liebman and Don-Yehiya 1983.

6. The connection between the Law of Return and the Nationality Law had the effect, initially, of denying Israeli citizenship to many of the hundred thousand Arab residents not covered by the original grant of citizenship. This problem has been rectified, however, by a 1980 amendment to the Nationality Law (Kretzmer 1990, 37–39).

7. I do not provide a complete account of the status of Israel's Arab citizens, only review some of their civil and political rights. Of the authors cited, Smooha and Lustick provide the most comprehensive accounts of the situation of Israeli Arabs, while Kretzmer focuses specifically on their legal status.

8. In the absence of a formal written constitution and a bill of rights, the courts (especially the Supreme Court) have become the foremost guardians of civil rights and liberties in Israel (Arian 1989, 200–201; Lahav 1990). The Supreme Court has been playing that role primarily in its capacity as the High Court of Justice, entrusted by law with the authority to "issue orders against any body which performs a public duty under law" (Kretzmer 1988, 122). Although judicial review of legislation does not exist in the Israeli system, in recent years the Court has adopted an increasingly activist attitude vis-à-vis the Knesset, agreeing to pass judgment on all types of Knesset decisions, except primary legislation (Kretzmer 1988). The main reason for this development has been the inability of the other branches of government to make politically difficult decisions and their growing disregard for civil rights (Rubinstein 1991, 28). In what follows, I shall rely heavily on Supreme Court decisions in election appeals as indicative of changes in Israel's political culture.

9. It should be noted that the threat posed by the Socialist list was purely ideological. In the words of Justice Cohn, "In the material which was in front of the Central Elections Commission, and which was presented to us too, there was nothing to justify, let alone mandate, the finding that there is a [real or clear or present] danger" posed to the state or to any of its institutions by the activities of the list. This factual statement was not disputed by either of the other justices (*Yardor* v. *CEC* 1965, 381; see also Gavison 1986, 154). Gavison herself also believes that "principled commitment to the idea of Israel as the state of the Jewish people *does not* follow from, and is not mandated by, any conception of democracy. Therefore, if making such a commitment is allowed [under the law], then it may be justified to place certain limits on formal democracy in order to protect" that commitment (Gavison 1986, 160; emphasis original). This, in Sandel's (1984, 85–91) terms, is a perfect example of republican placing of good before right.

10. The PLP does not demand that the Law of Return be abrogated but, rather, that the right to return be extended to the Palestinian refugees, as well (D. Levin in *Ben-Shalom* v. *CEC* 1988, 241; Nakdimon et al. 1988, 118).

11. Justice Dov Levin objected specifically to, among other things, the preamble to the PLP platform, which is formulated in classic liberal terms: "In all of its struggles the List is guided by a paramount principle: Man is everything and the state is a tool in his hands. In this spirit the List shall struggle for another Israel—a state of all of its citizens" (*Ben-Shalom* v. *CEC* 1988, 240).

12. The distinction between expelling citizen and noncitizen Arabs is very much in the mind of the general Israeli public, as well. In an attitude survey conducted by Asher Arian in March of 1991, 39% of the respondents supported the idea of "transfer" when its intended victims were not specified; 34% supported it for noncitizen Arabs, and only 22% supported it for citizen Arabs (personal communication; see Peled 1990b, 356–57).

13. It has recently been suggested that the growing political polarization among Jews in Israel over the future of the occupied territories, coupled with the Arabs' rising numerical importance in the electorate, could result in Arab parties being included in the governing coalition in the not-too-distant future (Lustick 1989, 1990). But even disregarding the current massive immigration of Jews from the former Soviet Union, which has already diminished the Arabs' electoral weight (and which could not have been foreseen by Lustick), Arabs are not very likely to be allowed into the inner sanctum of political power in Israel in the foreseeable future. Even if the problem of national security could be overcome by having Arab parties support the government from "outside" (i.e., without sharing ministerial responsibility), the notion of a government dependent on Arab support runs against the grain of Israeli ethnorepublicanism (see Benvenisti 1991; Rabinowitz 1991).

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THE WELFARE STATE, TURNOUT, AND THE ECONOMY: A COMPARATIVE ANALYSIS

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While the economic voting literature is voluminous, comparatively little attention has been paid to the question of how—or whether—the economy affects turnout. I address this issue by examining national elections in 29 countries. Using time series data, the initial findings are replicated by a case study of American presidential and midterm elections since 1896. It is argued that the effect of economic adversity depends upon the degree of welfare state development. This relationship is argued to be nonlinear, so that mobilization occurs at either extreme while withdrawal obtains in the middle range. The importance to democratic theory, the study of elections, and the politics of welfare policy are discussed.

The past two decades have seen a tremendous amount of scholarly attention paid to the question of whether—and in what ways—macroeconomic conditions affect electoral outcomes. (For reviews, see Eulau and Lewis-Beck 1985; Monroe 1984). While this research program has become a veritable cottage industry, comparatively little attention has been paid to the related issue of how the economy affects turnout. Although much is known about how economic considerations affect the decision of how to vote, it remains to be seen how such considerations structure the decision of whether to vote.

The issue is relevant to a variety of theoretical concerns. If the economy does affect voter participation, our understanding of turnout is incomplete without a specification of this relationship. Similarly, without knowing how the economy affects who votes, it may be impossible properly to specify how the economy influences election results (Achen 1980). If turnout varies with the economy, there may also be implications for democratic responsiveness. If recession depresses turnout by keeping potential "punishers" from the polls, then government may escape retribution for economic mismanagement because the very people with the most interest in seeking such retribution may not vote (Rosenstone 1982).

TAKING STOCK

The basic proposition is that turnout deviates from an expected, or "normal," value in response to changes in the state of the macroeconomy. This relationship may take any of three possible forms. The first is what Schlozman and Verba (1979) call "mobilization." In this view, economic hardships encourage people to enter the political world as a means of seeking redress for grievances. If the literature on the "negativity bias" in political attitudes and behavior (see Lau 1982, 1984) can be accepted at face value, then individuals suffering hardship may be more strongly motivated than others to go to the polls to vent frustration at government's failure to manage the economy successfully.

The second possibility is that a sour economy encourages citizens to withdraw from the political process. Wolfinger and Rosenstone (1980) report, for example, that the poor and unemployed voted less in the 1972 presidential election (see also Caldeira, Patterson, and Markko 1985). Similarly, Rosenstone (1982) argues that economic adversity reduces turnout at both the individual and aggregate levels. Simply, economic downturns may foster preoccupation with personal problems that reduce the time and attention that may be paid to the more peripheral concerns of politics (Rosenstone 1982; Sniderman and Brody 1977).

The third possibility is the null of no effect. In this view, declines in financial well-being neither politicize nor alienate individuals. There is, again, some evidence supporting this hypothesis. Using survey data, Fiorina (1978) finds "no discernible relationship between economic conditions and voting turnout." (For similar conclusions, see Conway 1981.) Using aggregate data, Arcelus and Meltzer (1975) find insignificant or negligible relationships between congressional election turnout and several economic indicators, while Lane (1959) reports similar results for presidential elections. Schlozman and Verba (1979) also find that unemployment has essentially no effect on political consciousness, participation, or policy preferences.

The most striking aspect of the literature may be its inconsistency. In surveying it, Rosenstone (1982) concludes that mobilization, withdrawal, and no effect are all "plausible claims, but the conflicting and weak empirical findings make it difficult to choose one over the others." Although his own careful analysis makes a strong case for withdrawal, later findings do not uniformly support that conclusion (Southwell 1988).

In part, the confusion may result from measurement and methodological shortcomings.[1] Conceptual problems may also play a role. Longitudinal studies do not consider that response to economic adversity may vary temporally as social and political circum-

stances change. While cross-sectional studies avoid this problem by holding such factors constant, they are insensitive to the possibility that the results may not obtain outside of the particular economic or social setting. The lack of consensus in the literature may be a function of the fact that the phenomenon varies over time. Similarly, the exclusive focus on U.S. elections introduces a spatial constraint, as well.[2] It may be that the electoral importance of the economy varies both spatially and temporally due to contextual conditions that vary from place to place and time to time. One obvious possibility of such an intervening contextual variable is the extensiveness of security programs designed to insulate citizens against the arbitrariness of macroeconomic cycles.

DATA AND METHOD

The relationship between economic conditions and turnout is estimated with a standard linear model where the dependent variable is turnout, as measured by the proportion of the eligible electorate voting. The principal independent variable is the election year proportion change in real per capita national income.

Several controls are also introduced to account for noneconomic impacts on voter participation. One such term is a dummy variable representing sudden expansions of the electorate: the enfranchisement of large numbers of inexperienced voters may depress overall turnout. Another is an autoregressive measure of previous turnout. This variable represents any trend that is unrelated to economic performance. As such, it should capture all remaining noneconomic factors that systematically affect the vote series (Rosenstone 1982; see also Kramer 1971).

The data cover national elections for 29 countries from 1960 to 1987. Included are all countries with democratic electoral systems and a population over one million for which data are available.[3] Given that each case is an election in country c at time t, the resulting data set is a pool created across both spatial and temporal dimensions. As a consequence, the estimation procedure must account for the pooled structure of the data. (For a discussion, see Stimson 1985.) Because the source of potential bias is large between unit differences in the dependent variable (i.e., extreme international variation in the general level of turnout), the solution is to remove it by introducing a dummy variable for each spatial unit (except a reference category). In essence, this strategy estimates a "fixed effect" for each nation.

The inclusion of the dummies also serves a substantive purpose in that these terms, coupled with the lagged turnout variable, effectively embody the institutional, constitutional, and developmental factors that affect long-term turnout levels (see Jackman 1987; Lijphart 1984; Powell 1980, 1986). Thus, national differences in the overall pattern of voter participation are absorbed by the fixed effects. The moving average of past turnout (for each nation) should serve a similar function by providing a benchmark against which contemporary turnout can be judged, as well as adequately controlling for intrastate secular trends.[4]

A second issue that must be considered is the possibility that the relationship between the economy and participation may differ between wildly different political and socioeconomic environments. In part, of course, it is this consideration that necessitates the inclusion of variables that control for noneconomic factors. However, the distinction between the industrialized and developing worlds (or, equivalently, between nations with and without extensive social security provisions) may not be adequately tapped by such efforts. Because the model already includes a dummy for each country, it is impractical to include a developing world dummy and interaction term. Hence, to test for different economic effects in the different settings, I have simply divided the sample and performed the analysis separately for each set of nations:[5]

$$\text{TURNOUT} = a + b1(\text{INCOME}) + b2(\text{EXPECT}) + b3(\text{NEWVOTERS}) + b4(\text{State}_1) + b5(\text{State}_2) + \ldots + b31(\text{State}_{28}) + e,$$

where TURNOUT is the proportion of eligible voters casting ballots at time t;[6] INCOME is the election year proportion change in real per capita national income;[7] EXPECT is expected turnout based upon past voting trends, calculated as a moving average of the turnout at times $t - 1$ and $t - 2$; NEWVOTERS is a dummy variable coded one for years in which there occurred sudden expansions of the electorate due to the enfranchisement of previously excluded groups, and zero otherwise;[8] State_n are dummy variables for each of the $n - 1$ countries included in the study; and a and e are the intercept and error terms, respectively.

RESULTS

As indicated in Table 1, the coefficients of INCOME are statistically significant for both the industrialized and developing world. Interestingly, however, both the magnitude and sign differ between the nation groups. In the former, the coefficient is .17, while in the latter it is $-.75$.

The substantive conclusion is clear: the economic effect in the first world is withdrawal, while in the developing world it is mobilization. In the former, real income is directly related to turnout, so that sour conditions depress turnout. In the latter, income is inversely related to turnout such that when things are bad, citizens tend to vote in greater numbers. Furthermore, turnout in the first world is less elastic in relation to economic conditions than in the third world. A 5% decline in national income lowers turnout by approximately 1% in industrialized nations, while in the developing world the same economic fluctuation increases turnout by more than 3.5%.

TABLE 1

Turnout and the Economy

REGRESSOR	INDUSTRIALIZED NATIONS	DEVELOPING NATIONS
INCOME	.170*	−.747*
	(.081)	(.356)
EXPECT	.477**	.470*
	(.093)	(.214)
NEWVOTERS	−.019	.041
	(.079)	(.089)
CONSTANT	.407**	.280*
	(.080)	(.127)
R^2	.918	.423
N	174	38

Note: Table omits country dummy variables. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error).
*$P \leq .05$.
**$P \leq .001$.

The implication of the magnitude difference is that outside of the first world, citizens are more sensitive to economic conditions. In many ways this is unsurprising. The industrial countries have established reasonably generous security programs that mitigate against the most arduous affects of economic dislocation. In a situation where the level of economic attainment has reached the point where the sociopolitical system has significant cushions against minor economic perturbations, and where, as a result of that fact, many citizens may tend to be more politically sensitive to other than strictly economic issues, it may be that the economy has less of an impact on the decision to vote. Further, the emphasis on noneconomic issues may marginalize the immediate political importance of ordinary fluctuations in the business cycle. The effect, again, is to dampen the potential for either mobilization or withdrawal.

The more interesting (and unexpected) result is the qualitative difference in citizen reaction to economic performance. In the industrial world, poor economic performance tends to depress turnout in much the way that Rosenstone (1982) suggests. When faced with financial difficulties, citizens withdraw from the political process to devote attention to personal concerns. In the developing world, exactly the opposite occurs. Poor economic performance drives more voters to the polls.

The difference may be attributable to the stakes involved. In the absence of security programs, the potential human costs of poor economic performance are much greater. Thus, individuals may be considerably more sensitive to changes in the macroeconomy. In terms of costs and benefits, citizens may view the benefits of voting to be comparatively large when the economy is in decline. While the opportunity costs may in fact increase when the individual is faced with financial difficulties, the perceived benefits of voting may also increase in environments where citizens are more sensitive to short-term variations in economic performance. In industrialized nations, the benefits from going to the polls may be comparatively static. Hence, as a weak economy increases opportunity costs for many people, turnout can be expected to fall.

A commonly used measure of comparative spending on security programs is the percentage of gross domestic product devoted to government (or otherwise legislatively mandated) programs guaranteeing minimum income, health care, unemployment insurance, and pensions. (For a complete description, see International Labor Organization 1985, 1–10.) There is an enormous gulf between the developing and industrialized worlds on such spending. Using 1975 spending levels (roughly the midpoint of the time frame), no developing state spent more than about 5% of gross domestic product on social security, while many developed nations spent more than 20%. Even comparatively poor Western countries such as Portugal or Spain spent more than three times that of Turkey or Jamaica and nearly five times as much as India or Sri Lanka.

If the real issue behind the variable economic effect is the presence or absence of security programs, then presumably such would be apparent if this social security measure (SECURITY) and its interaction with INCOME were included in the model and the developing-versus-industrialized distinction dropped. Table 2 provides the results. The coefficient of INCOME is negative, while the interaction term is positive. The total impact of the economy on turnout at some level of security spending is the sum of (1) the INCOME coefficient and (2) the SECURITY × INCOME coefficient times the given level of spending.[9] Thus, the initial effect (at zero level of spending) is extreme mobilization, which is quickly dampened—and ultimately reversed—as a social commitment to security programs emerges. The absence of security provisions leaves economic discontent to be politicized. The introduction of such programs quickly reverses this effect. Withdrawal occurs as a level of security is established such that the most severe hardships of recession are blunted. In such cases, the deleterious effects of economic downturns are sufficient to cause preoccupation with personal affairs to the exclusion of now seemingly remote political concerns but no longer so threatening as to encourage political participation.

There is not, however, any a priori reason to believe that this pattern must necessarily be linear. Two logical alternatives are displayed graphically in Figure 1. The figure illustrates the economic affect on turnout as a function of welfare spending, that is, the total slope of INCOME (y-axis) at given levels of security spending (x-axis).

The first alternative is that the effect of security provisions may quickly level off after some minimum has been reached. Thus, the dampening of turnout in a sour economy might not be appreciably greater in Sweden or Denmark than it is the United States or

TABLE 2

Turnout and the Economy: Linear Security-Income Interaction Model (N = 212)

REGRESSOR	COEFFICIENTS
INCOME	−.580**
	(.207)
INCOME * SECURITY	.041*
	(.018)
SECURITY	.004
	(.005)
EXPECT	.290*
	(.144)
NEWVOTERS	.020
	(.025)
CONSTANT	.602**
	(.079)
R^2	.721

Note: P (INCOME < 0) > .997; P (INCOME * SECURITY) > 0) > .989. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error). Table omits country dummy variables.
*$P \leq .05$.
**$P \leq .01$.
***$P \leq .001$.

Australia. If true, this would imply a hyperbolic relationship between security spending and voter response to the economy, that is, $T = -aI/S + bI$, where T is the deviation of turnout from its expected value, I is INCOME, and S is SECURITY.

A second and equally plausible hypothesis is that a sufficiently large commitment to a security system further reduces the stress associated with economic dislocation and, with it, the opportunity costs so involved. Thus, one might expect withdrawal in the United States but no effect (or perhaps even mobilization) in Sweden. This would imply a second degree polynomial of the form $T = -aIS^2 + bIS - cI$, with terms as just defined.

The hyperbolic function requires that the interaction of the economic variable and the inverse of economic security spending be added to the original model. For the polynomial, the first-order economic and security interaction is retained and an additional interaction of INCOME and SECURITY^2 included. The results are reported in Table 3. In both cases, the coefficients are significant and of the correct signs. Further, the increase in the coefficient of determination (adjusted for the loss in degrees of freedom) in moving from the simple interaction (Table 2) to either

FIGURE 1.

The Economic Effect on Turnout as a Function of Welfare Spending

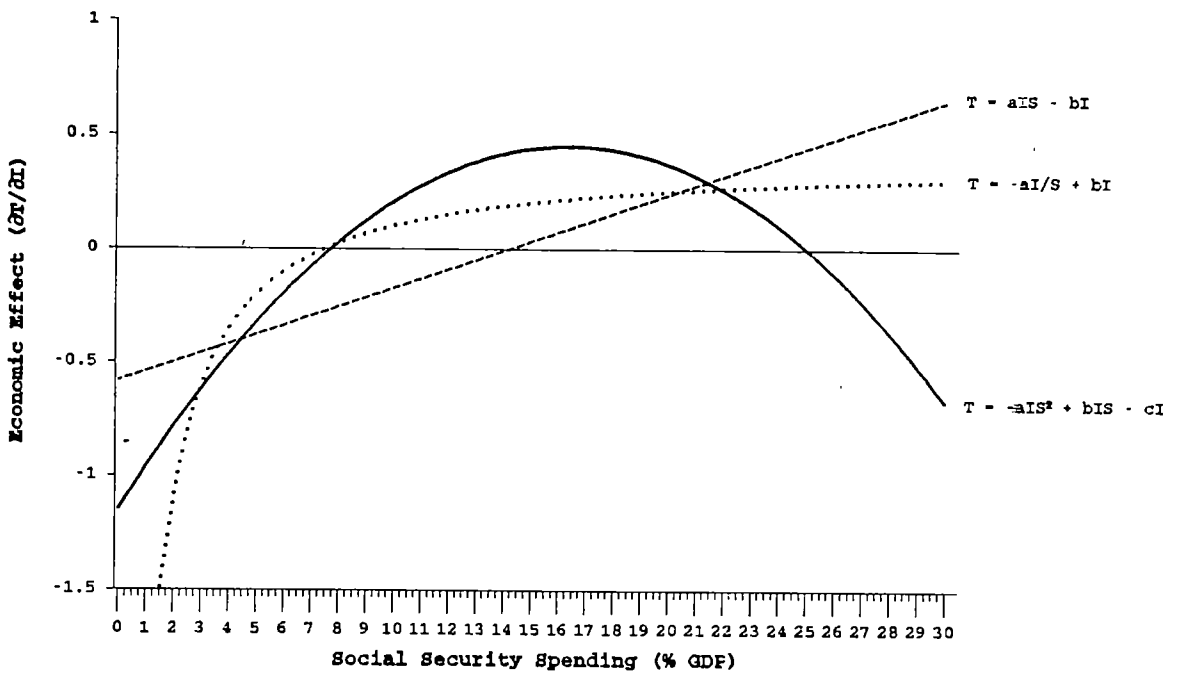


TABLE 3

Turnout and the Economy: Hyperbolic and Polynomial Interaction Models (N = 212)

REGRESSOR	HYPERBOLIC (1)	POLYNOMIAL (2)
INCOME	.403*	−1.15***
	(.191)	(.303)
INCOME/SECURITY	−3.00***	—
	(.750)	
1/SECURITY	−.288	—
	(.152)	
INCOME * SECURITY	—	.196**
		(.066)
INCOME * SECURITY^2	—	−.006*
		(.003)
SECURITY	—	.057***
		(.015)
SECURITY^2	—	−.003***
		(.001)
EXPECT	.906	.178
	(.556)	(.110)
NEWVOTERS	.031	.020
	(.038)	(.021)
constant	.690***	.489***
	(.086)	(.082)
R^2	.817	.848

Note: P (INCOME > 0) > .983; P (INCOME/SECURITY < 0) > .999; P (INCOME * SECURITY^2 < 0) > .993; P (INCOME * SECURITY > 0) > .999; P (INCOME < 0) > .999. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error). Table omits country dummy variables.
*$P \leq .05$.
**$P \leq .01$.
***$P \leq .001$.

TABLE 4

Economic Security Affect on Turnout in the Developed World

REGRESSOR	EXTREME LEVELS OF SPENDING (1)	MODERATE LEVELS OF SPENDING (2)
INCOME	−.274	.186*
	(.180)	(.081)
EXPECT	.132	.580**
	(.084)	(.095)
NEWVOTERS	.020	−.020
	(.036)	(.028)
Constant	.752**	.351**
	(.215)	(.076)
R^2	.806	.929
N	23	151

Note: Dependent variable is turnout; entries are unstandardized regression coefficients (standard error). Table omits country dummy variables.
*$P \leq .05$.
**$P \leq .001$.

the hyperbolic or the polynomial is extremely significant ($p < .001$ in both cases).

Which of these is (statistically) preferable is determined by goodness of fit. Based on this criteria, the polynomial is the superior specification, given that the R-squared in this model is greater in statistically meaningful terms than in the hyperbolic ($p < .001$).[10] The preferred specification is thus the polynomial just discussed, where a is the INCOME × SECURITY^2 coefficient, b is the INCOME × SECURITY coefficient, and c is the INCOME coefficient.[11]

The adequacy of the polynomial can be tested in another way. If this is the preferred model, then the relationship between the economy and turnout should be essentially constant among subsets of nations with relatively homogenous levels of security spending. Thus, the coefficient of INCOME in the original model (i.e., omitting the security interactions) should vary in a predictable fashion if the analysis is performed on appropriate subsets. It was previously demonstrated that dividing the sample into more- and less-developed nation groups produced a positive coefficient for the former and a negative one for the latter. A further distinction should manifest itself if the more-developed category is further divided into nations with moderate-to-high economic security spending and nations with the highest levels of spending.

Assuming a polynomial relationship, the coefficient should be positive in the former and negative or zero in the latter. A hyperbolic function (or, similarly, a logarithmic) would be indicated by both sets' having positive coefficients. As Table 4 illustrates, the data suggest the polynomial. In performing the regression separately for (1) the states at the extreme of economic security spending (France, the Netherlands, and Sweden) and (2) the remainder of the developed-world sample, the coefficient in the latter is positive and significant as expected, while in the former it is negative in sign though not significant at the conventional .05 level.[12] The failure to reach significance may be the result of a comparatively small sample size or may reflect an actual zero relationship. In either case, the result is congruent with expectations.[13]

EXTENSION TO UNITED STATES DATA

If citizen response to economic conditions is a nonlinear function of the extensiveness of social security provisions, then the same curvilinear pattern should manifest itself not only across countries with widely divergent levels of such programs but also within nations across time as security programs arise and develop. This hypothesis is examined with time series data for the United States from 1896 to the present.[14]

This strategy allows for a comparison with a similar

analysis by Rosenstone (1982). Examining elections until 1980, he shows that instability in consumer prices depressed turnout in presidential and midterm elections, with a 5% change in election year prices reducing participation by about 1% (Rosenstone 1982, 40–41).[15] I utilize the same model as follows:

$$\text{TURNOUT}(t) = a + b1(\text{PRICES}) + b2[\text{TURNOUT}(t-1)] + b3[\text{TURNOUT}(t-2)] + b4(\text{MIDTERM}) + b5(\text{WOMEN}) + b6(\text{YOUTH}) + b7(\text{WAR}) + e,$$

where TURNOUT(t) is the proportion of the eligible population that voted; PRICES is the absolute value of the election year proportional change in consumer prices;[16] TURNOUT($t-1$) is the proportion voting in the immediately prior national election; TURNOUT($t-2$) is the proportion voting in the penultimate national election; MIDTERM = 1 for midterm years, 0 otherwise; WOMEN = 1 if year is 1920 (advent of women's suffrage) 0 otherwise; YOUTH = 1 if year is 1972 (enfranchisement of 18-year-olds) 0 otherwise; and WAR = (1/(months at war)^2 if year is 1918, 1942, or 1944 (to account for lower turnout during the wars), 0 otherwise. The use of the inverse of squared months reflects the fact that the depressive effect of war on turnout declines over time as people accommodate themselves to the accompanying disruption of their lives (see Rosenstone 1982). For purposes of comparison, Table 5, column 1, replicates Rosenstone's result; parameter estimates are essentially identical with those he reports (1982, 40).

Some evidence for the importance of security spending is evident in the instability of Rosenstone's result. Simply expanding the time frame from 1980 to 1988 reduces the coefficient of the economic variable to insignificance ($t = 1.40$), as column 2 of the same table illustrates. The implication is that the significance of the original result depends upon the predominance of elections early in the series. Adding more recent cases dilutes the importance of the earlier elections, leading to a smaller coefficient (both absolutely and relative to standard errors). In effect, the slope of the regression line becomes flatter as more recent elections are added, suggesting that withdrawal has been lessened. This is precisely what one would expect if voter response to economic conditions was mediated by the level of social security commitment in the hypothesized fashion, in that the growth in security spending in recent decades should have eroded the withdrawal effect.

By the same logic, withdrawal should be most pronounced in the middle period between the birth and maturity of the American version of the welfare state. This era is demarcated on one side by the 1935 passage of the Social Security Act and, with it, the establishment of the first federal unemployment, old age, invalidity, and death insurance programs and by the era of expansion that began in 1965 with such new ventures as Medicare, Medicaid, foodstamps, and Aid for Families with Dependent Children.[17] Because there were delays from the actual passage of the legislation to meaningful implementation, I have added a five-year lag period. This approach also factors out the latter years of the Depression, so that the magnitude of the coefficients are not inflated (or otherwise "contaminated") by the extraordinarily large variation in economic conditions that characterized this period (Gough 1984). As Table 5, column 3, attests, the data confirm the expectation of a greater economic impact on turnout in this period. The coefficient of consumer prices is significant and is more than twice as large as the value for the original time frame of 1896–1980.[18]

TABLE 5

Economic Affect on Turnout in the United States By Era

REGRESSOR	1896–1980 (1)	1896–1988 (2)	1940–1970 (3)
PRICES	−.249*	−.200	−.629*
	(.121)	(.143)	(.246)
TURNOUT_{t-1}	.437**	.424**	.489*
	(.081)	(.070)	(.189)
TURNOUT_{t-2}	.252**	.260**	.065
	(.078)	(.097)	(.217)
MIDTERM	−.190***	−.183***	−.241*
	(.024)	(.027)	(.060)
WAR	−8.35*	−8.66*	−8.20
	(3.34)	(3.71)	(4.43)
WOMEN	−.057	−.082	—
	(.031)	(.047)	
YOUTH	−.045	−.039	—
	(.025)	(.041)	
Constant	.274**	.259***	.377***
	(.037)	(.037)	(.092)
R^2	.952	.871	.968
N	43	47	16

Note: Dependent variable is turnout; entries are unstandardized regression coefficients (standard error).
*$P \leq .05$.
**$P \leq .01$.
***$P \leq .001$.

A more rigorous test is to replicate the polynomial model from the previous section for the United States. Unfortunately, reliable data on security spending do not exist prior to 1949. Accordingly, I rely upon an alternative suggested by Cutright (1965) and successfully employed by several others (e.g., Aaron 1967; Jackman 1975; Pryor 1968). This measure is essentially a count of the number of extant economic security programs, weighted by the number of years they have been in operation. The International Labor Organization and the U.S. Social Security Administration index five types of such programs: work injury, sickness or maternity, old age and invalidism benefits, unemployment compensation, and family allowances. (For complete details, see International Labor Office 1985; U.S. Social Security Administra-

tion 1984). Thus, for each year a score of one to five is possible, indicating the extensiveness of coverage. While spending data is generally superior in that it represents both depth and extensiveness, the coverage index is highly correlated with spending (Cutright 1965; Pryor 1968) and is generally thought to be "an essentially equivalent measure of policy effort" (Jackman 1975, 15).[19]

The same interaction terms as in the previous polynomial model are added to this specification, subject to the already-discussed caveat that the SECURITY variable is replaced by the social insurance index (INDEX). Note that the expected sign of PRICES is the opposite of that of INCOME: a positive value indicates mobilization, a negative one withdrawal. Accordingly, the hypothesized total relationship between price instability and turnout, at various levels of security, is now of the form $ax^2 - bx + c$ (the mirror image of the relationship with real income). Table 6 indicates the results. The coefficients of PRICES and both interaction terms are statistically significant and of the correct sign. This evidence, coupled with the a priori expectation of this functional form, is strong reason to accept the polynomial as the preferred specification.[20]

TABLE 6

Economic Affect on Turnout in the United States: 1896–1988 (N = 47)

REGRESSOR	COEFFICIENTS
PRICES	.364* (.220)
PRICES * INDEX	−2.11** (.768)
PRICES * INDEX^2	1.08** (.400)
INDEX	.128*** (.035)
INDEX^2	−.079*** (.018)
TURNOUT$_{t-1}$	.105 (.084)
TURNOUT$_{t-2}$	.605*** (.074)
MIDTERM	−.079*** (.024)
WAR	−13.8*** (4.23)
WOMEN	−.122** (.039)
YOUTH	−.029 (.035)
Constant	.236*** (.039)
R^2	.935

Note: P (PRICES * INDEX^2 > 0) > .996, P (PRICES * INDEX < 0) > .997; P (PRICES > 0) > .951. Dependent variable is turnout; entries are unstandardized regression coefficients (standard error).
*$P \leq .05$.
**$P \leq .01$.
***$P \leq .001$.

DISCUSSION

I began with a test of the notion that turnout varies as a linear function of the state of the macroeconomy. In this formulation, deviations from an expected, or "normal," level of turnout can be expressed by the equation

$$T = aE + b, \quad (1)$$

where T is deviation in turnout and E is a measure of short-term economic performance. Perhaps the single commonality in the literature is an acceptance, explicitly or otherwise, of this specification. The disagreement is not over the model per se but over the sign of a—positive, negative, or zero.

I have argued that the actual relationship between turnout and the economy is conditioned by an intervening variable in a nonlinear fashion. Specifically,

$$T = -aES^2 + bES - cE + d, \quad (2)$$

where S is the level of societal commitment to social security and T and E are as just defined. For any given level of security, then, the economic effect can be estimated by taking the first partial derivative of T with respect to E: $\partial T/\partial E = -aS^2 + bS - c$. This quantity represents, in essence, the value of a in equation 1 at a specific level of security spending. As spending changes either temporally or spatially, the economic effect changes, as well.[21]

Still, the question who is affected by the state of the economy remains. In other words, what sorts of citizens are withdrawing or being mobilized? There are three hypotheses: (1) middle- and upper-income groups, (2) the whole population uniformly, and (3) the economically marginal. While the data do not speak directly to this issue, some tentative conclusions may be drawn.

The empirical results I have reported offer strong circumstantial evidence to suggest that hypothesis 1 is wrong, in that the middle and upper classes are the least likely to place much reliance on welfare programs. Hence, the hypothesis leaves little room for the welfare state variables to play a role. If it were these groups that primarily respond to economic conditions, the economic effect on turnout should be essentially uniform across different levels of welfare state development. Given that it is not, this hypothesis seems untenable.

An appraisal of the remaining hypotheses must begin with the observation that the economically marginal are more likely to suffer economic adversity than the middle- and upper-middle classes. Verifying what common sense suggests, Rosenstone (1982) reports that those who were unemployed or de-

scribed themselves as worse-off financially (on the usual National Election Study [NES] item) were much more likely to be poorly educated, low-income earning persons. Rosenstone also documents that those suffering from these problems are less likely to vote. Hence, even if we assume that economic adversity depresses turnout across all income strata, lower-income groups will be demobilized at significantly greater rates because individuals within these groups are disproportionately likely to face adversity. Thus, even if hypothesis 2 is correct, it will be the economically marginal that, as a group, are most affected. In terms of net results, then, hypothesis 2 reduces to hypothesis 3: oscillations in the business cycle predominantly affect the participation of working-class citizens.[22]

There may also be implications for economic voting and, thus, democratic accountability. While there has been a continuing debate over whether the economic criteria in voter decision calculi predominantly involve personal finances ("pocketbook voting") or assessments of the macroeconomy ("sociotropic voting"), there is now little doubt that the former plays an important (though not necessarily exclusive) role, at least in presidential voting. Using a pooled cross-sectional design and addressing the criticisms raised by Kramer (1983) and others over the inadequacies in the literature, Markus demonstrates that self-reported changes in one's personal financial situation are strongly linked to candidate choices in presidential elections. Hence, those who are worse-off are more likely to vote against the incumbent administration. Using precisely the same NES measure, Rosenstone (1982) demonstrates (as we have seen) that those who report that they are worse off are much more likely to abstain. Thus, given that (1) those personally affected by economic adversity are more prone to vote for the opposition and (2) precisely these same people are also less likely to vote, then, clearly, many of the people who would otherwise have "punished" the government will not turn out.

In assessing the political relevance of security measures in light of these findings, one is forced to consider the radical critique of the American welfare state. Welfare provisions, some critics have suggested, actually contribute to the marginalization of the poor in that such programs reinforce the status quo by preventing economic grievances from being politicized. Although the present study touches this issue only tangentially, the data do provide some evidence for precisely this sort of argument. In the United States, at least, the effect of security programs has been to help insulate the political system from the (electoral) expression of economic discontent. By effectively (and perhaps unwittingly) encouraging such individuals to withdraw from the political process, security programs have reduced their political importance.

On the other hand, the data suggest that the problem is not the welfare state per se but the distinction between "marginal" and "institutional" welfare systems (see, e.g., Korpi 1980). In the former (as exemplified by the United States), security provisions are poorly funded, highly selective, means-tested, and targeted toward specific groups that consequently "suffer stigmatization or resentment" by the rest of society (Ringen 1987). In the latter, epitomized by Sweden, programs tend to be universal, well-funded, and socially integrative (rather than divisive). Economic security is thus both more available and less fraught with social scorn in the institutional type than in the marginal. Thus, those in financial duress find opportunity costs to be higher in the context of the marginal welfare state. Turnout, consequently, is lowered in times of recession. As the institutional ideal type is more closely approximated, withdrawal is lessened and ultimately reversed. In such contexts, security programs not only fulfill their primary purpose of meeting material needs but may also encourage electoral participation in the face of economic hardship. In this fashion, the welfare state may actively contribute to the political empowerment of the disadvantaged.

Lastly, the results suggest that comparative studies of turnout should consider levels of welfare state development. Security programs appear to affect electoral participation systematically. Thus, it may be useful to consider security programs along with other institutional factors that are thought to account for cross-national differences (see Jackman 1987; Lijphart 1984; Powell 1986). In the same vein, more attention might profitably be paid to other directly political consequences of the modern welfare state. While the literature has addressed the sociological and economic results of this phenomenon, relatively little is known about its effect on the political world.

Notes

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1. With the exception of Rosenstone (1982), the aggregate studies generally fail to control for changes in the legal environment (such as the enfranchisement of 18-year-olds in 1972, which depressed turnout) and to factor out secular trends in turnout (due, e.g., to decreased party identification). The survey research also tends to inadequately control for individual-level correlates of turnout (e.g., Fiorina 1978) or to rely on a sample skewed by excluding citizens not registered to vote (Schlozman and Verba 1979). For a discussion, see Rosenstone 1982.

2. The one exception is Lewis-Beck and Lockerbie's (1989) analysis of hypothetical general elections in four West European countries. Using Eurobarometer data, they find that those who see the economy improving in the coming year are more likely than others to vote "if the election were held tomorrow." There are two serious problems with their analysis. First, their economic variable asks for an assessment of "whether, a year from now, the government's policies will

have improved the country's general economic situation." This question clearly taps an evaluation not only of the economy but of the government's management of it. As such, it would seem to be an indicator of partisan attitudes and more appropriate to how one might vote rather than whether one will vote. Second, opinions about one's propensity to vote are likely to be poor predictors of actual behavior, given that the cost–benefit calculations governing the expression of an attitude and the performance of an act differ quite radically.

3. Data are for legislative elections (lower house), except for strong-presidential, nonparliamentary systems. Data for Portugal include the constituent assembly of 1975. The 1983 Jamaican election is excluded due to opposition boycott and other irregularities. Similarly, the 1982 Sri Lankan election is omitted because of both ethnic violence and a dramatic change in electoral procedure. Data were unavailable for the 1986 Costa Rican election and the 1987 elections in Australia, Belgium, Italy, New Zealand, Switzerland, Trinidad, Turkey, and the United Kingdom.

In assembling a list of democratic countries, I have relied on the states identified as such in Butler, Penniman, and Ranney 1981 and Weiner 1987. These include the Western European democracies, Australia, Canada, Costa Rica, India, Israel, Jamaica, Japan, New Zealand, Sri Lanka, Trinidad, Turkey, the United States, and Venezuela. I have also excluded Colombia, on the grounds of both the extremely limited competition that occurred during the National Front era and the more recent problems of electoral violence. Malaysia, too, had to be omitted, because of the (arguably) limited competition characterizing its electoral system, as well as a lack of reliable data. The Dominican Republic is also excluded due to lack of reliable data. For the remaining countries data are generally acknowledged to be reliable.

4. The use of lagged data carries with it the potential for violations of the assumptions regarding the error terms. To account for this, all equations were also estimated with a generalized least squares model that controls for serial correlation. However, since the parameter estimates in this model did not differ meaningfully from those derived from ordinary least squares, I have reported the results from the simpler procedure.

5. Developing countries are Costa Rica, India, Jamaica, Sri Lanka, Trinidad and Tobago, Turkey, and Venezuela. While both Trinidad and Venezuela score in the same range as poorer European countries such as Portugal on some measures of economic development (e.g., per capita gross national product), the values for the former are artificially inflated by oil revenues. Further (as will be argued), the critical distinction between developed and developing worlds appears to be one of welfare state development. On such a measure, Venezuela and Trinidad belong squarely with the other developing countries.

6. Following Powell (1980, 1986) and Jackman (1987), I calculate turnout as the ratio of total (valid plus null) votes to the total eligible population. The eligible population is calculated as suggested by Jackman (1987, 419–21). Vote data is from Mackie and Rose 1982; Weiner and Ozbudun 1987; *The World Atlas of Elections*, *Elections on File*, and various years of the official statistical yearbooks of India, Jamaica, Trinidad, and Turkey. In a few cases, it is not entirely clear whether the reported figures represent total votes or merely valid votes. However, differences between the two tend to be very small, and the data are consistent for each nation.

7. The economic data are from the World Bank. A few missing cases have been filled with data from various editions of the United Nations *Statistical Yearbook*. While data from the two sources are not strictly comparable, there is no overlap—i.e., all economic data for a single country for a given year are from a single source. The election year time frame is traditional in the economic voting literature (e.g., Kramer 1971; see also Rosenstone 1982). Unfortunately, since only annual (rather than quarterly) data is available for much of the sample, economic change can be measured only in calendar years. This presents no difficulty as long as the election is held late in the year, but it is troubling for early elections. Hence, for contests in the first part of the year (before June), I have used change in the year prior to the election year.

8. While the institutional arrangements structuring voting remained essentially constant in most countries during the time frame in question, there were a few instances of important changes (e.g., the elimination of mandatory voting in the Netherlands in 1970). I experimented with fitting dummy variables for these events, as well, but dropped them, given that their inclusion did not affect the results. Note that the impact of these changes, as well as those of an expanded electorate, become absorbed into the EXPECT variable for later elections.

9. An additional statistical (rather than substantive) test of the predicative utility of the additional variables is to determine whether the increase in R-squared with their inclusion is statistically greater than the simpler model that excludes them. The difference between the R-squared reported in Table 2 for the expanded model and that which occurs when omitting the extra variables (i.e., for the basic model when examining the entire sample; details not shown) is significant at the .05 level ($F = 3.72$).

10. For the first order (Table 2) versus hyperbolic (Table 3, col. 1), $F = 21.6$. For the first order (Table 2) versus polynomial (Table 3, col. 2), $F = 83.3$. For the polynomial versus the hyperbolic (Table 3, cols. 2 and 1, respectively), $F = 19.2$. For a description of these tests, see Cohen 1968. Note that while the difference in explained variance between the polynomial and the hyperbolic is modest, it is highly significant. The relatively small increase is attributable to the fact that two functional forms are geometrically similar for most of the range of the data. It is only at extremely high levels of security spending that the fitted curves begin to diverge (see Figure 1). There are very few data points at this extreme, making it difficult for the proper specification to dramatically alter R-squared.

11. The use of multiple interaction terms does introduce the prospect of multicollinearity. The practical effect of this phenomenon is to inflate standard errors, so as to bias parameter estimates toward insignificance. However, when such (potential) inflation is not sufficient to interfere with the test of the relevant hypotheses (the signs of the coefficients), "then collinearity has caused no problem" (Belsley, Kuh, and Welsh 1980, 116). In the present application, the parameter estimates are quite large relative to their standard errors, so that there is little question as to their correct sign. For the terms of interest in equations where collinearity is a potential problem (the equations reported in Tables 3, 5, and 6), the probability of each coefficient's being the correct sign is .95 or larger. (See relevant tables for details.) Note also that the reported result is symmetric about the sign of INCOME, i.e., the same substantive implications obtain when dividing the sample into INCOME > 0 and INCOME < 0 subsets. Hence, both growth and recession affect turnout.

12. Still, the probability of INCOME's being of the correct sign is about .93. Note also that including additional countries with extreme social security spending (e.g., Denmark and Germany) produces the same substantive result.

13. Additional evidence for a (quadratic) polynomial form can be found by including the first-order INCOME = SECURITY interaction in each of the developing and developed world subsamples. In the former, the INCOME coefficient is negative and the interaction positive, indicating mobilization that gives way to withdrawal as security spending increases. In the latter, INCOME is positive and the interaction negative, indicating withdrawal that becomes mobilization with sufficient spending. If the true form is, in fact, a quadratic, then dividing the sample in this fashion and utilizing a linear estimation procedure would produce exactly this result.

14. The United States provides an ideal test case in that it has an electoral time series that is both long and dense. Other countries provide greater temporal variation in security spending but are hampered by a less-dense time series. The United States has constitutionally mandated biennial elections, leading to a far greater number of data points than

parliamentary systems with variable (and generally longer) interelection periods. Further, while the across-time range of security spending is larger in many other countries than the United States, effective variance relative to electoral data is less, given that substantial progress in the development of the welfare state was generally obtained much earlier in these nations than in the United States.

15. Turnout data are derived from Burnham's figures in *Historical Statistics, Colonial Times to 1970* for the period 1896–1934 and *Historical Statistics of the United States* (U.S. Department of Congress, 1990) from 1935 to the present. Consumer prices are from the same sources. To account for slight differences between the series, change in prices are determined for any given year entirely from a single source. Similarly, lagged turnout values are taken from the same series as the current value.

16. Note that PRICES is not strictly equivalent to the rate of inflation, in that it represents the *absolute value* of changes in prices and thus measures not inflation but instability in consumer prices, regardless of direction. Rosenstone (1982) introduces this term as a surrogate for two indicators that he found related to turnout: short-term unemployment and proportion of the population reporting itself worse off financially. Neither of these direct measures of economic adversity are available prior to 1948, which makes them clearly inappropriate for a time series analysis. I have followed Rosenstone in the use of this variable, in order to compare my results explicitly with his. However, the same substantive implications follow when using election year proportion change in per capita real income in its place.

17. A handful of states introduced unemployment schemes in the period shortly prior to the federal legislation of 1935. Prior to this period the only significant governmental programs concerned only work injury and were administered (and financed) by the states (along with a national plan applicable only to federal employees).

18. The result does not depend upon the selection of these precise dates as cutoff points. Similar findings obtain when using other points, such as 1935 and 1965.

19. The items comprising the index also form a Guttman scale, i.e., index scores genuinely represent a continuum of social insurance development, so that higher scores uniformly represent greater development than lower scores. (For a discussion, see Cutright 1965.) Note that I have actually used the index value divided by one hundred, so that the resulting parameter estimates will be of the same general order of magnitude as for the other variables. Note also that the same substantive results reported for the cross-national sample are also derivable from utilizing the social insurance index, instead of the spending variable. I have utilized spending data where possible because such data are more readily interpretable.

20. The increase in R-squared (adjusted for loss in degrees of freedom) is significant, since both the first- and second-degree interactions are sequentially introduced ($p < .001$ in both cases). Thus, the polynomial interaction is statistically superior to both the basic model and that containing only the simple PRICES × INDEX interaction. Note also that reestimating the model in Table 6 using real income instead of PRICES (and keeping the model otherwise unchanged) produces the same substantive implications.

21. Note that these values are, of course, precisely those derivable from the regression equation in Table 3, col. 2. Using this information, the maximum level of withdrawal is easily determined by setting the value $\partial^2 T/\partial S \partial E$ to zero and solving for S. The result indicates that this occurs at a security spending level of 16.1% gross domestic product, such that a 5% decline in INCOME translates into a 2% decline in turnout.

22. In fact, it seems unlikely that uniform magnitude would obtain, in that upper-income households will tend to have greater ability to withstand adversity than lower-income households. As Weatherford puts it, the working class is a "descendant group—one whose income opportunities decline faster than the overall average during a recession, and one which can less afford to cushion the decline with accumulated savings, property, etc." (1978, 922–23). Thus, not only will the proportion of working-class individuals affected by economic adversity be larger than that of the middle and upper-middle classes, but such adversity also will tend to have a greater affect on the former as individuals. Hence, at both the individual and group levels, financial difficulties should have a disproportional impact upon the turnout of the working class.

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RESEARCH NOTES

COMMUNITY VERSUS MARKET: A NOTE ON CORPORATE VILLAGES

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We return to the literature on collective villages and reexamine its central arguments. In doing so, we focus on an institution for allocating land that we call the Rule. As claimed by its advocates, the Rule secures land allocations that result in outcomes differing from those that would be achieved by markets. The outcomes are not constrained to be efficient. But the economic costs paid fail to secure the attainment of guarantees of subsistence, greater equality, or a greater sense of community resulting from the elimination of envy. However, in the face of high levels of risk, the form of collective property rights embodied in the Rule may create what we call "communities of (mis)fortune," in which no one can envy others' gains or losses resulting from the random shocks of nature.

Barrington Moore (1966) identified the peasantry as the revolutionary class of our time. The power of guerrilla forces in Vietnam rapidly convinced scholars of the validity of Moore's claim. Particularly when viewed from a developmental perspective, the revolutionary behavior of peasants posed deep analytic problems, however. Why should members of what arguably is a precapitalistic social formation stand at the forefront of modern revolutions?

Scholars offered competing answers to this question: dependency theory,[1] classical Marxism,[2] moral economy,[3] and rational choice.[4] Among the most heated of the resultant debates was that between the moral economists and the rational choice theorists. We reenter that debate, seeking to extend and deepen it by formalizing its central arguments.

Moral economists based their explanations of peasant revolution on an analysis of rural institutions, arguing that key peasant institutions (e.g., the corporate village) create collective, as opposed to private, property rights. They therefore support the attainment of values often sacrificed by markets: equality, community, and (above all) the right to subsistence. When market economies penetrate peasant societies, then, peasants react with moral outrage. Their defensive rejection of incorporation into the modern world economy provides a radical thrust to political protest in agrarian societies.

Rational choice theorists have attacked this argument on both empirical and logical grounds. For example, Popkin challenges Scott's depiction of the internal politics of corporate villages and demonstrates that the consequence of peasant revolution does not follow from the premise of peasant values, given the incentives to free-ride (Popkin 1979).

We focus on the logic of the arguments put forward by the moral economists. Our critique is formal and deductive; it isolates and examines the properties of an institution that we call the Rule. We aim our attack at the foundations of the moral economists' argument: the link between peasant institutions and values. In particular, we show that an archetypical institution of peasant society—the corporate village with collective rights in property—cannot logically provide a defense of the core values of peasant society: equality, community, and the guarantee of subsistence. The implication, of course, is that the moral outrage that fuels peasant revolutions must originate elsewhere than from the breakdown of this institution.

Although there are many other peasant institutions, (e.g., lineages, reciprocity, patron–client ties), other, highly complex forms of property rights, and other sources of peasant revolution, claims about what we call the Rule lie at the foundation of the moral economy argument. Hence, we have chosen to model that feature.

THE MORAL ECONOMY ARGUMENT

Central to the literature on peasant rebellions are studies of corporate villages. Corporate villages limit individual rights in property. The community restricts the rights of individual families to purchase, lease, alienate, rent, or use land. In some cases, corporate villages enforce forms of communal tenure. Land rights then remain fully vested in the community, and the village distributes the land among its members. In such cases, the corporate village may be called a repartitional commune.

While apparently relatively rare, corporate villages and repartitional communes have long attracted great attention. At least since the mid nineteenth century, scholars have written extensively about these forms of villages and made strong claims on their behalf.[5] Most recently, they have played a central role in the moral economy school of peasant behavior (Migdal 1974; Scott 1976; Wolf 1973).

The Russian *mir* constitutes perhaps the most famous example of such an institution. We shall draw our "stylized facts" from the literature on the *mir*. We focus upon several features that loom large in the scholarly debates, which provide the subject of our analysis. At any given time, these features may have

characterized some village communities but not others; or they may have been typical of the *mir* in one period but not in another.

The *mir* consisted of a collection of families who gained rights and incurred obligations through membership in a village. The most important right was to share the village's land, while the most important obligation was to share in the burden of its taxes. As the literature on the *mir* makes clear, the two were connected.[6] The rural community was assigned a tax bill by the state, the size of which was proportional to the number of the village's souls (a soul being a working-age person).[7] To meet its obligations to the state, the village then divided the tax bill among its member families while also apportioning them a share of the village's land—and thus the ability to pay the taxes.

Throughout, we will focus on the rules that governed the partitioning of village lands. Assuming the village to possess different kinds of qualities of land K_1 and K_2, the Rule assumes the following form: if a family i contains $L^i/L = C^i\%$ of the community's souls then that family receives $C^i = K_1^i/K_1$ of the village's land of type 1 and $C^i = K_2^i/K_2$ of the village's land of type 2, where L is the number of souls in the village, L_i is the number of souls in the family, K_1 and K_2 are the number of acres of type 1 and type 2 land in the village, and K_1^i and K_2^i are the amount of each type of land given to family i. In our model, type 1 land is of a better quality than type 2.

Discussions of collective villages stress the high levels of risk that confront agrarian societies. Rains can fail; disease can infect crops, livestock, or human beings; and prices can change, lowering the purchasing power of rural households. Given their small endowments and marginal physical and economic locations, rural families facing such shocks can starve. The possibility of the loss of subsistence constantly confronts the inhabitants of poor, agrarian societies.

Noting these characteristics of the institutions and environment of collective villages, scholars develop a powerful line of argument. Their basic contention is that collective villages are social—as opposed to economic—institutions. By this they mean that the allocation of factors of production in collective villages is determined by social relationships rather than by markets.[8] Land is not bought and sold; it is allocated by the community. Nor is labor a commodity to be exchanged in markets; rather, peasants draw their labor from their families. Markets thus play a limited role in the allocation of factors of production; the allocation is, instead, structured by social institutions.

From this premise, the contributors mount their first claim—that rural economies governed by collective villages generate outcomes that differ from those of markets. In particular, they do not allocate resources so as to maximize total output. The allocation of resources in such societies is not constrained to be efficient.[9] A second claim is that collective villages constrain the allocation of resources so as to enable rural societies to secure certain key values—presumably values threatened by the kind of behavior necessary to achieve the maximization of the total social product. The most important of these other values is the *security of subsistence*. Polanyi writes, "It is the absence of the threat of individual starvation which makes primitive society, in a sense, more human than [the] market economy, and at the same time less economic" (1957, 164). Characterizing the writings of Scott, Wolf, and others, Popkin notes: "Village procedures reflect this overriding concern of peasants with survival. . . The 'right to subsistence' is the primary normative concern" (1979, 11). Another important value secured by collective villages is *justice*. While not rigorously defined, the term is used in ways that imply equity and a concern with sharing. The village is a community of fortune, with no one able to control resources in ways that permanently privilege their life chances.[10]

In short, collective villages, based upon the division of the land among *all* families, are held to be "moral economies"[11]—in contrast to market economies, which tolerate inequality and where a person, if poor, can starve for want of access to productive resources.

CRITIQUE

As might be expected, the communal allocation of land generates allocations that represent an alternative to the outcome secured by markets, allocations that are not efficient. Contrary to what is also claimed, however, the losses in production do not generate benefits in terms of enhancing the attainment of social values in the shape of security of subsistence, equity, or the elimination of envy. The justification for the collective allocation of land must lie elsewhere.

The Set-Up

We assume that there are no markets for land or labor, land being allocated by the community and producers employing family labor; and that peasant production functions are concave.[12] We also assume the existence of two forms of taxation:

1. *Tax rule 1.* Families are taxed according to the number of souls, that is, working-age adults in the family.
2. *Tax rule 2.* Families are taxed according to the size of their land holdings.

Lastly, we omit from our analysis cases in which the community's tax bill exceeds its total product. We thus rule out cases where tax rule 1 is in effect but where the land is scarce or of very poor quality. We also rule out cases where, under tax rule 2, the receipt of additional land imposes losses upon families. In the absence of hired labor, the acquisition of additional land by a family can lead to a decline in the marginal product. Under tax rule 2, at some point,

the marginal product of an additional unit of land could thus be less than the increase in the tax bill resulting from its acquisition; land would then become an undesirable good. We shall assume that neither situation holds, although Wallace confirms that such situations did in fact occur in practice (1961, 283). The result is to bias the analysis in favor of the arguments in the literature, making our criticisms more telling.

Efficiency

Let there be two types or qualities of land, K_1 and K_2. Applying the Rule, each family i then receives a share of each type of land proportional to its share of the total village population; that is, if the relative size of family i is $C^i = L^i/L$, then it receives $C^i = K_1^i/K_1$ of type 1 land and $C^i = K_2^i/K_2$ of type 2.

If we assume that production on land of each type is Cobb–Douglas, then family i's production function can be written

$$Q^i = (K_1^i)^{\alpha_1}(L_1^i)^{\beta} + (K_2^i)^{\alpha_2}(L_2^i)^{\beta},$$

where K_1^i and K_2^i are the family's allocation of each type of land and L_1^i and L_2^i are the family's allocation of its labor L^i [$L^i = L_1^i + L_2^i$] across its holdings of the two types of land.[13]

Allocating its resources optimally, each family i will shift workers between plots until the marginal product of labor is equal on each type of land. But when individual families behave rationally and maximize their total production, the result is not "socially rational" in the sense of maximizing the total product of the village community. To see this, note that seeking to optimize, each family will devote labor to its land of type 1 until

$$\left(\frac{L^i - L_1^i}{L_1^i}\right)^{1-\beta} = \frac{(K_2^i)^{\alpha_2}}{(K_1^i)^{\alpha_1}}.$$ [14]

For a given family, the optimum use of labor on land of a given type is thus a function of its *total endowment of land of all types*. Given that land is allocated among the different families according to the Rule, the size of this endowment will vary among families.

The community allocates land so as to equalize the land–labor ratio across families for each kind of land; each family, behaving optimally, equalizes the marginal product of its labor on the two kinds of land. The result is that the community's allocation does not yield behavior by families (even though they optimize) that produces marginal products of land and labor that are identical for all families. Given that labor is not traded, we cannot expect subsequent reallocations of labor to equalize the marginal products of land among the village families. And given that land is not traded, we cannot expect subsequent reallocations of land to equalize the marginal products of family labor. The first-order conditions for the maximization of the total community product, therefore, would not hold, given the Rule, even though each peasant family might allocate its own land and labor so as to maximize the total family product.

The implication is that one of the claims of the moral economy is correct. Imposing rules to equalize the allocation of land per laborer, communal villages do indeed achieve outcomes that fail to maximize the productive use of their resources. As claimed in the literature, such communities abide by principles other than those embodied in the market. A key question then becomes whether (as is also claimed) the resultant allocation, while economically costly, is socially desirable, in that it supports the attainment of noneconomic values.

Subsistence Guarantees

A key value that communal institutions are supposed to provide is the guarantee of subsistence. However, the rule of allocating land to a family in proportion to the number of souls fails to provide that guarantee. Most obviously, in a bad year the Rule could not guarantee subsistence: guarantees of access to land would not guarantee access to output; for with natural calamities, production could fail. Under such circumstances, few systems could succeed. In fairness, then, the Rule should be subject to a less demanding test.

One test is the ability to underpin the subsistence needs of less fortunate families, that is, those with low marginal products. We can define subsistence needs as $sn + t$, where n is the number of mouths to feed, s the subsistence requirements of each "mouth," and t the tax bill. It is possible, of course, that members of a family could be so incapacitated that no amount of land, if operated by its members, could yield sufficient output to meet its subsistence needs.[15] It is also possible that the marginal product of labor of the family could be so low that given the increase in its tax bill (under tax rule 2) associated with the acquisition of additional land, the subsistence needs of the family would be imperiled by the receipt of greater amounts of land.[16] Even apart from such problems, there remains an additional difficulty: the lower the productivity of a family's labor, the greater the amount of land it requires to meet its subsistence target. Should the community seek to provide unfortunate families with sufficient land to meet their subsistence needs, it would have to reduce the amount of land supplied to other families. In seeking to guarantee the subsistence needs of its unfortunate families, in other words, it would necessarily place the subsistence needs of others at greater risk.

To illustrate, we once again assume that family production functions are Cobb–Douglas. Then, solving for the maximum possible total output yields an expression for the conditions necessary to guarantee subsistence:

TABLE 1

Applying the Rule: Equity, Envy, and Guarantee of Subsistence

FAMILY	MOUTHS (1)	WORKERS (2)	ACRES (3) TYPE 1	ACRES (3) TYPE 2	ACRES PER WORKER (4) TYPE 1	ACRES PER WORKER (4) TYPE 2
Family 1	10	3	6	12	2	4
Family 2	5	2	4	8	2	4

	TOTAL FAMILY PRODUC. (5)	PER CAPITA CONSUMP. (6)	TAX BILL (7)	SUBSIST. NEEDS[a] (8)	MARGIN OVER SUBSIST.[b] (9)
Family 1	5.6	.56	2.7	6.7	−1.1
Family 2	3.9	.77	1.8	3.8	.1

Note: This example assumes that the production $Q = K_1^{.5}L_1^{.5} + K_2^{.3}L_2^{.5}$, where $L_1 + L_2 = L$ is the number of workers. It also assumes that $s = .4$, $t1 = .3$, and $t2 = .15$, where $t1$ = tax per soul and $t2$ = tax per acre; the values are those necessary for the village to pay a total tax bill of $4.5 thousand.
[a]Defined as $sn + t$
[b](5) − (8)

$$L_i^{\beta}(K_1^{i(\alpha_1)/(1-\beta)} + K_2^{i(\alpha_2)/(1-\beta)})^{1-\beta} \geq sn + t \quad \text{for every i.} \qquad (1)$$

Clearly, families with low values of L^i would require larger quantities of K_1^i and K_2^i. If

$$\sum_i K_1^i > K_1 \quad \text{and/or} \quad \sum_i K_2^i > K_2$$

for every possible combination of K_1^i and K_2^i satisfying the above inequality, *then the requirements for guaranteeing the subsistence needs of all families cannot be met.*[17]

The Rule, then, does not, in general, provide subsistence guarantees. It can do so only under specific conditions, such as those just specified. It is important to note that these conditions are more restrictive the poorer the village community: the lower the quality of the land, or the poorer the quality of the labor, or the higher the tax bill, the smaller (according to expression 1) the margin over and above subsistence needs. The poorer the community, in short, the lower the margin left for it to secure the subsistence requirements of its least productive members while not imperiling the needs of others. Hence, applying the Rule cannot guarantee subsistence. In fact, it can increase the degree to which subsistence is threatened. And this threat becomes more severe, the poorer the rural community.

Other Values

By reducing the efficiency of land use, corporate villages impose economic costs. We have argued that these costs are not offset by the major benefit often claimed for them, namely, that unlike the market, corporate villages can guarantee that rural dwellers will not starve. In this sense, we can reject the claim that this alternative to the market is "more human" even though "less economic" (Polanyi 1957, 164).

Perhaps, then, the justification of communal villages lies in their ability to secure allocations that support the attainment of other values: the promotion of equality, the reduction of envy, or the strengthening of the potential for community. In exploring this possibility, we employ an example (Table 1). The example consists of a village containing two families living on 30 acres of land. One family has twice as many mouths to feed as the other: 10 for family 1, 5 for family 2. The families also differ in the number of souls (working-age persons): three for family 1, two for family 2. The village lands are of two types. The village is liable for a total tax bill of $4.5 thousand, which it assigns to its member families either by the number of souls belonging to the family or by the amount of land that the family farms.[18] Applying the Rule, the village then allocates to the two families shares in each type of land, the size of the share depending on the proportion of the village's souls who are members of the family. Family 1 therefore receives 6 acres of land of type 1 and 12 acres of land of type 2. Family 2, having two-thirds of the number of workers as family 1, receives 4 acres of land of type 1 and 8 acres of land of type 2 (Table 1, col. 3).

We can now examine the impact of the Rule upon equality, envy, and the attainment of subsistence. We assume a Cobb–Douglas production function with constant returns to scale, with arbitrarily selected parameter values. The validity of this counterexample rests solely upon the assumption of concavity, however.[19]

From column 4, it is clear that applying the Rule equalizes access to the means of production. As a consequence, neither family could envy the other's land endowment. Columns 5 and 6 remind us of an elementary but important truth, however: equality of land endowments fails to guarantee either equality of total product or equality of consumption. Family 1

TABLE 2

Payoffs Under Given States of Nature and Types of Land

TYPE OF LAND	STATES OF NATURE	
	GOOD YEAR	BAD YEAR
Type 1	4	1
Type 2	3	2

contains more workers, gets more land, and therefore produces more output; family 2 could therefore envy its neighbor's production. But family 1 also possesses more members; its members would therefore envy the consumption of members of family 2, who, having a lower ratio of consumers to producers, consume more per capita, even with their lower total product.

The general point is obvious: producing equality in one aspect of economic life, the Rule generates inequality in another. Insofar as the attainment of community requires the reduction of envy, the Rule, therefore, fails to provide the institutional foundations for a community in rural life.

In recognition of this point, some—especially James Scott (1976)—stress the significance of envy in rural life. They emphasize the jealousy faced by those who prosper and the pressures mounted upon the prosperous to divest themselves of accumulated assets by sponsoring public festivals and private clients. Insofar as these behaviors characterize the life of communal villages—i.e. villages governed by the Rule—they underscore the failure of this institution to promote community by reducing envy.

The example illustrates a last point, one made in the previous section. As seen in column 9, the Rule fails to equalize subsistence risk, much less to guarantee subsistence. It gives family 1 insufficient land to feed itself and pay its taxes.

AN ALTERNATIVE INTERPRETATION

Alternatively, we argue, constraints placed on the initial allocation of land could be construed as providing not guarantees but, rather, insurance.[20] The Rule could provide a form of insurance that equalizes the subjective impact of the variability of nature, thereby rendering villages communities in the sense of communities of (mis)fortune.

Consider Table 2.[21] Nature provides good years and bad. There are two types of land. Type 1 does better than type 2 in good years but worse than type 2 in bad. The application of the Rule ensures that every family will hold the two types of land in the same proportion; that is, for families i and j, $K_1^i/K_2^i = K_1^j/K_2^j = K_1/K_2 = \gamma$. As suggested in Table 2, the production achieved by a family depends upon how much of each type of land it holds and whether the year is good or bad. If good, then family i's production will be $4K_1^i + 3K_2^i$, which (given that $K_1^i = \gamma K_2^i$) can be rewritten as $K_2^i(4\gamma + 3)$. Reasoning in a similar fashion, family i's production in a bad year will be $K_2^i(\gamma + 2)$.

The consequence of equalizing the proportion of the two types of land in the endowments of each family is that should a bad year occur, each family experiences the same regrets.[22] For each family i, the difference in the production between a good year or bad is $K_2^i(4\gamma + 3) - K_2^i(\gamma + 2) = K_2^i(3\gamma + 1)$, yielding a percentage loss of $(3\gamma + 1)/(4\gamma + 3)$. In the absence of the Rule, γ would differ across families. Each family i's percentage loss in a bad year would then be $(3\gamma^i + 1)/(4\gamma^i + 3)$; and because γ^i varies across families γ^j, each family would experience different levels of regret under unfavorable states of nature.

The equalizing impact of the Rule upon land endowments therefore equalizes regrets in the face of unpredictable nature. This result represents a "positive" finding and, indeed, is the first positive result of this analysis. It should be noted, however, that it generates an interesting contradiction.[23]

We have already demonstrated that the Rule, while guaranteeing equality of endowments, cannot guarantee equality of production (or consumption). Therefore, by generating equal percentage losses (or gains) in the face of random shocks by nature, the Rule preserves these inequalities. Once ranked by production levels, when shocked by variations in nature, peasant families subject to the Rule would find their ranks preserved; for they would experience equal proportional gains or losses. We thus encounter a contradiction: the Rule equalizes regrets but at the expense of preserving inequalities in production and consumption.

One response is deeper skepticism about the claims in the moral economy literature concerning peasant equality. A more charitable response (which, indeed, we favor) is to garner an insight into peasant attitudes toward equality. Inequality is tolerable in agrarian societies so long as no family can gain relative to another as a result of the random acts of nature.

CONCLUSION

Claims about peasant society lie at the core of theories of peasant revolution. They also lie at the heart of debates between competing schools of scholarship. In particular, debates between the moral economy and rational choice theories of peasant revolution focus on one of the central institutions of peasant society, the corporate village.

We have returned to the literature on corporate villages, abstracted an idealized representation of a core property of that institution (the Rule), and examined its ethical features. We have found that (as claimed by their advocates) villages applying the Rule secure land allocations that result in outcomes differing from those that would be achieved by markets, namely, inefficient outcomes. But we find reasons to contest other claims made by the moral economists.

The economic costs incurred by using the Rule do not secure the attainment of other values, such as guarantees of subsistence, greater equality, or a greater sense of community resulting from the elimination of envy. In the face of high levels of risk, however, the form of collective property rights embodied in the Rule may create communities of (mis)fortune, in which no one can envy the gains of others resulting from random shocks of nature.

Our formal exploration of central arguments in the peasant literature yields two further lessons. The sense of moral outrage that impels peasants to violence cannot result from the breakdown of institutions based upon conventions resembling the Rule; for we have shown that such institutions cannot underpin the realization of the social values that provide the radical impetus to revolution. In light of this finding, the analysis also underscores the danger of basing theories of peasant revolutions on idealized images of self-contained peasant communities.[24] Theories of peasant revolution might better be based on the analysis of conflicts between peasants and "external" agents, such as exploiting classes or extracting states. In this sense, our analysis suggests explaining peasant revolutions by relying less on studies of the internal culture of peasant communities and more on studies of their external political and economic relations.

Fruitful extensions of our theory would include comparative analysis. The behavior of the Rule should be compared to that of the market. For given distributions of land and compositions of households and given distributions of random interventions by nature, the Rule might well secure socially desirable outcomes with a greater likelihood than the market. Our negative findings might appear less damaging, then, when subject to comparative analysis. Similarly, our investigation of the Rule should be replicated for other peasant institutions, such as lineages (Bates 1990; Posner 1980); common pool resource systems for water, forests, and grazure (Ostrom 1990); and share tenancy contracts (Stiglitz 1986). Only in this way can we fully assess the implications for the claims by the moral economists. Whereas one might argue that should the claims not hold for something as rigorously egalitarian as the Rule, then, a fortiori, they are unlikely to hold for other peasant institutions, another might reply that while the claims might not hold for this particular, highly stylized version of the Rule, they may hold for other institutions. We cannot resolve the matter, then, until we analyze these other institutions.

In addition to comparative analysis, we need to investigate the Rule itself more deeply. In what sense might it represent a "best response" by the members of a peasant community to a government-imposed tax? If, under the Rule, poor people could not advance relative to rich (given random acts of nature) why, then, would the Rule be chosen by a peasant community? We need answers to such questions.

Notes

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1. De Janvry (1981) offers a sophisticated version. More representative would be the interpretive sections of Barnett and Njama 1966 or Palmer and Parsons 1977.

2. Lenin's *Development of Capitalism in Russia* (Lenin 1964) stood as parent to this tradition, just as his *Imperialism* (1924) stood as parent to the dependency school. Some of the most interesting studies of rural accumulation and class formation come out of Africa. See, e.g., the contributions to the peasant debate in *Review of African Political Economy* 20 (1981).

3. Most notably, perhaps, Migdal 1974; Scott 1976; and Wolf 1957, 1973.

4. Most notably, Popkin 1979; see also Bates 1976.

5. See, e.g., the debates between the backers of Baron Haxthausen and B. N. Chicherin, summarized in Blum 1961, 508 ff.

6. This discussion draws on Blum 1961, 1971; Robinson 1949; and Wallace 1961.

7. Occasionally, an adult couple was defined as one soul.

8. The most notable development of this argument is that of Polanyi 1957. An influential amplification and elaboration is contained in Dalton 1971. See also Migdal 1974; Scott 1976; and Wolf 1957, 1973.

9. In typically vivid language, Eric Wolf distinguishes between collective villages and market economies: "Capitalism cut[s] through the integument of custom, severing people from their accustomed social matrix in order to transform them into economic actors, independent of prior social commitments to kin and neighbors. They [have] to learn how to maximize returns and how to minimize expenditures, to buy cheap and to sell dear, regardless of social obligations and social costs" (1973, 279).

10. For a vivid recreation of the power of this ethic, see Hinton 1972.

11. The phrase is, of course, Scott's (1976).

12. We employ a Cobb–Douglas production function throughout. In the extended example (Table 1), we impose an even more restrictive assumption, namely, that production functions are not only Cobb–Douglas but also possess constant returns to scale. We make these assumptions in order to generate numerical results. Our arguments, however, unlike our illustrations, depend not upon the specific form of the production function but solely on its being concave.

13. Note that we are assuming that the form of the production function is similar on each type of land. This might not be the case if, say, one type of land were used for arable production and the other for pastoral. See also n. 12.

James Scott, in a personal communication, indicates that the communal villages of Russia re-allocated land and labor that were "slack," with the result that the process of production in practice approximated the pattern that would prevail, had inputs been allocated through factor markets.

14. This result does not depend upon the assumption of maximizing behavior by the individual families. Using a Chayanov-type production function for the peasant household, for example, would only strengthen the result (Thorner, Kerblay, and Smith, 1966). Secondly, there is some evidence that following the initial allocation of land in accordance with the Rule, farm families did exchange portions of their land holdings (Robinson 1949, 112). These transactions, however, appear to have been limited and unauthorized and not to have fundamentally altered the initial allocation.

15. That is, the total product secured by a family, even with infinite amounts of land, would lie below $sn + t$.

16. As noted by Robinson under tax rule 2, such families would be placed at risk by being given more land (1949, 274).

17. Note that allowing laborers to have varying productivity levels will not alter this result.

18. In practice, given that the Rule allocates an amount of land to each family in proportion to the number of souls, the tax bill assigned each family is identical under the two tax systems (see col. 7).

19. The choice of specific values for the parameters of the production function are arbitrary. So, too, is the choice of a numerical value for s. The values of $t1$ and $t2$ are determined once the number of souls, the quantity of land, and the size of the tax bill are given.

20. The locus classicus of this approach remains McClosky 1976. For a critique, see Dahlman 1980. See also the very interesting work of Quiggin 1988.

21. The argument follows if (as would be natural in the context of the problem), the following general relationships hold: the cells of Table 2 are labeled a, b, c, and d (left to right; a, b in top row; c, d in bottom row), and $a > b$, $c > d$, $a > c$, and $d > b$.

22. It is important to stress that we do not mean *regrets* in the same sense as in decision theory. Our agents, after all, have no choices and so cannot regret their choice of strategies. Rather, by *regrets* we mean losses by comparison to what they could have received in a good year.

23. Note, as well, a less charitable interpretation, namely, that the Rule prevents a shift in the relative rankings of the village families, measured in terms of output, as a consequence of a random shock from nature.

24. It thus joins the criticism of the political economists who criticize traditional anthropological studies of peasant villages, treating them in abstraction from broader political and social forces. See, e.g., the critique contained in Wolf 1973.

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IDEOLOGY, STATUS, AND THE DIFFERENTIAL SUCCESS OF DIRECT PARTIES BEFORE THE SUPREME COURT

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A substantial literature on lower federal courts and state courts suggests that the "haves" usually come out ahead in litigation because they possess superior resources for it and they reap advantages from their repeat player status. We investigate the success of 10 categories of litigants before the Warren, Burger, and Rehnquist Courts to determine whether the resources or experience of litigants has effects on Supreme Court outcomes paralleling those found in the courts below. While different categories of litigants are found to have very different rates of success, those differences do not consistently favor litigants with greater resources. A time series analysis of the success of different categories of litigants over the 36 years studied suggests that the changing ideological complexion of the Court has a greater impact on the success of litigants than differences among litigants in resources and experience.

We examine the impact of litigant status and the changing ideology of the U.S. Supreme Court on differences in the success rates of direct parties before the Court. Simply, we seek to explain why some categories of litigants win more frequently than others when appearing before the Court. Previous explanations have attributed differential success rates in lower federal courts to, inter alia, disparities between litigants of different status in judicial experience and resources. We argue, however, that differential success rates in Supreme Court decisions have more to do with the ideological composition of the Court and the Court's receptivity to the different types of legal claims made by litigants of different status.

Previous research indicates that the status of litigants before American courts has substantial influence on judicial outcomes. Higher-status parties enjoy significant advantages in appellate courts and usually win. This has been demonstrated in the U.S. courts of appeals (Sheehan and Songer 1989) and, to a lesser degree, in state supreme courts (Wheeler et al. 1987). Curiously, the impact of litigant status on U.S. Supreme Court decisions has not been widely studied.

Among other arguments frequently advanced to explain status differentials in litigant success rates are that higher-status litigants typically possess superior resources or greater litigation experience (or both). Galanter (1974), for example, cites the advantage that corporations and especially governments enjoy as "repeat players" in comparison to less-experienced single-shot litigants. Repeat players are presumably better able to "play for the rules" in the legal process. They can settle cases likely to be lost in the courts and appeal cases they have the best chance of winning—and thus maximize their success rates.

In contrast, Sheehan and Songer (1989) stress the importance of resource differentials. They observe that even among single-shot litigants, the poor lose more frequently than wealthier individuals. Litigation, of course, is expensive. Litigants with superior resources can arguably retain better legal counsel, undertake more extensive research, and otherwise invest more in case preparation.

Another possibility, largely neglected in the literature, is that status differences in success rates reflect not only the relative resources and experience of litigants but also the values, ideological preferences, and prejudices of the Court. Numerous studies (Rohde and Spaeth 1976; Segal and Cover 1989; Tate 1981) document the effects that the political values of judges have on judicial decisions. Although no one to our knowledge has linked these values to litigant status, the connection is easily made in theory. Litigant status is related to the types of cases brought before the Court and to the positions taken by the parties. Cases brought by minorities, the poor, and individuals against businesses or government frequently emphasize claims of individual rights and liberties. Thus, they are likely to appeal to liberal values and to enjoy greater success in courts with liberal majorities. Conversely, cases brought by businesses against governments more typically seek regulatory relief, appealing to conservative values.

Our contention is that ideology is likely to be especially salient in cases before the U.S. Supreme Court. One reason is that litigant resource and experience differentials are likely to diminish both in size and importance at higher levels of the judicial system. Litigants whose cases have survived the gauntlet of the appeals process have proven they possess both considerable stamina and substantial resources. They also have acquired significant experience in the process. Assuming that status differences bear as much on decisions to grant certiorari as on decisions on merit, cases between litigants with the largest resource and experience differentials are unlikely to make it onto the Court's docket in the first place. Moreover, a case raising questions important enough

TABLE 1

Net Advantage of Parties, 1953–88 (%)

TYPE OF PARTY[a]	OVERALL SUCCESS RATE (1)	SUCCESS RATE AS RESPONDENT (2)	APPELLANT'S OPPONENTS SUCCESS RATE (3)	NET ADVANTAGE[b] (4)
Poor individual	31.2	38.5	70.5	−32.0
Minorities	55.6	44.3	34.9	9.4
Individual	42.9	35.2	52.6	−17.4
Unions	53.9	39.0	36.4	2.6
Small business	42.7	29.2	46.9	−17.7
Business	44.3	36.0	47.9	−11.9
Corporations	38.6	29.3	48.6	−19.3
Local govt	45.8	43.1	49.4	−6.3
State govt	54.1	51.0	39.8	11.2
Federal govt	67.3	59.1	23.2	35.9

[a]In order of increasing status.
[b]Col. 2 − Col. 3.

to be granted certiorari is likely to have sufficient cachet to attract quality counsel willing to argue the case for the visibility and prestige the case might bring. Parties to such cases can also frequently supplement their resources with contributions from concerned individuals and (especially) interest groups.

A second reason for hypothesizing that ideology will have a greater impact on the success of direct parties before the Supreme Court is our belief that ideological considerations are likely to be more sharply defined in cases reaching the Supreme Court. Again, because the Court controls its docket, it can select cases where peripheral issues are minimized and issues of principle predominate. Although there are exceptions, lower-level courts generally have less docket control and confront more cases where philosophical issues are obscured by other considerations.

DATA AND MEASURES

Data on the differential success rates of direct parties before the Supreme Court are available from the U.S. Supreme Court data base. The data base includes all cases decided with opinion by the Supreme Court between 1953 and 1988. Parties to these cases are identified by the labels given them in the Court's judgment or opinion. The detailed listing of parties in the data have been reduced to 10 categories: poor individuals, minority group individuals, other individuals, unions, small businesses, corporations, other businesses, local governments, state governments and the federal government.[1]

Since the Supreme Court data base is constructed from the written opinions of the Court, it does not provide direct measures of the financial resources or previous litigation experiences of litigants. To circumvent this problem we have adopted a strategy suggested by Wheeler and his colleagues (1987), ranking the 10 categories of litigants from strongest to weakest based on estimates of their financial resources and likely experience as repeat players in litigation. Although using status as a proxy for resources and experience precludes an assessment of the independent effects of resources versus experience, it does permit assessment of their combined effects vis-à-vis ideology.

To measure the changing ideological predisposition of the Supreme Court across the 36 years encompassed by this study, we use the ideology scores developed by Segal and Cover (1989) for individual justices.[2] To create a composite measure of the ideology of the Court we sum Segal and Cover's individual ideology scores across the nine justices who comprise the Court in any year.

RESULTS

Table 1 reports the success rates of the 10 categories of litigants in cases decided by the Supreme Court during the years 1953–88. As expected, success rates vary widely across the several categories of litigants, ranging from 67% for the federal government to only 31% for poor individuals. However, the overall success rates of litigants can be misleading. Because the Supreme Court is well known for its tendency to reverse decisions from below,[3] litigants whose cases are accepted on appeal are more likely to win than those who appear as respondents. To compensate for this, Table 1 also reports the net advantage of each class of litigant. This measure controls for appellant status by focusing upon the success rate of litigants in cases in which they are respondents and subtracting the success rate of their opponents in cases in which the opponents are respondents.[4]

If the resources and experience hypotheses are correct, we would expect the net advantage of the 10 litigant groups to vary directly with their status rankings. In fact, the relationship is quite weak. The federal and state governments enjoy significant advantages over other litigants, but so do minorities.

Individuals, especially the poor, are severely disadvantaged before the Court; but so are corporations and small businesses.

The overall net advantage of a category of litigants can be misleading, because the nature of the cases in which they typically are involved may bring them into conflict disproportionately with stronger or weaker opponents. To control for this possibility, we calculated the net advantage of each category of litigants against each of the other nine categories of litigants in our analysis. The results reinforce our observations both of the extreme variation in success rates among parties and of the limited impact of status differences (resources and experience) on success.

Predictably, during the 36 years spanned by our analysis, the federal government enjoyed very large net advantages against all other parties. But the federal government is the only party that consistently dominated lower-status parties. In Table 2, the party assumed to have superior resources and experience had a net advantage in 20 of the 29 comparisons (69%) for which sufficient cases are available. However, in 5 of those 20 cases, the advantage of the stronger party was negligible (i.e., five percentage points or less). Thus, the party presumed to be stronger enjoyed a clear-cut advantage in only 15 of 29 comparisons—just about what would be expected by chance if we knew nothing about the relative status of competing parties.

A different perspective on the resources argument is provided by examining the success rates of litigants over time. If resources and experience are determining factors in Supreme Court decisions, we would expect success rates to be relatively stable over time, since the relative resources and judicial experience of parties probably have not changed much over the years. In contrast, the ideological composition of the Court has changed dramatically. Thus, if ideology is a determining factor we would expect the success rates of different status litigants to vary significantly.

Table 3 displays the success rates for various combinations of litigants[5] across three periods corresponding roughly with the Warren Court (1953–70), the early Burger Court (1971–80) and the later Burger and Rehnquist Courts (1981–88).[6] Consistent with the ideology hypothesis and contrary to the resource hypothesis, the relative success rates of parties before the Supreme Court vary considerably. Predictably, individual litigants have suffered most from the growing conservatism of the Court, their success rates falling in relation to all other categories of litigants.[7] The pattern among other pairs of litigants is more complex. Businesses have fared better against the federal government but worse against state and local governments. State governments fared better against the federal government during the early Burger years but much worse during the Rehnquist years. Although the reason for this is not immediately apparent, an explanation is offered in a subsequent section.

TABLE 2

Net Advantage for Different Combinations of Parties, 1953–1988

COMBINATIONS OF PARTIES	NET ADVANTAGE BY %
Poor individuals vs.	
Federal government	Federal government, 52.4
State government	Poor individuals, 1.8
Minorities vs.	
Federal government	Federal government, 24.6
State government	Minorities, 12.2
Local government	Minorities, 24.7
Businesses (incl. corp. & sm.)	Minorities, 15.7
Individuals vs.	
Federal government	Federal government, 33.0
State government	State government, 17.6
Local government	Local government, 5.4
Businesses	Businesses, 2.1
Corporations	Individuals, 17.1
Unions	Unions, 10.6
Unions vs.	
Federal government	Federal government, 22.9
Businesses	Unions, 30.7
Corporations	Corporations, 16.7
Small businesses vs.	
Federal government	Federal government, 21.3
State government	State government, 23.1
Businesses	Businesses, 50.0
Corporations	Corporations, 5.7
Businesses vs.	
Federal government	Federal government, 43.8
State government	State government, .9
Local government	Local government, 5.7
Corporations	Businesses, 20.4
Corporations vs.	
Federal government	Federal government, 50.1
State government	State government, 28.6
Local government	Corporations, 32.1
Local government vs.	
Federal government	Federal government, 16.2
State government	Local government, 15.0
State government vs.	
Federal government	Federal government, 30.3

MULTIVARIATE MODELS

So far, examination of the resource and ideology hypotheses has been limited to comparisons of success rates across different combinations of litigants over time. Although illuminating broad patterns, these comparisons do not provide systematic estimates of the relative effects of either status or ideology. To obtain such estimates, we report logistic regressions in Table 4 for all Supreme Court decisions between 1953 and 1988 that involve some combination of our four aggregate litigant groups (i.e., individuals, businesses, state and local governments, and

TABLE 3

Success of Party Combinations over Time, 1953–88 (%)

TYPE OF PARTY	1953–70	1971–80	1981–88
Individual[a] vs.			
Business[b]	75.3 (174)	48.6 (138)	42.6 (129)
State and local	65.6 (518)	40.5 (870)	32.8 (585)
Federal	52.5 (503)	28.0 (414)	16.6 (259)
Business vs.			
State and local	58.7 (179)	42.6 (209)	47.5 (139)
Federal	25.2 (361)	27.6 (243)	31.5 (111)
State and local vs. federal	37.1 (35)	46.8 (47)	22.6 (31)

Note: Percentages are of cases won by first party listed. Number of cases in which the two parties opposed each other are in parentheses.
[a]Poor individuals, minorities, and other individuals.
[b]Small businesses, corporations and other businesses.

the federal government). Logistic regression is used since the dependent variable for each case is coded 1 if the litigant group won and 0 if it lost. Predictor variables in the four models include the status differential of the litigants (which serves as a proxy for differential resources and litigant experience)[8] and the aggregate ideological composition of the Court described previously. As a control variable, we also coded whether the litigant group in question was the appellant or respondent in the case.

Confirming the well-established tendency of the Supreme Court to reverse decisions from below, the models in Table 4 clearly indicate that all four classes of litigants have greater probabilities of success when they are appellants. The relationship is statistically significant and strong in all of the models and holds irrespective of the ideological composition of the Court or the relative resources of the litigants.

More important from our perspective, however, is the evidence of the relative effects of litigant resources and the ideology of the Court on the success of litigants. The evidence regarding the former is weak at best. In the business model and state and local government model, resource differentials appear to contribute significantly to the outcome of the case, although the size of the effect is quite modest. Resources, however, are not significant predictors of decisions in cases involving individuals or the federal government.

Further weakening the resource argument is the evidence that the effects of resources in the business model and state and local model stem almost entirely from the tendency of these parties to lose consistently to the federal government. In analyses in which cases involving the federal government are removed (not shown), resources cease to be statistically significant in the business model and are reduced to borderline significance in the state and local government model.

In contrast to resources and experience, the ideology of the Court has had considerable impact on the success rates of litigants of different status. This is most clearly seen for individual litigants who have enjoyed much greater success before the Court during more liberal periods regardless of the resources of opposing litigants or of their status as appellant or respondent.

Predictably, the ideology of the Court also has strong and significant effects on the success of state and local governments. Given that conservatism traditionally accords priority to the claims of order and community over the rights of individuals, it is not surprising that the success of state and local governments before the Court has been related inversely to the liberalism of the Court.

As we have seen, the federal government wins the great majority of its cases against all comers regardless of the relative resources of opposing parties or of the ideology of the Court. Nevertheless, the advantage enjoyed by the federal government in all situations increases significantly as conservatives gain in strength on the Court, although it increases at only about half the rate that the advantage enjoyed by state and local governments does.

TABLE 4

Logit Estimates for Models of Party Success

INDEPENDENT VARIABLES	INDIVIDUAL	BUSINESS	STATE AND LOCAL	FEDERAL
Appellant	.724*	.903*	.222*	.897*
	(.079)	(.104)	(.087)	(.102)
Ideology	.155*	−.018	−.140*	−.079*
	(.013)	(.032)	(.014)	(.016)
Resources	−.094	.231*	.257*	−.072
	(.062)	(.043)	(.058)	(.083)
Intercept	−.843	−.718	−.228	.653
Error reduction (%)	28	1	17	2

Note: Success = 1. Numbers in parentheses equal standard error.
*$p \leq .01$.

TABLE 5

Time Series Models of the Effects of Ideology on Direct Party Success

	INDIVIDUAL VS.			BUSINESS VS.		STATE VS.
	BUSINESS	STATE	FEDERAL	STATE	FEDERAL	FEDERAL
Ideology	2.9 (1.31)	3.7 (.973)	3.1 (.837)	1.1 (.611)	−1.3 (.393)	2.8 (2.00)
Noise model[a]	MA(3) −.39 (.156)	MA(3) −.29 (.145)	MA(1) −.34 (.094) MA(5) −.76 (.091)	—	MA(3) .39 (.162)	—
RMS[b]	289	193	80	289	100	1,239
ΔRMS[c]	14.1	18.5	35.4	18.8	25.9	2.3
Q[d]	14.0	7.1	6.8	5.4	5.8	12.0

Note: Models estimated with BMDP2T. Standard errors for parameter estimates in parentheses. Estimates more than twice as large as their standard errors are significant at the .05 level.

[a]Moving Average specification for noise; the order of the Moving Average process is in parentheses.

[b]RMS is the mean of the residual sum of squares for the model; it measures the model's "goodness-of-fit." Lower values indicate a better fit.

[c]Percentage improvement (i.e. reduction) of the full model compared to the noise model only.

[d]The Ljung-Box Q tests of autocorrelation in the model's residuals; with 12 degrees of freedom, none of the Q estimates are statistically significant at the .05 level.

A TIME SERIES PERSPECTIVE

The evidence so far suggests that the ideological composition of the Supreme Court has a much greater impact on the success of litigants than the resources the litigants possess. To better assess the aggregate effects of the changing ideological character of the Court on litigant success over the past three-and-a-half decades embraced by this analysis, we estimated a series of transfer function models using yearly success rates for each of the six pairs of litigants as dependent variables and the yearly index of the ideological composition of the Court as the independent variable. The transfer function models were estimated using standard Box–Jenkins methods (Box and Jenkins 1976). These enable us to measure the effects that ideology has had on the changing fortunes of the different categories of litigant over the 36-year period, while controlling for the well-known problems associated with time series (Granger and Newbold 1986).[9]

The transfer function models are reported in Table 5.[10] Since the ideology of each justice is measured on a scale of 1 (*most liberal*) to −1 (*most conservative*), the impact coefficients represent, in effect, the average increase in the success rate of one party over another resulting from the replacement of a relatively moderate member of the Court by a liberal member or a conservative member by a moderate one. For example, the coefficient for the impact of ideology on the success rate of individuals involved in litigation against state governments indicates that each unit-increase in the conservatism of the Court has reduced the success rate of individuals against states by slightly less that four percentage points. Although this impact may appear small, its effect over time can be substantial. Thus, from the high-water mark of liberalism on the Court in 1969 (*ideology* = 6.50) to the high tide of conservatism in 1988 (*ideology* = −2.02), the changing ideological composition of the Court was associated with a cumulative reduction in the success rate of individuals of almost 32 percentage points.

The pattern of effects in these models is very similar to that reported in the individual analyses. New is the evidence of the cumulative magnitude of the effect of the changing ideology of the Court. The impact of ideology on the success rates of individuals is especially dramatic. The increasing conservatism in the Court since the Warren years has been associated with a reduction in the success rates of individuals against business by 25 percentage points, against states by 32 percentage points, and against the federal government by 26 percentage points. As suggested in the analysis of individual cases, the conservative tide has hurt businesses relative to state and local governments. However, the cumulative loss, although statistically significant, has been less than 10 percentage points and has been compensated by an 11% increase in business successes against the federal government.

The one model in which ideology is not statistically significant involves cases where the federal government opposes local or state governments. Contrary to our expectation that the increasing conservatism on the Court should benefit states in their struggles against the federal government, the direction of the impact coefficient indicates that states have won less frequently against the federal government as the Court has grown more conservative. Although not statistically significant, the size of the coefficient is substantial. One possible explanation for this is simply that the number of cases involving the federal, state, and local governments as opposing litigants is relatively small. Since only slightly more than one hundred such cases have been decided over the

36-year-period, the success rates for states are highly volatile—especially in the early years, where the number of cases is especially small. Although this would account for the lack of statistical significance, it does not explain the large, contrary-to-theory impact coefficient.

Another possibility is that the impact of ideology on the success of the federal government may depend on the ideological or partisan complexion of the federal government as much as that of the Court. The increasing conservatism of the Supreme Court has not occurred in a vacuum. Rather, it is closely linked to the control of the executive branch of the federal government by conservative Republican administrations. Similarly, the increase in the liberalism of the Court in the 1960s was associated with the domination of that decade by liberal Democratic administrations. Although the ideological complexion of individual state and local governments also changes, the large number of states and localities and the cross-cutting nature of change mean that the aggregate ideology of states and localities is much stabler over time than the ideology of the federal government. Given this pattern, it is possible that the ideological orientation of the federal government as a litigant before the Court is linked to the ideology or partisan orientation of the administration and, therefore, that the success of the federal government versus states is a product *jointly* of the ideology of the Court and of the federal government. In other words, we hypothesize that Democratic administrations will win more frequently against state and local governments as the liberalism of the Court increases but that Republican administrations will win more frequently against states and localities as the Court becomes more conservative.

To test this possibility, we created a partisan-controlled measure of the success of state and local governments versus the federal government and estimated a new transfer function model of the impact of ideology. The results are encouraging. The impact coefficient is strong, negative, and statistically significant, indicating (as predicted) that the success of state and local governments against democratic administrations has declined as the Court has become more liberal and that state successes against Republican administrations have declined as the Court has become more conservative. Although the reduction in the residual mean square produced by this model still is quite small (about 5%), it is double the reduction achieved in the model without partisan controls.

Finally, the pattern suggested in these data help to explain the observation in Table 3 that the success of state and local governments against the federal government increased in the 1970s before declining in the 1980s. During the early 1970s, Republican administrations faced a still largely liberal Court where, as at the end of the decade, a Democratic administration faced a Court well on its way to becoming conservative. The result was an ideological and partisan environment much more conducive to the interests of states and localities than the 1980s with its conservative Court and Republican administration or the 1960s with a liberal Court and Democratic administrations.

CONCLUSION

Examination of the success of direct parties before the Supreme Court suggests that litigant resources and experience are considerably less important than in other appellate courts. The contrast between the Supreme Court and the U.S. courts of appeals is especially dramatic. Our analyses confirm the existence of marked differences in the success rates of different categories of litigants before the Supreme Court. However, these differences are not related consistently to litigant status and vary substantially over time in ways suggesting that these differences have little to do with litigant resources or judicial experience and much to do with the changing ideological composition of the Supreme Court.

Moreover, what little evidence there is for the importance of resources and experience stems almost entirely from the observed dominance of the federal government against all other categories of litigants. Although the federal government's consistent success before the Court may be a consequence partly of its superior resources and experience as a repeat player, it also may be due to a variety of other, non-resource-related factors. Among the more important of these, we speculate, is the long-standing *substantial evidence doctrine*, which encourages judicial deference to the federal government. In addition, our analyses provide evidence of what Dahl (1957) contends is a long-term political dynamic that tends to produce an ideological and partisan "alliance" between members of the Court and presidential administrations. This dynamic is most clearly evident in the increasingly conservative tenor of presidential Court appointments since 1980 and the Court's increasing support for the positions of conservative, Republican administrations during the same period.

Of course, this research can be faulted in a variety of ways. For example, the use of litigants' status as a proxy for their resources and judicial experience introduces considerable measurement error into the analysis, as does the identification of the ideological interests of litigants based on their group identities. As we have noted, even poor defendants with interesting cases can attract skilled and experienced counsel and the financial support of powerful interest groups. Were more direct measures of litigant resources available, we might observe smaller resource differences between litigants and these small differences might have greater effects on judicial outcomes.[11]

Similarly, the diversity of ideological points of view within categories of litigants means that the effects of ideology are almost certainly underestimated. For example, the weak effect that ideology appears to have on the success of state and local governments versus the federal government may result because

state and local governments frequently press claims (e.g., that their environmental regulations not be overridden by more conservative federal standards) that are more liberal than our underlying assumption recognizes.

Clearly, direct measures of litigant resources and ideological claims would be desirable. Unfortunately they are unavailable. Moreover, such data could not reasonably be obtained for a set of cases as extensive as the Supreme Court data base without a commitment of time and money equal, perhaps, to those already invested in that project.

Nevertheless, although limited by the availability of data and the need to rely on proxies as measures for key concepts, we have provided the first systematic analysis of the success rates of different categories of litigants appearing before the Supreme Court. In addition to demonstrating that success rates of litigants vary widely both across different types of litigants and over time, the analysis is at least suggestive as to the causes of these differences. Theory suggests—and our data support—the argument that litigant success before the Supreme Court depends substantially on the ideological composition of the Court but little, if at all, on the resources and prior judicial experience of the litigants.

Notes

We wish to thank Harold Spaeth for providing us with a preliminary copy of the U.S. Supreme Court data base, phase I.

1. The category of poor individuals includes only parties who were clearly identified in the Court opinion as *indigent*. The individual category is obviously underinclusive, since it excludes individuals who are poor or members of groups usually considered to be protected minorities. It also excludes individuals who appear before the court in their professional capacity. Corporations are defined simply as very large business entities (e.g., airlines, railroads, banks, insurance companies, and oil companies). Small businesses are those which are more likely to have an individual owner–proprietor, including bookstores, realtors, restaurants, and theaters. The residual business category includes businesses in between the other two or whose sizes are ambiguous. A number of parties were not encompassed in any of these categories, including professional associations such as the American Medical Association, churches, clubs, environmental groups, public interest organizations, and nonprofit groups. Since the resource levels or ideological orientations for most of these groups were ambiguous and the number of cases involving a number of these groups were quite small, we have excluded them from the analysis.

2. The construction of these scores and their underlying assumptions and limitations are discussed in detail by Segal and Cover (1989). Simply, Segal and Cover assign each justice an ideological score between −1 (*most conservative*) and 1 (*most liberal*) based on content analyses of newspaper editorial commentaries of the justices' judicial philosophies at the time of their appointments to the Court. The measure was constructed by, in essence, computing the percentage of paragraphs in the editorials of four newspapers that were coded *liberal, conservative,* or *moderate* by three separate coders: "Liberal statements include (but are not limited to) those ascribing support for the rights of defendants in criminal cases, women and minorities in equality cases, and the individual against the government in privacy and first amendment cases. Conservative statements are those with the opposite direction" (p. 559). The resulting construction reflects the perceived ideology of new justices relative to prevailing values at the time. There were a few early justices who were not included in the Segal and Cover study. We estimated a score for these justices from the scale analyses reported in Rhode and Spaeth 1976 and Schubert 1965. Based on their relative scale scores, the justices were placed within the Segal and Cover scale. Most of these justices were involved in a small number of cases. The two who were involved in a large number of cases were Black and Douglas, both of whom it was very easy to classify as liberals.

3. During the period under study, the Court's reversal rate was 67%.

4. This measure reverses the logic advanced by Wheeler and his colleagues in measuring the effects of the opposite tendency of state courts to *affirm* cases from below (1987, 418).

5. Because the number of cases for several categories of litigants becomes very small when disaggregated over time, we collapsed the 10 categories of litigants used in other analyses into four categories: individuals, businesses, state and local governments, and the federal government. Since unions do not fit naturally into any of these categories and were involved in relatively few cases, we dropped them from the remainder of the analysis.

6. These periods were chosen primarily on the basis of political factors surrounding the appointment of new justices and the effect of these appointments on decisional trends. The 1953–70 period corresponds with the appointment of Chief Justice Warren up to the appointment of Justice Blackmun. After the appointment of Blackmun in the early Burger Court, we see a significant change in the number of liberal decisions emanating from the Court. During the period 1981–88, we see an even greater drop in the number of liberal decisions delivered by the Court. These three periods allow us to obtain an indication of the effect of changes in the Court's composition and ideological direction, on the success of different types of parties.

7. It could be argued that the individual success rates are the result of the low success rates that criminal defendants generally have in appellate courts. To control for this possibility, the success rates of individuals were analyzed for criminal and noncriminal cases. Utilizing the same court periods, the success of individuals in criminal cases were 58.1% (Court 1), 31.8% (Court 2), and 25.1% (Court 3). In civil cases, individual success rates were 61.1%, 41.7%, and 30.1%, respectively. Thus, we conclude that the success of individuals has consistently declined across time in both criminal and noncriminal cases, and these data provide further evidence of the impact of ideological change on the fate of individual litigants.

8. The *status/resource* variable assumes a status continuum, along which *individuals* = 1, *business* = 2, *state and local governments* = 3, and *federal government* = 4. Thus, when a case involves the federal government versus individuals as litigants, the status/resource differential is 3 or −3, depending on whether the model is explaining federal success or individual success. Similarly, when an individual opposes a business, the business would be coded as having a status/resources advantage of 1 if the individual was coded as having a disadvantage of −1.

9. The use of Box–Jenkins procedures also makes it possible to test for Granger causality in the relationship between the Court's ideology and litigant success rates (Freeman 1983; Granger 1969). Strictly speaking, Granger causality implies that lagged values of X explain variance in current values of Y that cannot be explained by past values of Y. In this case, it means that past values of the Court's ideology explain current litigant success that cannot be explained by past values of litigant success. (E.g., the introduction of lagged values for ideology result in a significant improvement in the residual mean square.) In fact, the best-fitting model specifications for all litigant groups is a model in which the effects of ideology on success rates are unlagged. In every case, a model specified with a lagged ideology effect of one year was also

significant, albeit somewhat smaller. Although the specification of unlagged ideology effects weakens the case for Granger causality in these relationships, the significant improvement in the residual mean square registered in all of the models except the state-and-local-versus-federal-government one provides at least modest evidence of Granger causality.

10. The impact coefficients in these models measure the expected change in the success rates of an opposing pair of litigants produced by a one-unit change in the aggregate ideology of the Court. Several of the models have significant moving average components in the error term. These indicate the existence of short stochastic cycles. The statistics at the bottom of the table provide summary measures of the models' performance and indicate that all of the models perform acceptably (again, except for the federal-versus-state-and-local model). Ideology produces reductions in the residual mean square in each model of between 15% and 35%, and the Ljung–Box Q-statistic indicates that the residuals in all models are appropriately free of autocorrelation.

11. It should be noted, however, that our measure of litigant status/resources is the same used in previous studies where resources have been shown to be significantly related to judicial success rates (Sheehan and Songer 1989). Thus, the suggestion that resource differentials are smaller and less consistent among Supreme Court litigants and have smaller effects on litigant success is less a problem of measurement than confirmation of our initial hypothesis about the limited impact of resources on Supreme Court outcomes.

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CONTROVERSY

QUESTION WORDING AND MACROPARTISANSHIP

MacKuen, Erikson and Stimson used quarterly Gallup poll data to show in this Review *in 1989 that changing levels of macropartisanship, the two-party division of partisans, responded to presidential approval ratings and perceptions of the economy and predicted national election results. In a 1991* Review *research note Abramson and Ostrom argued that the NES and GSS questions more commonly used by scholars generated macropartisanship measures less sensitive to short term factors and less predictive of election outcomes. In this* Controversy, *Erikson and Stimson respond to the challenge and present new data from CBS News and* New York Times *telephone surveys to buttress their earlier analyses, arguing against substantial effects of the different question wordings. Abramson and Ostrom explain their continuing reservations.*

In a 1989 article (MacKuen, Erikson, and Stimson 1989), we presented the case for macropartisanship as a political indicator. As we defined the term, *macropartisanship* is simply the two-party division of party identifiers, as observed from quarterly readings of the Gallup Poll going back to World War II. We demonstrated major variation over time in the Gallup-based measure of party identification. Moreover, according to our statistical analysis, this movement in macropartisanship follows changing presidential approval and perceptions of the economy as monitored by the index of consumer sentiment. Based on our findings, the net balance of party identification fluctuates regularly in response to the political forces of the moment and predicts national election results (see also Weisberg and Smith 1991). If correct, our thesis has important implications for the understanding of partisanship in the United States.

Paul Abramson and Charles Ostrom (1991) challenge our conclusions about macropartisanship. Abramson and Ostrom concede most of our findings to be correct for the Gallup-based measure of macropartisanship. They contend, however, that our findings would be quite different had we used the party identification question asked by the Survey Research Center (SRC) of the University of Michigan instead of the Gallup version. Gallup has asked the question two slightly different forms.

> In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?
>
> In politics today, do you consider yourself a Republican, Democrat, or Independent?

The SRC asks,

> Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?

Echoing an earlier conjecture by Converse (1976), Abramson and Ostrom claim that the Gallup wording (*In politics, as of today*) invites short-term responses to transient events, whereas the SRC wording (*Generally speaking*) encourages stable responses anchored by long-term partisan attachments.

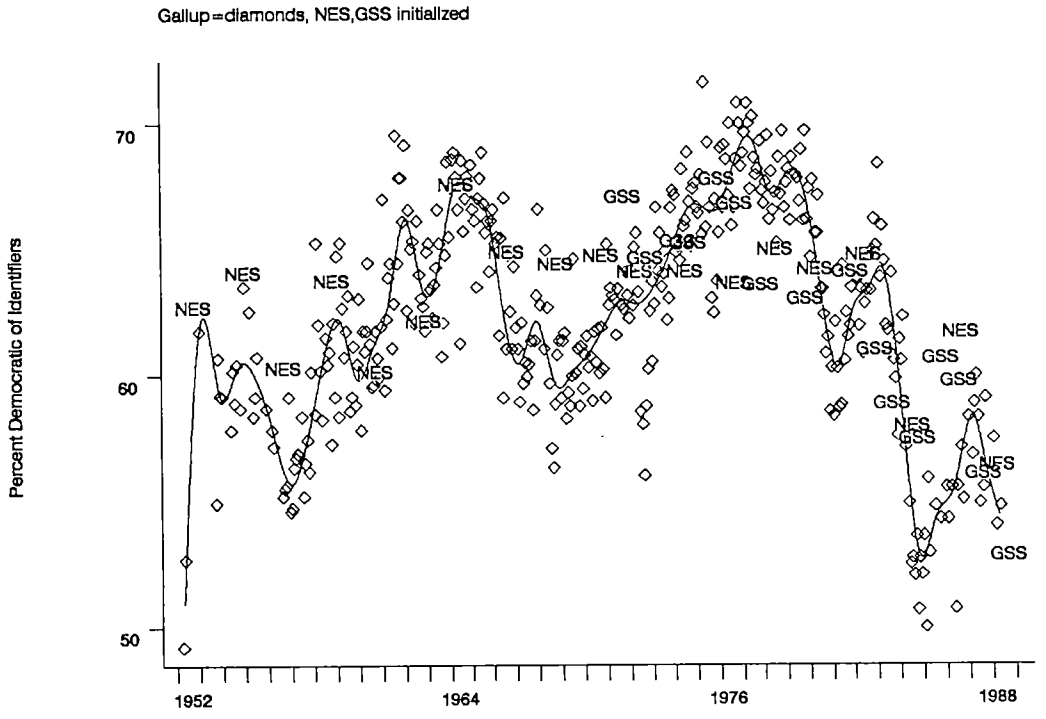
According to Abramson and Ostrom, most of our claims are "undermined" when the SRC question is used (p. 188). They say that we "exaggerate" the volatility of partisanship and "should have been more careful" in generalizing from the Gallup measure (pp. 190–91). For evidence, Abramson and Ostrom analyze the partisan division found in the 19 National Election Study (NES) surveys covering 1952–88, plus 16 General Social Surveys (GSS), each using the SRC question wording for party identification. Based on these limited time series, they claim that their preferred SRC version of macro party identification follows a more stable trajectory than the Gallup version. Moreover, they claim that their preferred SRC version is resistant to short-term influence from presidential approval or consumer sentiment. Abramson and Ostrom also question the electoral relevance of macropartisanship. They claim that our earlier paper overstates the degree to which even the Gallup version of macropartisanship can predict national election outcomes.

At stake is not just who got the story right but, potentially, the very meaning of party identification both for the individual and in the aggregate. More than ours, the Abramson-Ostrom position is compatible with the traditional orthodoxy regarding party identification, namely, that it rarely changes either for individuals or in the aggregate (e.g., Campbell et al. 1960; Miller 1991).[1] If Abramson and Ostrom are correct, true (NES) macro party identification moves sluggishly enough to sustain the traditional orthodoxy and our earlier report was a false alarm based on detection of major movement in the wrong variable.

Does so much turn on how different survey researchers frame the time horizon for their party identification question? We are thankful to Abramson and Ostrom for raising the matter of question wording. We certainly concede that question wording can be important in surveys. We also concede the likely presence of minor "house effects" affecting responses to Gallup, NES, and GSS party identification questions. But we also contend that Abramson and Ostrom's critique of our macropartisanship thesis misses the mark. As we will show, there is every reason to believe that our conclusions drawn from the Gallup time series would also hold for a lengthy time series of macropartisanship based on the SRC wording. Also (as we will show), routine movement in

FIGURE 1

Monthly Gallup Readings Overlayed with NES and GSS Readings of Macropartisanship



Note: Trend line is a computer-generated cubic spline for Gallup series.

macropartisanship does offer a clear signal of electoral change.

ARE THE GALLUP AND SURVEY RESEARCH CENTER PARTISAN SERIES DIFFERENT?

Abramson and Ostrom (1991) present a visual time series comparison of the NES and GSS party identification marginals with the Gallup marginals for roughly the same small number of quarters. Their NES and GSS readings (using the SRC wording) present a visibly and statistically flatter time series than the comparable Gallup quarterly readings. Figure 1 presents a comparison of all 35 NES and GSS readings for 1952–89 with the full set of 380 monthly readings (personal interview only) of the Gallup poll for the same 37-year period.[2] One cannot contest the fact that both series contain real movement. But consistent with Abramson and Ostrom's presentation, the NES and GSS readings display the least variance. The question is, Given the limited number of NES and GSS readings, do the NES and GSS "snapshots" of the macropartisanship series portray a phenomenon substantively different from the continuous Gallup series? On the one hand, virtually all NES and GSS observations are within the range of Gallup monthly observations for the same time period. And two key reference points—the 1964 Democratic peak and late 1980s Democratic nadir—are replicated nicely using NES and GSS measures. On the other hand, the sporadic NES and GSS series appears entirely to miss the "post-Watergate" Democratic surge of the mid-1970s captured so clearly by the Gallup measure. Are the two series really quite different, as Abramson and Ostrom argue? Or are the observed samples drawn from similar time series but with observable differences that are largely attributable to sampling error?

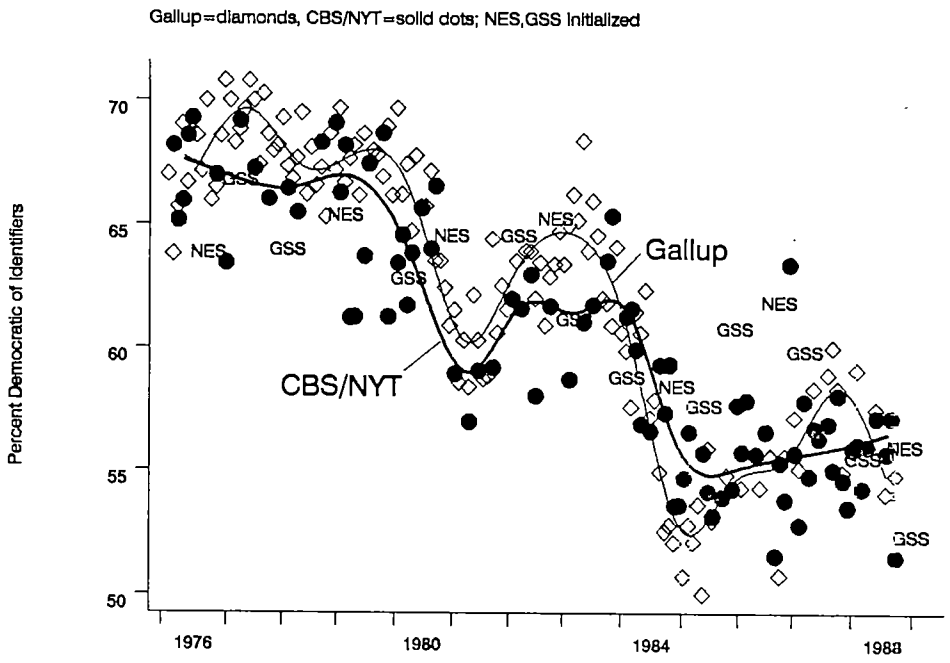
Happily, the NES and GSS are not the only surveys that measure party identification using the SRC question wording. Since 1976, CBS News and the *New York Times* (CBS–*Times*) have together conducted regular telephone surveys, using the SRC preface to the party identification question:

> Generally speaking, do you consider yourself a Republican, a Democrat, an Independent, or what?

Except for the innocuous substitution of *consider yourself* for *think of yourself as*, this is the SRC party identification question. Suppose we compare the CBS–*Times* party identification time series with the Gallup series for the same period. This comparison ought to decide whether the two-party identification questions produce substantially different marginals. Between 1976 and 1988, we have 133 monthly Gallup readings, with an average N of 2,052 partisan Dem-

FIGURE 2

Monthly Gallup and CBS–*Times* Readings of Macropartisanship, 1976–88



Note: Trend lines are computer-generated cubic splines.

ocrats or Republicans. For the same period, we have 88 CBS–*Times* readings, with an average N of 1,097 partisans.[3]

Figure 2 presents the CBS–*Times* versus Gallup comparisons, using monthly readings. (NES and GSS readings are also shown for comparison.) Figure 3 presents the same data in different format, as a comparison of two sets of collapsed "annual" surveys. Either way, there can be no doubt that the macro responses to the CBS–*Times* and Gallup party identification questions are virtually identical. At the beginning of the comparison in the later 1970s, the CBS–*Times* measure portrays the same post-Watergate Democratic superiority that the Gallup measure does. Pooled over all 1976–78 surveys, Gallup showed 68.0% of party identifiers to be Democrats (N = 81,195). For the same period, CBS–*Times* showed 67.2% of identifiers to be Democrats (N = 13,846). Also like the Gallup measure, the CBS–*Times* measure portrays the Republican resurgence of the 1980s. The Gallup and CBS–*Times* Democratic percentages of identifiers for 1986–88 are 56.4% (N = 31,626) and 55.6% (N = 28,777), respectively. The two measures also show similar bumps and wiggles over the 13-year span. Both show the two temporary Democratic surges (reflecting the "Reagan recession" and Iran–Contra).

The one difference is that the Gallup measure moves over a slightly greater range on the partisanship scale. This possible "house effect" may, indeed, be due to the difference in question wording highlighted by Abramson and Ostrom. But its magnitude is far too small to support an argument that the two measures measure inherently different things.[4]

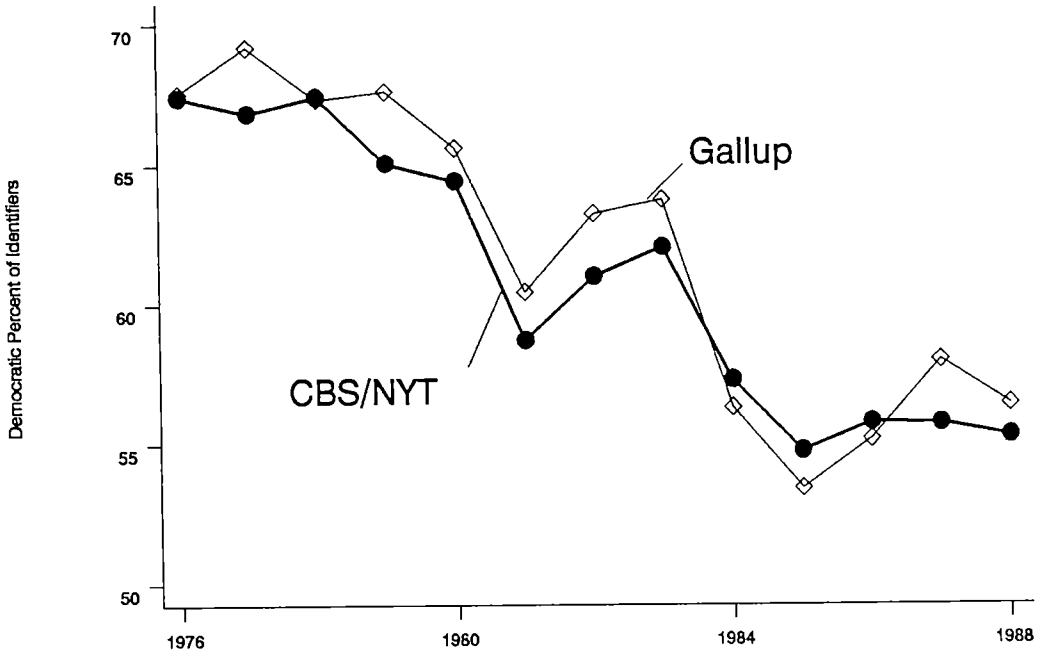
The Gallup versus CBS–*Times* comparison gives no support for any major difference in the party identification responses in respect to the time frame difference between the Gallup question (*In politics, as of today*) and the SRC question (*Generally speaking*). It is ironic, therefore, that the most obvious outliers among the observations of macropartisanship are found using the original NES and GSS "SRC" items themselves. Conceivably, an analyst might insist that the occasionally deviant marginals in the NES and GSS series (see, e.g., Figure 2) reflect the true measure of macropartisanship, while the more frequent Gallup readings and CBS–*Times* readings both offer false signals. But one can no longer use the rationale of question wording.

ACCOUNTING FOR MACROPARTISANSHIP

One of Abramson and Ostrom's claims is that unlike the Gallup measure, the SRC measure of party identification does not respond to presidential approval or consumer sentiment. Their evidence consists of a regression analysis predicting 18 NES readings of

FIGURE 3

Annual Gallup and CBS and NYT Readings of Macropartisanship, 1976–1988



SRC macropartisanship for 1954–88. In their analysis, the coefficients for consumer sentiment and presidential approval were not statistically significant and, in the case of approval, bore the wrong sign.

This sort of test is almost guaranteed to make the null hypothesis shine. The small number of cases (18), the gap of two years between observations, and the inevitable arbitrariness of their specification decisions all make any conclusions extremely shaky. The sparse data are not up to the task.[5] In any case, our demonstration with the CBS–*Times* series would seem to make the matter moot. Our 1989 analysis showed strong significant effects of approval and consumer sentiment, using the larger Gallup time series, with its intraadministration comparisons. There is every reason to believe that had an SRC-based series of similar length been available for analysis, it would have tracked the Gallup series closely. Therefore, it logically follows that an SRC-based equation would show something close to the same strong approval and sentiment effects as found for the Gallup series.

Still, some empirical support of this point may be appreciated. Accordingly, Table 1 presents the results of two sets of ordinary least squares equations predicting quarterly macropartisanship during the Reagan presidency. In one set, the dependent variable is Gallup readings of macropartisanship; in the other, it is CBS–*Times* readings of macropartisanship, using the SRC wording. Independent variables include Reagan approval and consumer sentiment (each lagged one quarter). To make the test difficult, we also include the lagged value of the macropartisanship measure from the previous quarter.

Levels of statistical significance and explained variance are stronger in the Gallup equation—expected, because the Gallup measure is more accurate due to the larger numbers of cases from Gallup surveys. In theory, the coefficients for approval and consumer sentiment should be similar in the two equations; and they are. The coefficients are higher in the Gallup equations, but the difference is slight. Collectively, the coefficients for approval and consumer sentiment are significant (.024) for the Gallup equation and on the edge of significance (.059) for the CBS–*Times* equation.

MACROPARTISANSHIP AND ELECTION OUTCOMES

Our 1989 analysis offered only a cursory analysis of the electoral relevance of macropartisanship. We reported that macropartisanship (in the third quarter of the election year) accounted for 38% of the variance in congressional seats and 23% of the variance in the congressional vote (1946–86). These are not strong statistical relationships. Abramson and Ostrom report even weaker ones when the time frame is shifted to 1952–88 and weaker ones still when the NES measure of macropartisanship is used. On the other

TABLE 1

Predicting Macropartisanship, 1981–88 (N = 26)

INDEPENDENT VARIABLES	GALLUP MACRO-PARTISANSHIP, *T*	CBS–*TIMES* MACRO-PARTISANSHIP, *T*
Gallup Macropartisanship, $t-1$		
b	.53	—
st. err.	(.14)	
p	[.001]	
CBS/NYT Macropartisanship, $t-1$		
b	—	.62
st. err.		(.14)
p		[.000]
Reagan approval, $t-1$		
b	−.10	−.09
st. err.	(.07)	(.06)
p	[.21]	[.146]
Consumer sentiment, $t-1$		
b	−.12	−.07
st. err.	(.04)	(.04)
p	[.011]	[.105]
Joint significance of Reagan approval, consumer sentiment	.024	.059
Adjusted R^2	.779	.654
Standard error of est.	1.84	1.84

Note: Macropartisanship is measured as percentage Democratic of identifiers. The cases are all Reagan quarters where both Gallup and CBS–*Times* measures of macropartisanship are measured for times *t* and $t-1$. Six quarters are omitted, including quarter 1.

hand, by shifting the time frame from 1948–84 to 1952–88, Abramson and Ostrom boost the R-squareds for the presidential vote from .17 to .45 (Gallup's fourth-quarter macropartisanship) or .29 (NES).

Estimated as a crude bivariate time series relationship, the relationship between macropartisanship and election outcomes is not all that impressive. There are several contributing factors obscuring macropartisanship as an electoral indicator. First, single-survey estimates of macropartisanship contain too much error for precise readings. Second, the statistical relationships between macropartisanship and the vote (or seats) is not necessarily time-invariant. We would not necessarily expect, for instance, that any particular level of macropartisanship would have the same electoral result in both 1950 and 1990. Finally, a serious investigation should control for other causes of the vote, such as the effect of party control of the presidency in midterms.[6]

In investigating this problem, we learned that a revealing way to demonstrate the electoral connection is to relate electoral change to the observed change in macropartisanship between pairs of elections. The frequent (nearly monthly) readings of (Gallup) macropartisanship allow the measurement of partisan trends with considerable precision. For each biennial or quadrennial pair of elections, we measured the macropartisanship trend as the slope of the ordinary least squares equation predicting macropartisanship from time, in months (from January after election 1 to October prior to election 2). We index this variable as annual shift, so that a measure of 1.00, for instance, indicates a 1% Democratic gain in macropartisanship per year.[7]

Table 2 shows the results from a series of equations predicting the biennial change in the congressional (House) vote division from the biennial change in macropartisanship. The coefficients for all 22 interelection periods and for all 11 midterms (but not the periods from midterm to presidential year) are highly significant. The midterm result is particularly impressive. Two variables—the macropartisanship change and the party controlling the presidency—account for a huge 92% of the variance in the presidential-year-to-midterm vote change. The macropartisanship coefficient of .60 suggests that every 1% Democratic partisanship gain over the biennium is worth about a .3% gain in the congressional votes.

Furthermore, the macropartisanship shift over four years is related to the presidential vote change. With a coefficient over 4.0, each 1% change in macropartisanship over four years is associated with a vote change of greater than one percentage point in magnitude. The correlation, a modest .67, is illustrated in Figure 4. Note the one severe outlier, 1972. With this election omitted, the correlation between interelection change in macropartisanship and the interelection vote change soars to .89. (There is an excellent reason to consider dismissing the 1972 outlier. The Democratic 1968–72 gain in party identification was "artificially" generated by the introduction of the 18-year-old vote in 1971.)[8]

The ability of macropartisanship shifts to predict vote changes reinforces the importance of party identification as an electoral variable. If we were to find that no votes are affected when people collectively change their partisanship, it would be a major blow to the causal significance of party identification. Instead, when a small portion of the electorate shifts Democratic or shifts Republican, the change carries electoral consequences.

CONCLUSION

We hope that this response satisfies the doubts about macropartisanship raised by Abramson and Ostrom. Clearly, macropartisanship is important: it moves over a meaningful range, it responds to identifiable political forces, and it affects electoral outcomes. Further, the underlying phenomenon of macropartisanship does not depend on any particular question wording for its substance. What cannot be settled here is the important matter of reconciling macro movement of partisanship with micro theory about individual partisanship. That remains a challenge.

TABLE 2

Vote Change as a Function of Macropartisanship Change

INDEPENDENT VARIABLES	CONGRESSIONAL VOTE CHANGE[a] MIDTERMS	PRESIDENTIAL YEARS	ALL YEARS	PRESIDENTIAL VOTE CHANGE[b] ALL YEARS	EXCEPT 1972
Annualized 2-year change, macropartisanship					
b	.60	.67	.53	—	—
st. err.	(.22)	(.37)	(.21)		
p	[.024]	[.10]	[.022]		
Annualized 4-year change, macropartisanship					
b	—	—	—	4.34	5.45
st. err.				(1.61)	(.98)
p				[.024]	[.000]
Midterm party in power[c]					
b	−3.10	—	−3.17	—	—
st. err.	(.53)		(.81)		
p	[.000]		[.001]		
Constant	.28	1.12	.31	.89	2.91
Adjusted R^2	.920	.186	.633	.386	.767
Standard error of est.	1.29	3.10	2.39	6.90	4.07
Number of cases	11	11	22	11	10

[a]1944–46 to 1986–88.
[b]1944–48 to 1984–88.
[c]1 = Democratic administration at midterm; −1 = Republican administration at midterm; 0 = presidential year.

POSTSCRIPT: MACROPARTISANSHIP IN 1989 AND BEYOND

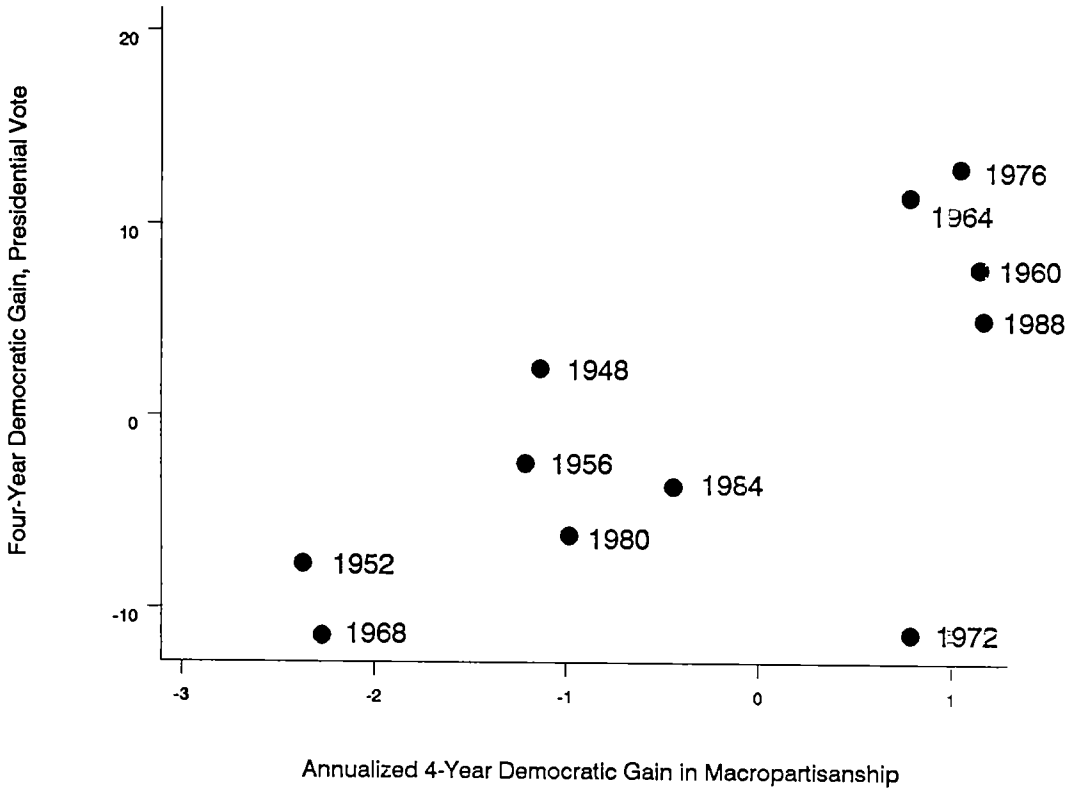
Measurement of macropartisanship beyond 1988 requires a change of data source because Gallup shifted almost entirely from surveys in person to the telephone variety between 1985 and 1988. We can, of course, measure post-1988 macropartisanship from Gallup's telephone interviews; but this requires some adjustment, since (for reasons that are not clear) Gallup's telephone surveys have always generated fewer Democrats than have surveys in person. For instance, whereas Gallup surveys in person and CBS–*Times* telephone surveys regularly showed similar levels of Democratic and Republican identification, Gallup's phone surveys persistently show more Republican identifiers than do CBS–*Times* surveys. Over the 1985–91 period, Gallup showed an average of 2.9% more Republicans among party identifiers than did the CBS–*Times* telephone series. Figure 5 shows the two telephone-based macropartisanship series from 1985 (when Gallup turned heavily to telephone series) through 1991. The figure shows the two series moving roughly in tandem, a few percentage points apart.

The post-1990 series offers a fresh challenge to our findings. During the Desert Storm operation in the Middle East, news reports suggested a surge to the Republican party, as if in gratitude for a successful war effort. Indeed, Gallup's 1991 surveys showed the first persistent Republican partisan pluralities since the 1940s. Was this Republican surge in some significant way an artifact of Gallup's question wording (*In politics today*) as opposed to the SRC and CBS–*Times* wording (*Generally speaking*)?

Figure 6 zooms in for a close-up of the monthly Gallup (telephone) and CBS–*Times* series from August 1990 to November 1991. Reflecting a continued decline in Democratic fortunes, the two series begin with only slim Democratic leads. Following a late 1990 pro-Democratic blip, both series show marked, if temporary, Democratic erosion for 1991. The war effect is most visible as the March 1991 pro-Republican spike in the Gallup series (a burst of about four points in the Republican direction), compared to a single-point movement in the CBS–*Times* series. Much of this Republican gain quickly dissipated.[9] Note also a minor Republican spike in August 1991, visible only in the Gallup series. This is the likely spillover from the failed Soviet coup. (CBS–*Times* polled immediately after the coup attempt; Gallup polled immediately after the restoration). Lest one imagine that the only movement is in the Gallup series, note that the CBS–*Times* series registers more dramatically the pro-Democratic shifts associated with the late 1990 budget fiasco and the late 1991 recession fever. The sudden appearance of persisting Republican pluralities in the Gallup data is not mainly due to lability in the question wording. Instead, it arises from the long-term decline in Democratic mac-

FIGURE 4

Presidential Vote Change by Macropartisanship Change, 1944–48 and 1984–88



ropartisanship (evident in both series) coupled with Gallup's shift to telephone interviewing.

Given the modest Republican surge of the Desert Storm period, the 1989–91 linear trend in Gallup's (telephone) series favored the Republicans at a rate of 1.4 points per year. Using the equations of Table 2, this trend in party identification translates into a projected Republican presidential vote gain of about five percentage points beyond the 54% Republican outcome of 1988. While this early forecast matches the conventional wisdom of 1991, later updates could turn with events of 1992, as macropartisanship responds to shifts in the economy and in presidential approval.

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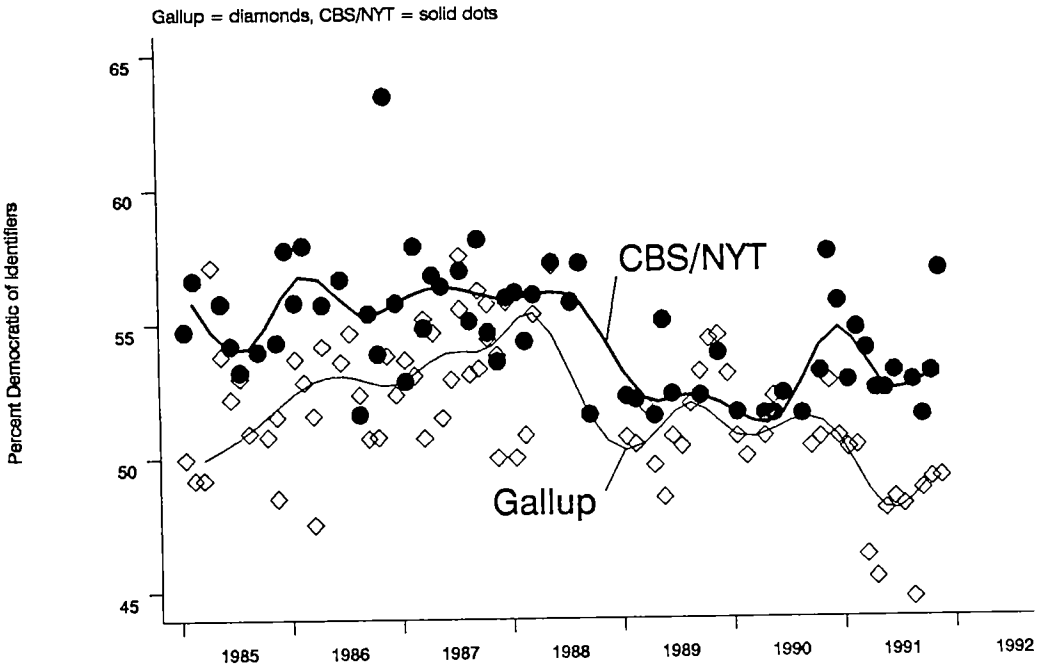
RESPONSE

In their original article on "macropartisanship" Michael MacKuen, Robert Erikson, and James Stimson (1989) made sweeping claims about the importance of their findings, arguing that there was more short-term variation in partisanship than would be expected based upon the traditional view of party identification.[10] They argued that their findings had important implications for the "standard view of party systems and realignment theory" (p. 1139). They suggested that salvaging the traditional view of party systems theory was futile: "As pre-Copernican astronomers preserved the Ptolemaic system, we may simply add medium-term partisan shifts to the longer-run cycles implicit in current party systems theory" (ibid.). Defending the traditional view of partisan change, they seemed to suggest, was as pointless as defending the geocentric view of the universe. At the same time, they argued that the implications of their findings for future theoretical work were unclear. "The mid-range dynamics" of partisan change, they argued, "are of tangible importance" (ibid.): "They yield partisan movements of realignment magnitude (though not realignment duration) that require neither miracles nor catastrophes but instead arise from the routine success and failure of ordinary politics. We argue for a quotidian, as well as a chiliastic, view of political change" (ibid.).

Despite these ambiguous theoretical claims, comparisons with astronomy are relevant in one basic respect. Just as advances in astronomy have depended upon accurate observation, advances in po-

FIGURE 5

Monthly Gallup (Telephone) and CBS–*Times* Readings of Macropartisanship, 1985–1991



Note: Trend lines are computer-generated cubic splines.

litical science also require proper measurement; and measurement lies at the heart of our controversy. In fact, scholars familiar with the research literature on partisan loyalties had good reason to suspect that MacKuen and his colleagues might have chosen a measure of partisan loyalties that enhanced short-term variation. Their analysis was based upon the Gallup party affiliation question, "In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?" Although they reported that they were relying on Gallup data, they never reported the wording of the Gallup item or raised the possibility that their results might be affected by its wording. Yet the research literature had already suggested that the short-term focus of the Gallup item might lead to greater short-term variation in responses than the traditional SRC question widely used in the party identification literature (see Borrelli, Lockerbie, and Niemi 1987; Converse 1976). The Michigan question, by contrast, asks respondents, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?"

Their justification for selecting the Gallup item was the frequency with which it has been asked. One of the main goals of our research note (Abramson and Ostrom, 1991) was to ascertain whether it was possible to compare the Gallup and the Michigan measure in a meaningful way. We reported that almost all of MacKuen, Erikson, and Stimson's findings were supported by our reanalyses when we used Gallup surveys. But we also demonstrated that similar results did not obtain when we used the party identification question employed biennially by the NES since 1952 and annually by the GSS since 1972. Both these surveys employ the standard Michigan question, which is designed to measure long-term attachments to the political parties. When the standard Michigan SRC question was employed, short-term variation in partisan loyalties was smaller, the total variation was reduced, and party loyalties were not significantly related to short-term outcomes in congressional elections. Moreover, our time series analyses demonstrated that short-term variation in the Michigan measure was not greatly affected by short-term evaluations of the economy and the president, whereas short-term evaluations explained substantial variation on the Gallup measure. Throughout our analyses, we demonstrated that the MacKuen, Erikson, and Stimson findings *could* be supported with Gallup data when biennial observations were employed. We therefore urged scholars to be cautious about generalizing from their findings to the research literature employing the Michigan SRC party identification question.

In their response, MacKuen and his colleagues question our actual findings in only a single instance.[11] But they argue that differences between the NES, GSS, and Gallup results do not result from differences in question wording. They base this conclusion mainly upon a comparison of 88 CBS–*Times* telephone polls conducted between 1976 and 1988

with 133 Gallup monthly readings based upon interviews in person. The CBS–*Times* polls, they argue, employ a preface very similar to the standard Michigan SRC question and allow scholars to compare the Gallup and Michigan SRC measure using a large number of observations.

In fact, in the course of preparing our note, we had considered using the CBS–*Times* data but decided that it might be questionable to compare the results of these telephone polls with the results of Gallup polls based upon interviews in person. To employ the CBS–*Times* telephone polls, one needs to weight the results to compensate for the tendency of telephone polls to underrepresent Democrats.[12] Although such weighting is feasible, we were a good deal less confident about using the CBS–*Times* series than we were in using the NES surveys conducted by the University of Michigan SRC and the GSS studies conducted by the University of Chicago National Opinion Research Center.

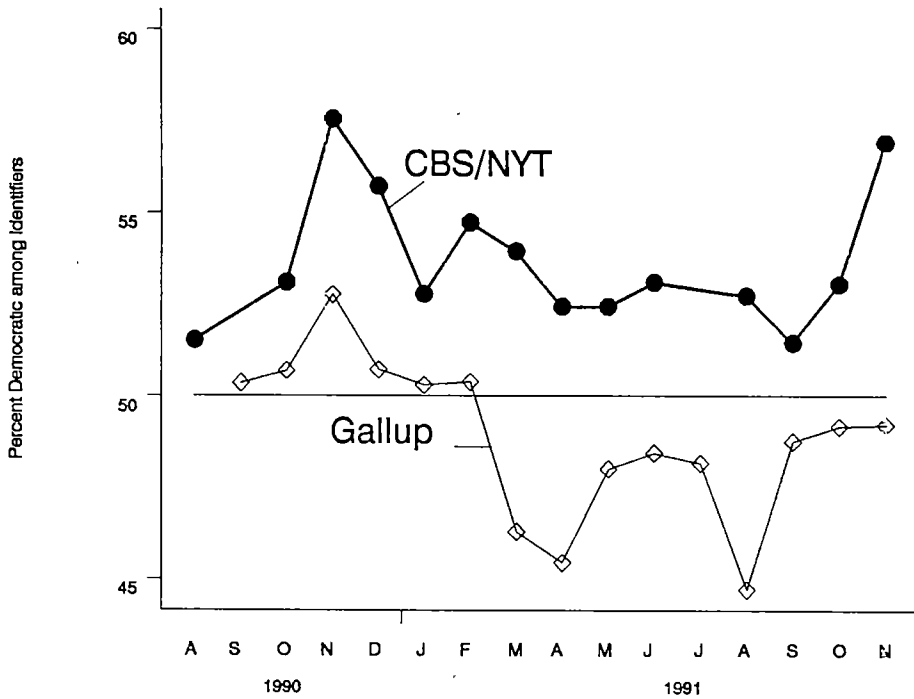
Although we remain skeptical about comparing trends based upon telephone polls with those based upon interviews in person, we are willing to suspend our skepticism if only because MacKuen, Erikson, and Stimson's analyses of the CBS–*Times* polls often support our conclusions. Short-term variation is greater with the Gallup surveys than with the CBS–*Times* surveys, and the total range of variation is greater. Moreover, between 1981 and 1988 changes in Gallup partisanship were significantly related to changes in consumer sentiment, whereas changes in partisanship as measured by the CBS–*Times* polls were not. Even so, Gallup and the CBS–*Times* results between 1976 and 1988 do not differ as much as results using the somewhat longer GSS, or the much longer NES, time spans.

MacKuen and his colleagues assert, "There is every reason to believe that had an SRC-based series of similar length been available for analysis, it would have tracked the Gallup series closely" (p. 478). In fact, there is good reason to question this claim. Our analyses demonstrate that Gallup and NES results correlate more closely during the 1970s and 1980s than they do when surveys from the 1950s and 1960s are included (see Abramson and Ostrom, 1991, Table 2). Differences are sharp if one compares the relationship between the NES trend and the Gallup trend for 1952–74 and 1976–88 (the years covered by the CBS–*Times* surveys). Between 1952 and 1974, our basic NES measure and the Gallup measure correlate at only .61; for the more recent period the correlation is .76. Most of our analyses were based upon a comparison of Gallup and NES results over the full 36-year period, and there is no way to test the claim that SRC and Gallup results would have been similar if a larger number of surveys employing the SRC measure had been available.[13]

However similar Gallup and CBS–*Times* results may have been between 1976 and 1988, there are major discrepancies between Gallup and CBS–*Times* results in more recent years. This may be seen in MacKuen, Erikson, and Stimson's postscript (pp. 480–81), which begins by comparing Gallup *telephone* polls conducted from 1985 through (November) 1991 with CBS–*Times telephone* polls during this period. They maintain that for this seven-year period the

FIGURE 6

Monthly Gallup (Telephone) and CBS–*Times* Readings of Macropartisanship, August 1990–November 1991



trend lines move "roughly in tandem" (p. 480). Although it is not practicable to assign exact values to the large number of data points plotted in their Figure 5, it is readily apparent that variation is greater with the Gallup data. Apart from a single outlier, the CBS–*Times* results go from a low of 51 to a high of 58, a 7-point range. The Gallup results go from 45 to 57.5, a 12.5-point range.

Most of their postscript, however, focuses on the period between August 1990 and November 1991; and the differences between the trends are substantial. Admittedly, the variation is only somewhat greater with the Gallup data (4.75 points according to our reading of their Figure 6) than with the CBS–*Times* surveys (3.82 points); and the total range is only slightly greater (8 vs. 6.5 points). But it would be difficult to argue that these trends move in tandem, since, according to our reading, they correlate at only a modest .51.

The most interesting difference between recent Gallup and the CBS–*Times* results occurs during and after the Gulf War, and several scholars have attributed this difference to question wording. In a recent article, Michael Kagay compared a series of Gallup telephone polls with telephone polls conducted by CBS–*Times*.[14] In interviews conducted between 14 and 24 February, 1991, when the air war in the gulf was underway but before the ground war began, Gallup found that 32% were Democrats and 32% were Republicans. The ground war ended on 27 February. In polls conducted between 28 February and 10 March, Gallup found that only 30% were Democrats, while 37% were Republicans. In polls conducted between 16 and 26 May, Democratic support crept up to 32% while the Republicans dipped to 33%. But CBS–*Times* polls, employing the Michigan SRC question and carried out over a similar period, never showed a Republican advantage. In four polls—conducted in late February, early March, early April, and early May—the Republicans remained constant at 29%. Democratic support dropped from 38% to 32% across these four polls. Gallup polls suggest that at least briefly, the Republicans had become the majority party among the electorate; the CBS–*Times* polls merely suggest that the Republicans came close to parity with the Democrats.

Kagay attributes differences between the Gallup results and the CBS–*Times* results to differences in question wording. He ends by quoting Everett Carll Ladd: "Some polls caught a Republican bounce, while others never showed it. A plausible reason is that some polling organizations' question wording invites people to think about their current predilection, while other polling organizations' wording asks people to think about their political heritage."[15]

Andrew Kohut (1991), who directs the *Times Mirror* polls, reaches similar conclusions. He notes that both the Gallup polls and the *Times Mirror* polls (which use the Gallup question) found Republican pluralities shortly after the Gulf War, whereas the CBS–*Times* and the ABC News–*Washington Post* polls (which use the Michigan question) did not.[16] Kohut argues that these differences are likely to result from differences in question wording:

> There is some evidence that while the small differences in the prefaces are not material during politically stable periods, they take on greater meaning when people are rethinking their partisanship. The preface to the Gallup and the Times Mirror question emphasizes *now*, while the preface to the other formulations is more likely to get people thinking about their traditional allegiances. (p. 22)

Moreover, comparing the annual results of Gallup interviews in person and CBS–*Times* telephone poll results between 1981 and 1990, Kohut argues that "the Gallup measure seems to change more in response to contemporary events than does the CBS/Times question" (ibid.).

Kohut concludes that "what appears clear is that the Gallup and Times Mirror question form magnifies the public's partisan reconsiderations, while the other format tends to anchor respondents somewhat more during periods of change" (1991, 22). "The Gallup approach," he continues, "gives a fuller indication of how much public reconsideration has occurred, while the CBS/Times format provides a more robust measure which may have more analytical utility—when, for example, it is such structural features of party support as social group composition that are to be analyzed" (ibid.).

In spite of our reservations about their conclusions, MacKuen, Erikson, and Stimson's response to our research note provides a valuable service by raising the possibility that question wording differences are not the cause of the substantial variation in partisanship they documented in their original article. Although it is possible that these recent differences between the Gallup and CBS–*Times* results—as well as the differences we found between Gallup, NES, and GSS results over a much longer period—do not result from question wording, we believe the preponderance of the evidence suggests that question wording does make a substantial difference.

A future research strategy to determine why the Gallup results differ from the NES and GSS results may require experimental studies employing both items in the same surveys to determine the extent to which responses are correlated at the individual level. There may well be individuals who consider themselves to be Republicans "as of today" but "generally speaking" usually think of themselves as Democrats. And there may be short-term Democrats who usually think of themselves as Republicans. Without individual-level tests, the question wording explanation cannot be definitively accepted or rejected. We think that it remains the most plausible explanation, however, for the very substantial differences between the Gallup and NES results that obtain when comparisons are made over the nearly four decades for which such comparisons are possible.[17] We would therefore continue to urge scholars to remain cautious about generalizing from the MacKuen, Erikson, and Stimson findings to the research

literature based on the Michigan SRC party identification question.

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Notes

Abramson and Ostrom wish to thank Ada W. Finifter and Brian D. Silver for their suggestions.

1. Individual-level change and macro change are different phenomena. In an important contribution to the understanding of party identification, Green and Palmquist (1990) report impressive statistical evidence of only minimal movement of individual partisanship. This finding must be incorporated into any complete theory of party identification. Although Palmquist and Green portray a certain micro stability to party identification, this stability is not incompatible with our macro findings. By definition, their micro results must be compatible with the net aggregate change found in the NES panel studies from which they were drawn. Green and Palmquist's study reminds us that a small number of individual-level changes (due to what are sometimes called "period effects") can generate impressive-looking net movement on the macro scale of measurement.

2. All Gallup readings are obtained from the Roper Center. Where the sample number of cases was missing in a few surveys, we conservatively set it at one thousand partisans. National Election Study party identification is measured preelection in presidential years and postelection in midterms. We centered presidential-year NES readings in October and midterm NES readings in December of the election year, GSS readings in March.

3. CBS–*Times* readings were obtained as part of the Wright, Erikson, and McIver (1985) study of state politics. We exclude surveys of registered voters only. Given our interest in the national marginals, our inferences are based on the "weighted" rather than "raw" survey data. Since all CBS–*Times* surveys are telephone surveys, they are constant in survey method. Similarly, we include only Gallup surveys based on personal interviews, to maintain a constancy of method.

4. Whether the two measures are about the same can be subjected to statistical evidence, as well as mere "visual" tests. Consider the correlation between the Gallup and the CBS–*Times* (SRC wording) measurements of macropartisanship. For quarterly readings, this correlation is an impressive .917. This correlation is attenuated somewhat, however, by sampling variance in the two surveys. Let us conduct the technical exercise of estimating this sampling variance and adjusting the correlation estimate accordingly.

Assuming simple random sampling, we can estimate the sampling variance of a survey as the product of the two proportions, divided by the sample size. The reliability estimate for each measure of macropartisanship is the estimated ratio of true to total variance, or the ratio of the observed variance minus the mean sampling variance over the observed variance. For the set of 47 quarters with both Gallup and a CBS–*Times* readings of macropartisanship, the estimated reliabilities are .983 for Gallup and .944 for CBS–*Times*. Using these estimates to correct for reliability, the correlation between contemporaneous quarterly Gallup and CBS–*Times* measures of macropartisanship rises to .952. This high correlation is consistent with our contention that the two indices of party identification measure virtually the same thing. Consider, however, that the adjusted correlation falls short of perfection. The missing .048 we concede to differences in "house effects."

We can also examine the corrected correlations between macropartisanship measures in successive quarters. Corrected for reliability, the over-time correlations between macropartisanship at quarter t and quarter $t + 1$ are .965 for the Gallup measure and .945 for the CBS–*Times* measure. Corrected for reliability, the cross-correlations are .918 between the CBS–*Times* measure at time t and the Gallup measure at time $t + 1$ and .950 between the Gallup measure at time t and CBS–*Times* at $t + 1$. Thus, from the perspective of quarter-to-quarter change, macropartisanship is quite stable.

One difference between the two measures of macropartisanship using the two different question wordings is that the Gallup measure does have a larger range. Although this difference is consistent with Abramson and Ostrom's argument, it is slight. For the 47 quarters with joint readings of the Gallup and CBS–*Times* measures, the observed standard deviations are 5.72 for the Gallup measure and 4.98 for the CBS–*Times* measure. Corrected for reliability, the standard deviations fall slightly to 5.67 and 4.84. The ratio of the two standard deviations is .85. This is a modest difference in scale. The suggestion is that when the Gallup index moves 20 points, the CBS–*Times* index moves 17 points.

5. Part of Abramson and Ostrom's argument is that while consumer sentiment and presidential approval cannot predict NES partisanship with only 18 cases, the same 18 quarters are enough to predict Gallup macropartisanship from consumer sentiment and presidential approval. We have tried to replicate their first two equations (Abramson and Ostrom 1991, Table 4), using their reported specifications but were unsuccessful. We can report the reports of our replication using our own specifications. We measure macropartisanship for the party controlling the presidency and add a control for the party in power. This adds a degree of freedom but avoids the complex tangle of Abramson and Ostrom's standardization algorithm. (Standard errors are in parentheses.)

Independent Variable	Gallup	NES
Lagged partisanship	.52 (.23)	.12 (.36)
Consumer sentiment	.19 (.08)	.11 (.06)
Political approval	.10 (.10)	−.01 (.10)
Democratic president	11.08 (5.89)	23.30 (9.25)
R^2	.946	.966

Note that *political approval* represents a measure of *presidential approval* with the contribution of *consumer sentiment* removed. This variable is not significant in either equation. (Nor is "raw" presidential approval, when substituted.) Consumer sentiment is modestly significant in the Gallup equation but misses in the NES equation. The difference in the two consumer sentiment coefficients is trivial, however, considering their standard errors. Based on these two equations, one cannot make a serious case that consumer sentiment affects Gallup macropartisanship but not NES macropartisanship. The problem with making any kind of inference from these 18 cases lies with the limitations of the data.

6. In estimating the slope of macropartisanship on time for interelection intervals, we weighted the Gallup surveys by their number of respondents. Thus, our procedure was to pool the interelection set of Gallup partisans (as if it were a single survey) and regress their party identifications on time. We refrained from including postelection November and December readings in our estimates of partisan trends, because of a possible postelection macropartisanship bounce.

7. The presentation here does not exhaust ways of demonstrating the electoral importance of macropartisanship. Interested readers are referred to Stimson's treatment of predicting national election results from (among other variables) macropartisanship (1991, chap. 5).

8. One solution is to reestimate the 1969–72 measure of macropartisanship change with a dummy variable included for February 1971 (when Gallup began interviewing 18-year-olds) or later. The 18-year-old effect on macropartisanship is about three percentage points. With the revised estimate of partisan change for 1969–72, discounting the effect of the 18-year-old vote, macropartisanship change accounts for 62% of the presidential vote change.

9. Other polls using SRC's *Generally* preface (e.g., ABC–

Washington Post and NBC–*Wall Street Journal*) echoed the CBS–*Times* reading of a slim Democratic lead throughout the Gulf War, with no more than modest movement in the Republican direction. Meanwhile, the *Times Mirror* poll, which, like Gallup, asks respondents about their "politics today," probably showed the largest shift in party identification during the war. The *Times Mirror* readings shifted to Republican pluralities in January and March pollings; but unlike Gallup's, the *Times Mirror* readings bounced back to Democratic pluralities in May and July (Kohut 1991).

10. For the classic statement of the standard view of party identification, see Campbell et al. 1960. For a recent statement of this position, see Miller 1991.

11. MacKuen, Erikson, and Stimson maintain they could not replicate our findings that changes in the Gallup measure were related to changes in consumer sentiment and presidential approval when only 18 observations were employed. They complain that our procedures involve a "complex tangle" of a "standardization algorithm." We find their claim surprising, since we employed their standardization procedures for both consumer sentiment and political approval (1989, 1140–41, nn. 6, 13). Standardization does affect coefficients, but it does not affect t-ratios. When we compute the t-ratios for the Gallup equations in their response (p. 485, n. 5), we find that the t-ratios for both consumer sentiment (2.38) and political approval (1.00) are very close to the t-ratios in Abramson and Ostrom 1991, Table 4 (2.32 and 1.11, respectively). Moreover, both their results and ours reveal the same basic finding. Consumer sentiment is significantly related to Gallup partisanship but not to partisanship as measured by the NES.

12. In a recent interview in *Public Perspective*, Alec Gallup, cochairman of the Gallup Organization, acknowledges that the Gallup telephone polls regularly show a higher percentage of Republicans than Gallup polls based upon interviews in person. Moreover, he argues that these biases probably go beyond differences in socioeconomic status. He explicitly argues that telephone polls should not be compared with interviews in person. Although Gallup now employs both telephone polls and interviews in person to measure party affiliation, he maintains, "We feel that the two sets of data should be kept separate in charting trend lines" (quoted in "Measuring Things" 1991, 23.)

13. Part of MacKuen, Erikson, and Stimson's critique provides additional evidence that changes in Gallup partisanship are related to short-term electoral outcomes. Although we found somewhat weaker relationships between Gallup partisanship and congressional election outcomes than they reported, we were examining change during a somewhat different period. In all of our analyses (see Abramson and Ostrom 1991, Table 3), changes in Gallup partisanship were significantly related to changes in both congressional and presidential election outcomes. Since we never questioned MacKuen, Erikson, and Stimson's conclusion that Gallup partisanship is related to electoral outcomes, we will not discuss this portion of their critique.

14. Michael R. Kagay, "Rise in GOP Loyalty Now Ebbing, Poll Finds," *New York Times*, 2 June, 1991. For further details about these Gallup surveys, see Larry Hugick, "Post-War Surge in GOP Affiliation Already Slipping," Gallup Poll News Service, 10 April, 1991.

15. Quoted in Kagay, "Rise in GOP Loyalty Now Ebbing."

16. MacKuen, Erikson, and Stimson note that the *Times Mirror* polls probably showed the largest pro-Republican shift during the Gulf War but offer no explanation for their volatility.

17. With the addition of the 1990 NES survey, comparisons between Gallup and NES results are now possible over a 38-year period.

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NOTES FROM THE MANAGING EDITOR

Subfields, Manuscripts and Representation in the *APSR*

As is the lot of new managing editors, I have received a number of letters from scholars concerned that their subfield of political science is underrepresented in the pages of the *APSR*. I want to share my response to such concerns with all of our readers.

I recognize and emphasize that the official scholarly journal of the American Political Science Association has a responsibility to publish outstanding scholarship in all the subfields of political science. If you are concerned that work in your field is not appearing regularly in the *Review*, I urge you to help us. You can do so in three ways:

1. Encourage scholars in your field to submit their manuscripts to the *APSR* when they feel they have produced an outstanding piece of scholarship. We cannot publish what we do not receive. The assumption that *APSR* is unsympathetic to a particular subfield will become a self-fulfilling prophecy if the best work is sent elsewhere. As the various studies reported in *PS* by previous Managing Editors have repeatedly demonstrated, articles appear in the *APSR* roughly in proportion to the submissions from the subfield. It is not true that reviewers in various subfields exhibit strongly differential biases for or against their peers.
2. Encourage scholars in your field to follow the rules for submission, reprinted in each issue and discussed at more length in the *APSA Style Manual*, available from the Association. Follow the *APSA* format and style guidelines. Write a brief and clear abstract, which assists our evaluation process. Be particularly aware of our page limits. We can only publish about one manuscript from every ten submitted. Under these conditions we are unlikely to publish manuscripts that are substantially beyond our limit of 30 double-spaced pages. We do not even evaluate manuscripts whose total contents exceed 50 double-spaced pages, counting figures and tables, as well as double spaced notes, references and appendices. These limitations do create a bias in publication against work that cannot adequately be presented at article length. Most scholarly research, however, can be compressed or subdivided to fit journal constraints. No one likes to do this, but it can be done. Most often, tightening the presentation improves it.
3. The determination of quality is made primarily through the peer review process. Help us to understand your field and help us to find the appropriate reviewers for it. We are glad to be educated. We are always looking for skilled reviewers. Concerned about bias, we cannot follow the proposals of authors who send a selected list of referees with their manuscripts. But we are attempting to compile and check with members of our Editorial Board lists of reviewers appropriate for each subfield. Educate us before you submit a particular manuscript.

If your field does not publish frequently in the *APSR*, it might be especially useful for you to write me a brief letter, defining your field, outlining briefly its major concepts and theories, offering examples of outstanding recent research that can serve as benchmarks, and suggesting the names and addresses of peer reviewers. In the case of proposed peer reviewers, which is our most essential need, it is helpful to know about both their technical skills and their substantive interests. Having the best reviewers is essential for selecting the best manuscripts and for providing useful critiques and advice to authors whose manuscripts are not published.

I want to work with you to see that the *APSR* publishes the outstanding scholarly research in all fields of political science.

Transition to New Size and Press

We have now completed a second issue with the larger page size and the new press. Such a complex double transition involves overcoming many difficulties. I want to thank Cathy Rudder, Rob Hauck and, especially, Sheilah Mann of ASPA for their roles in supporting the transition and helping us work through the many details of the new arrangements. I also want to thank Assistant Editor Linda Lindenfelser for her untiring efforts and Tom MacDaniel of Byrd Press for his patient and cheerful assistance to the new editorial team.

Paper Error in March Issue

Due to a production error, the March 1992 issue of the *Review* was printed on paper with a glossy, rather than matte, finish. I sincerely regret the difficulty in reading the text printed on glossy paper.

FORTHCOMING IN SEPTEMBER, 1992

The following articles, research notes, and controversies have been tentatively scheduled for publication in the September, 1992, issue.

Jeffrey Berejikian. "Revolutionary Collective Action and the Agent-Structure Problem."

Bruce Bueno de Mesquita, Randolph M. Siverson, and Gary Woller. "War and the Fate of Regimes."

Alexander M. Hicks and Duane M. Swank. "Politics, Institutions, and Welfare Spending in Industrialized Democracies, 1960–1982."

John Huber. "The Role of Restrictive Legislative Institutions in France."

Michael B. MacKuen, Robert S. Erikson, James A. Stimson. "Peasants or Bankers? The American Electorate and the U.S. Economy."

Joshua Mitchell. "Protestant Thought and Republican Spirit: Luther's Peculiar Enchantment of the World."

Mark Alex Peterson. "The Presidency and Organized Interests: White House Patterns of Interest Group Liaison."

John T. Scott. "The Theodicy of the Second Discourse: The 'Pure State of Nature' and Rousseau's Political Thought."

Andrew Stark. "Corporate Electoral Activity, Constitutional Discourse, and Conceptions of the Individual."

Dana Villa. "Postmodernism and the Public Space."

Jan E. Leighley and Jonathan Nagler. "Socioeconomic Bias in Turnout 1964–1988: The Voters Remain the Same." A research note.

L. Marvin Overby, Beth Henschen, Michael Walsh and Julie Strauss. "Courting Constituents: An Analysis of the Senate Confirmation Vote of Justice Clarence Thomas." A research note.

Mark Schneider and Paul Teske. "Toward a Theory of the Political Entrepreneur: Evidence from Local Government." A research note.

Michael D. Ward and David R. Davis. "Sizing Up the Peace Dividend: Economic Growth and Military Spending in the United States, 1948–1996." A research note.

Richard D. Anderson; Margaret G. Herman and Charles Herman. "A Process Model vs. a Domestic Bargaining Model of Self-Frustrating Foreign Policy Decisions: Soviet Arms for Egypt in 1973." A controversy.

ERRATA

"The Political Manipulation of Macroeconomic Policy" by John T. Williams, September 1990, pp. 767–795. On page 790, column 1, sentence beginning on line 12, the reference to Tufte (1978) should be omitted.

In Bart Schultz's review (March, 1992, p. 215) of Chandran Kukathas and Philip Pettit's *Rawls: "A Theory of Justice" and Its Critics*, references to "The Theory of Justice" should read "A Theory of Justice."

In Edella Schlager's review (March, 1992, p. 231) of Donald Chisholm's book, the title given as *Structures in Multiorganizational Systems* should read *Coordination Without Hierarchy: Informal Structures in Multiorganizational Systems.*

BOOK REVIEW ESSAY

THE ROLE OF SEX AND GENDER IN COMPARATIVE POLITICAL ANALYSIS: INDIVIDUALS, INSTITUTIONS, AND REGIMES

BARBARA J. NELSON *University of Minnesota*

Engendering Democracy in Brazil: Women's Movements in Transition Politics. By Sonia E. Alvarez. Princeton: Princeton University Press, 1990. 304p. $45.00 cloth, $14.95 paper.

Women in Taiwan Politics: Overcoming Barriers to Women's Participation in a Modernizing Society. By Chou Bih-er, Cal Clark, and Janet Clark. Boulder: Lynne Rienner, 1990. 207p. $30.00.

Bringing Women In: Women's Issues in International Development Programs. By Nüket Kardam. Boulder: Lynne Rienner, 1991. 137p. $26.50.

Persistent Inequalities: Women and World Development. Edited by Irene Tinker. New York: Oxford University Press, 1990. 302p. $32.50 cloth, $15.95 paper.

Playing the State: Australian Feminist Interventions. Edited by Sophie Watson. New York: Verso, 1990. 243p. $59.95 cloth, $17.95 paper.

Women and Social Protest. Edited by Guida West and Rhoda Lois Blumberg. New York: Oxford University Press, 1990. 406p. $49.95 cloth, $18.95 paper.

In 1955 Maurice Duverger published *The Political Role of Women*, the first behavioralist cross-national comparison of women's electoral participation to use both election and survey data. Within the limits of the methodological individualism that shaped Duverger's research, his work set an early standard for the sensitive analysis of women's electoral activities. But Duverger's research was also severely hampered by its inability to theorize the relations between women and men and its silences about regime and economic factors. As such, his research foreshadowed the enduring limitations this approach bequeathed to the cross-national study of women's political participation. The strengths and weaknesses of Duverger's research are thus important points of departure for evaluating six recent books on women, gender, and the political economy.

Duverger analyzed women's patterns of voting, standing for election, serving in legislatures, and party and pressure group activity in Norway, France, the Federal Republic of Germany, and Yugoslavia in the period from the early campaigns to win female suffrage through the decade following World War II. In general, he found that across these four countries women voted somewhat less frequently than men (the difference narrowing the longer women had the franchise) and were slightly more conservative than men (findings sensitive to the sex and age composition of the population) and that married couples tended to vote similarly (whether men exercised "marital authority" [p. 122] over political choices or these decisions were determined together or individually). It is a credit to Duverger's understanding of the limits of cross-sectional analysis that he located each of his specific findings in the long-term social processes that shape it. Because these social processes have themselves changed, Duverger's caveats have proved more durable than his original findings.

Important as it was, *The Political Role of Women* had profound limitations as well. Its most serious difficulty was that although Duverger groped for a theory of the political or social relationships between women and men, he could not articulate one. Predating feminist political theorizing about sex and gender and avoiding Marxist interpretations of relations between the sexes, Duverger retreated uneasily to intrapsychic explanations, including men's warlike nature and the *mentalités* developed by women as a subordinate group. Similarly, he avoided discussions of regime and economic factors, perhaps because the research for *The Political Role of Women* was done under the aegis of the United Nations. For example, he describes Yugoslavia as having one-party elections but not as being an emerging state-socialist country riven with nationalist problems. Likewise, he mentions World War I far more often than World War II! From this volume the reader would never know that eight years before Duverger began his research, Europe was engulfed in a horrific war where more than 25 million Europeans died and a large proportion of the industrial capacity of the region was destroyed. The whole idea of European countries' reestablishing their political centers of gravity while simultaneously refashioning their economies is missing from Duverger's analysis. In this social context, women's electoral participation is more interesting and complex than Duverger's analysis conveys.

Feminist political theorizing on the distinctions between sex and gender responds to the theoretical emptiness that dogged the behavioralist approach. *Sex* focuses on the biological differences and similarities not just between women and men but also among groups of women or men; *gender* focuses on the social organization and meaning of these similarities and differences. This theorizing permits scholars to ask and attempt to answer the two questions Duverger did not have the vocabulary to articulate: How do women in their female bodies fit into the polity when gender itself is a complex constituent of the polity? And how can these patterns be compared across and within nations, institutions, social movements, and groups of people—indeed, across all the structures and processes of politics, broadly defined?

Each of these six newly published books on women, gender, and the political economy answers these questions differently. The four that emphasize politics (Alvarez's study of Brazilian women's move-

ments in the transition to democracy, Watson's edited volume on Australian feminists' trying to make the state and its programs amenable to feminist policies and perspectives, West and Blumberg's wide-ranging anthology on women's participation in a variety of social movements, and Chou and the Clarks' volume on women in the Taiwanese legislatures) succeed most fully when they link individuals, the state, and society. The two books that emphasize economics (Tinker's compendium on the persistence of a special form of female poverty in the face of decades of development programs and Kardam's study of how three international development agencies have approached adding women and development to their agendas) succeed most fully when they link macroeconomic patterns of economic development, political processes, and the labor that women perform. In both cases, these links are crucial because they call into question theories of politics, participation, the economy, and labor that hold men's place to be both normative and universal. Without these links, authors and editors fail to provide the stereoscopic vision that attention to sex and gender can give to political analysis.

Sonia Alvarez' *Engendering Democracy in Brazil*, which analyzes the development and political consequences of women's movements during the Brazilian transition to democracy, provides a richly detailed and theoretically important volume on gendered patterns of interest intermediation during regime transformation. Her work demonstrates how the fraternal structure of democracy responds to women's organizing when civil society is not subservient to formal political society. Focusing on greater São Paulo, Alvarez recounts the political histories of two types of women's groups: *feminine* groups that seek to advance practical gender interests to solve immediately perceived needs and *feminist* groups that seek to advance strategic gender interests by advocating more fundamental changes in society.

One of the most contentious problems facing both types of women's groups during the movement from military rule to elite-administered democracy in Brazil was that of determining the proper relationship between these groups, political parties, and the state. In the beginning, when electoral politics was part of the opposition to the authoritarian regime, there was relatively little debate within women's groups over the propriety of electoral participation. But as the situation stabilized, and a center–Right coalition became powerful both nationally and in the state of São Paulo, Paulistas were deeply and bitterly divided over whether to keep their women's organizations autonomous or work with parties or the state. The parties both courted and wanted to coopt these groups. The parties of the Left, with their Marxist consideration of sex as a "secondary contradiction" and their masculinist organizational practices, also offered the programmatic reforms both feminine and feminist groups most sought. This dilemma became especially difficult as democracy regularized and political institutions swamped civil institutions as the vehicles of political participation and problem solving.

The solution for some individuals and groups was the *dupla militança* (double militancy) of party work and activism in autonomous women's groups. But even those who argued that women had to make a nonauthoritarian state in order to live in a nonauthoritarian regime had to come to terms with the often unpalatable compromises of party politics. The experiences of the women's councils and departments established with the return to democracy made the dilemmas of these compromises real. Party leaders underfunded these units and filled them with party women, who, though often feminists, were not representative of the range of autonomous women's groups. Nonetheless, with strong leadership such units occasionally succeeded in promoting women's strategic gender interests—as when Eva Blay, an alternate senator and former leader of the State Council on the Feminine Condition, led the fight against the government's attempt to implement an internationally funded family planning program in a way that would control, rather than expand, women's contraceptive choices.

Like the Brazilian activists, Australian feminists experienced a similar conflict over the propriety of participating in the state apparatus. Indeed, this conflict was central to women's movements in most democratic countries from the late 1970s through the 1980s. As Sophie Watson, editor of *Playing the State*, says in her introduction, part of this conflict originated in orthodox and New Left ideologies from which socialist feminists drew their initial theoretical sustenance. Watson nicely summarizes the transformation of views about the state among feminists in Australia and other English-speaking countries within the commonwealth. This theorizing moved away from the reified state of classical leftist analysis to a recognition that the relationship between women and the state was complex and historically varying.

Watson does not recount the history of the vitriolic debates among Australian feminists over whether feminists should participate in social change through the state, a participation that was often seen to undermine autonomous organizing and to lead to "mere reformism." At the theoretical level, Judith Allen engages this question in the essay, "Does Feminism Need a Theory of 'the State'?" But Allen is unconvincing analytically when she says that the state is not an important category of feminist analysis. At best, her argument should be read as a call for better theorizing about the state—theorizing not done in the service of adjectival feminisms.

The rest of Watson's volume provides examples of how Australian feminists have "played the state." Nowhere in industrialized democracies have feminists succeeded as well at the "artful game" of "turning the state back on itself" (p. 191). Recognizing that there was little tradition of nongovernmental, nonparty solutions to problems in Australia, a group of well-educated, middle-class, Labour party feminists entered state and national bureaucracies in

the 1970s and set about creating, with the support of the Labour government, permanent governmental institutions to represent women's interests. At the national level the *femocrats* were remarkably successful. They used a dual strategy of creating a bureau committed to improving women's status within the office of the prime minister, while also establishing planning units (of varying seriousness and duration) within functional ministries.

The femocrats also helped to fund such feminist projects as women's refuges (battered women shelters). In "Interpretation of a Frontline State" Ludo McFerren, like Alvarez, documents the struggles of feminists to negotiate the forms of the relationship between women and the state. For example, because the national government initiated and funded a large-scale refuge program from 1975 to 1977, feminists in Canberra encouraged grassroots feminists to form a national organization to represent their interests. Women in the refuge movement resisted, however, and created a confederation instead. This confederation promoted the independence of the refuges but, without a national lobbying arm, also unwittingly created the situation where refuges became more dependent on state governments, rather than the national government.

While Australian feminists used the bureaucratic state quite effectively, gains in electing more women, regardless of ideology, and more feminists, regardless of sex, were less impressive. In this, the Australian case parallelled the experience of women around the world. Despite its general title, *Women in Taiwan Politics* is about the experiences of women candidates and legislators in a country whose yearly gross national product per capita grew from one hundred dollars in 1950 to seven thousand dollars in 1989. Like Duverger, these authors are successful at looking at women within Taiwanese local and national legislatures. They show that women, mostly from elite backgrounds, overcome cultural proscriptions against women's political participation by mobilizing resources from supportive natal and marital families. As legislators, these women are at least as good as men at constituent service, which includes promoting economic development projects, but are less active than men in introducing legislation.

This book is particularly interesting because Taiwan has a complex system of reserved seating for women in all legislatures. Two electoral systems are used to reserve seats for women. In the National Assembly reserved seats for women are elected by the membership of specified women's organizations that support the government. In all other legislatures the reserved seating process works in this manner: if women candidates do not win 10% of the seats in each multimember district, the female candidate(s) with the highest number of votes replaces the winning male candidate(s) with the lowest number of votes until women candidates comprise 10% of the members in that district. In practice, women usually win enough seats on their own to make the allocation of seats to women through the reserve process unnecessary. From comparisons with women's lack of success in winning executive offices, Chou and the Clarks argue convincingly that the reserved system has set both a floor and a ceiling on women's legislative success.

But like Duverger, the authors of *Women in Taiwan Politics* do not sufficiently integrate theoretical issues of gendered regime structures into their work. Taiwan is characterized by a military and bureaucratic authoritarianism that has only recently begun to change and whose effects on what must be considered at best quasi-democratic legislatures need further attention. One of the most important questions that arises from their work is the place of reserved seating systems in overcoming the gender inequalities structured into legislatures. Chou and the Clarks describe Taiwan's reserved seating system as "possibly unique" (p. 5) or "unique" (p. 81). In fact, Bangladesh, Egypt, and Pakistan have, or recently had, reserved seating for women; and India, New Zealand, and Zimbabwe have, or recently had, reserved seating on the basis of caste, ethnicity, or race. Taken together, these practices pose interesting questions about the nature of representative democracy, especially because most research on group representation in legislatures has focused on the cooptation of such groups in rubber-stamp parliaments. Representation based on one-person-one-vote is the norm in freely elected legislatures. Many bicameral legislatures also privilege regions (as a proxy for, e.g., language, ethnicity, religion), providing equal representation for regions of vastly differing populations as a partial resolution of historic regional conflicts. Seen in this light, reserved seating for women, castes, or races raises the same kinds of democratic questions as reserved seating for say, Wyoming, in the U.S. Senate. The question that needs to be asked in future research is, Why do legislative arrangements privilege some social cleavages over others, and with what results?

Of course, reserved seating is just one of the mechanisms used to promote greater physical and ideological representation in legislatures. The much more common approach is ticket balancing. Carol Schmid's chapter, "Women in the West German Green Party" in West and Blumberg's collection *Women and Social Protest* provides a detailed discussion of how the Greens, who believed both in virtual representation and compensatory democracy, developed the policy of equal female and male representation on party lists. This practice transformed the nature of gender intermediation in political parties all over Europe by both responding to, and increasing competition for, women's votes.

An important aspect of the West and Blumberg volume is that the chapters investigate women's participation in a large number of social movements, using a wide range of methodologies. The editors divide the book into four parts dealing with women's participation in different types of social movements: grassroots movements for economic survival; racial, ethnic, and nationalist struggles; social-nurturing and

humanistic protests; and women's rights activism. This typology, based on the different configurations of the nexus between women's identities and power politics, begins an important endeavor to conceptualize social movements by goal, location, and strategy, as well as opportunity structures and resources. The contributions also use an impressive variety of methods. As sociologists, the editors have more easily avoided the overemphasis on the nation–state that characterizes comparative politics as a field in political science. But their success derives from more than just emphasizing a wide variety of units of analysis. The editors have assembled a series of essays that use individual- and multiple-case studies, collective biography, historical analysis, quasi-experimental designs with extensive statistical analyses, and spatial analysis to examine women's and men's forms of social protest.

None of the articles employs economic theories or statistical techniques typical of economic analysis, however. Conventional economic theories and techniques are out of favor in social movement studies for two reasons. First, the theories that linked economic conditions directly to unrest were not supported by evidence; the relationship between economic conditions and protest is mediated by political, cultural, institutional, and individual considerations in complex ways. Second, the concepts of "unrest" and "protest" have themselves been questioned as too universal, too linked to theories suggesting that mass action is unusual, and (for women) too connected to men's access to public space and economic resources.

Irene Tinker's excellent anthology *Persistent Inequalities* looks not so much at protest as to the persistence of specifically gendered types of poverty in the less-industrialized world. As such, the volume contributes to the information necessary to build the more accurate models of political action that have replaced purely economistic ones. The collection is a festschrift in honor of the twentieth anniversary of the publication of Ester Boserup's *Women's Role in Economic Development* (1970). Like Duverger's work in political participation, Boserup provided the classic text that introduced the study of women in economic development to the development field. Her research argued that in systems of shifting cultivation, which she defined as *female farming*, women's economic input was at least as important as men's. With the advent of plow cultivation, which she defined as *male farming*, women's economic contribution was marginalized by male monopolies over the technologies of production.

One of the great values of Boserup's work was that it provided an analysis of the importance of women's contribution to economic development in the theoretical language of economic efficiency used by neoclassical economists. In "Gender and Justice in Economic Development," Jane Jaquette perceptively analyzes the justice claims in various development theories. She writes that "Boserup's argument speaks directly to this market conception of a merit claim for women. [Boserup asserts that] in the historical past, women were not only equal in status [with men], they were equally productive. . . . It is differential access to technology that has deprived women of their claim to equal productivity" (p. 61). From this point of view, the failure to include women in development schemes robbed less-developed nations of potential increases in economic growth. Boserup's argument about the economic inefficiency of overlooking women's contributions to economic development—more than arguments made from claims based on women's equality with men or their special needs—motivated the inclusion of women in development programs funded by international agencies and national planning commissions.

Boserup's work was also criticized on similar grounds as was Duverger's. Her theories were silent about forms of male domination that could not be explained by the economic impact of differential access to agricultural technology. She failed to develop a theory of how patriarchal social and political relations changed and modernized in ways not derivative of economic relations. Similarly, as Vina Mazumdar and Kumud Sharma discuss in "Sexual Division of Labor and the Subordination of Women," Boserup did not situate her analysis of male and female agricultural systems in a world economic system constituted by an elaborate sex/gender division of labor in every sphere of production.

Nüket Kardam's *Bringing Women In* examines how three development agencies (the UN Development Program, the World Bank, and the Ford Foundation) responded to the opportunities, and met the challenges, of adding women to economic development programs. Although excessively reportlike in its presentation, this book provides an interesting examination of the role of institutional culture in issue adoption. Kardam found that the Ford Foundation, with its mission to promote innovative, knowledge-based solutions to problems and its decentralized decision-making system, was most responsive to the demands for inclusion of women-sensitive programs within its development programming. The Ford Foundation also had the least money to spend and the fewest outside actors to satisfy. The World Bank, with enormous resources but the need to demonstrate economic efficiency, made a moderate level of commitment to women and development projects. The UN Development Program, having to coordinate funders, projects, and country officials, had the lowest level of commitment.

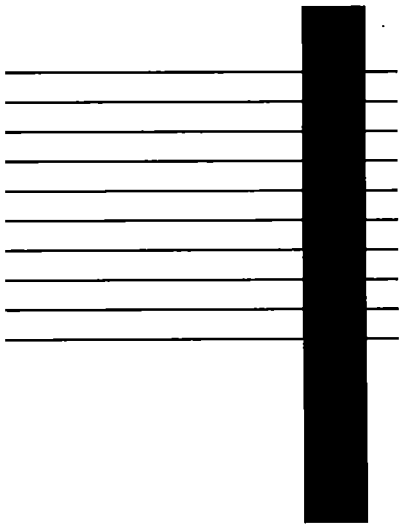
Kardam's research would have benefited from drawing wider intellectual boundaries. Her focus on the organizations' relations in the international development regime made her miss the important feminist epistemological questions her study raises. For example, Kardam gives a measured, but ultimately devastating, description of the intellectual culture of the World Bank, where women's equity issues are unwelcome because "they are not easily quantifiable and introduce many unknowns" (p. 72). But she does not draw the parallels between the World Bank's reliance on one type of analysis and stereotyped

views of Third World women as wedded to old-fashioned methods of problem solving. Who is modern, and who is traditional, in this scenario? Without explicit theorizing about the gendered construction of knowledge, *Kardam* cannot answer these questions.

Read as a conversation, these books suggest three directions for future research. First is the need to pay more attention to gender as a social and political cleavage. In particular, they suggest that it is crucial to examine how gender issues are mapped onto systems of interest intermediation. Second is the need to do more comparative research on the political activism of groups of both differently situated women within countries and similarly situated women across countries. Together, the similarities and the differences among groups of women illuminate the force of social structures and the power of individual agency in defining women's political engagement. Third is the need to give greater attention to the theoretical and empirical issues of sex and gender in the system-level studies of comparative politics, especially in the analysis of regime characteristics and regime change. In other words, the study of sex and gender in comparative political analysis, like the study of other topics within the field, needs to include a focus on individuals, institutions, and regimes. These six books contribute significantly to the foundations of this research.

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Pragmatic Liberalism. By Charles W. Anderson. Chicago: University of Chicago Press, 1990. 220p. $27.50.
Political Criticism. By Ian Shapiro. Berkeley: University of California Press, 1990. 338p. $35.00.

These two books are in important respects similar in intent, even though each employs different argumentative strategies and arrives at quite different conclusions. They are alike in that both are committed to criticizing the abstractly formal character of much of modern political theorizing (as found in, e.g., Rawls's fiction of an original position or Habermas' ideal speech situation). Each, moreover, proposes a less hypothetical and ostensibly more muscular alternative—Anderson opting for *pragmatic liberalism*, Shapiro for *critical naturalism*.

On Shapiro's telling, a good deal of contemporary theorizing amounts to a "turn away from Neo-Kantian political theory" as practiced preeminently by John Rawls. That form of theorizing gives pride of place to abstract, ahistorical, and dissociated individuals who come somehow to subscribe to universal principles under which each can pursue a personal vision of the good life. Shapiro's *Political Criticism* consists, in the main, of explications of the views of five critics of "Kantian" theorizing—and a motley crew they are. If politics makes strange bedfellows, anti-Kantian political theorizing makes even stranger ones. On his procrustean bed Shapiro places Richard Rorty, Michael Walzer, Allan Bloom, Alasdair MacIntyre, and J. G. A. Pocock (chaps. 2–6, respectively). What connects and makes kinsmen of this disparate group, Shapiro asserts without argument, is their shared antipathy toward neo-Kantian formalism. This seems to me to be the thinnest of threads with which to connect these thinkers, nor do I think that Shapiro succeeds in making his case in those terms. Three (Walzer, Rorty, and MacIntyre) fit the bed fairly well; but Pocock does not, and Bloom belongs on an altogether different bed. *Contra* Shapiro, Pocock, for example, recovered or reconstructed an "Atlantic republican tradition" not in order to provide a workable communitarian alternative to liberalism but to counter certain historiographical claims about the hegemony of liberal ideas in England and colonial America. Pocock's targets are, accordingly, historians like the late Louis Hartz and C. B. Macpherson, and not philosophers like Rawls (still less, Kant).

Such objections aside, however, Shapiro has written an interesting and important book. Its interest and importance lies in its very clear expositions and cogent criticisms of the views of these five theorists and the brief sketch (which he promises to fill in at a later date) of an Aristotelian critical naturalist alternative to neo-Kantian and antifoundationalist approaches alike.

If Shapiro's hero is Aristotle, Charles Anderson's is John Dewey, along with other exemplars in the pragmatic liberal tradition. Anderson is highly critical of the modern turn toward such abstract, ahistorical, and hypothetical constructs as the competitive free market (à la Milton Friedman and Robert Nozick), the social contract (in its Lockean and Rawlsian versions), and other simplisms. If liberalism is to be saved from itself (e.g., from the excesses of simplification and abstraction favored by too many of its modern defenders) it must take a practical, or pragmatic, turn that recognizes the changing historical contexts and varying kinds of social and institutional complexity that characterize modern societies. In the discipline of practical political reason, principles are not abstract and universal but, instead, derived from a workable vision of the fair and efficient functioning of disparate social practices and institutions. This vision is not an a priori philosophical construct but is instead arrived at pragmatically, through debate and dialogue about the place and purpose of particular institutions in their larger social setting. Anderson's appropriation of Dewey, Veblen, and others is richly suggestive, and points to political possibilities and nuances too often overlooked in arid confrontations between defenders of liberalism and advocates of communitarianism. Far from being dichotomous entities, these are, in Anderson's view, compatible and complementary strands in the pragmatic liberal tradition: *communitarian liberalism* need not be an oxymoron.

Even so, Anderson's analyses and criticisms are arguably directed less at academic political theorists than at planners and policymakers (the latter too often taking their cues from the former, with actually or potentially disastrous results, e.g., deregulation, privatization, and other theory-driven policies). Anderson warns not against the dangers of theorizing per se but against the Sirens of abstraction and oversimplification. Plain in its prose and powerful in its arguments, Anderson's *Pragmatic Liberalism* is a book to reckon with.

University of Minnesota TERENCE BALL

The Communicative Ethics Controversy. Edited by Fred Dallmayr and Seyla Benhabib. Cambridge: Massachusetts Institute of Technology Press, 1991. 378p. $37.50 cloth, $16.95 paper.
Critical Theory in Political Practice. By Stephen T. Leonard. Princeton: Princeton University Press, 1990. 295p. $35.00.

Although these books relate most specifically to the literature of what is termed critical theory, they raise issues exemplifying the ambiguous situation of political theory more generally conceived and, indeed, the discipline of political science. Political science and political theory have, historically, had their roots in critical engagée enterprises but now find themselves institutionalized and professionalized with little clear sense of their relationship to political life.

One of the great ironies in the history of social thought is how Marx's critical theory was transformed into an apology for dogmatic orthodoxy. Stephan Leonard suggests that no less an irony (although played out on a smaller stage) is how the modernist critical theory of the Frankfurt school (aimed at salvaging the emancipatory intentions of Marx and applying them to the conditions of the twentieth century, as well as postmodernist, critical theory (associated, especially, with Foucault), has become little more than an idea and a "form of academic, intellectual introspection."

A theory defined by the goal of bridging the gap between theory and practice and arguably representing "the most perspicacious extant understanding of what social inquiry is and must be" has, in its various forms, become a quintessential example of "shunting practical questions to a secondary status." It has failed to realize its own aims, make good on its commitments, and even to make clear how it could be related to concrete political practices. Leonard offers both an explanation and a solution.

The explanation is that critical theory became absorbed with a "metacritique of modernity" and "metatheoretical preoccupations" that subverted its practical intentions. Although it emphasized, for critical purposes, the "historicity and contextuality of our knowledge," nevertheless, from Marx through Habermas, it either paradoxically sought some version of universal reason (or normative universalism) and truth to ground its claims and justify its critical posture or else fell into a postmodernist skepticism like Foucault's, abandoning the very idea of reason and thereby undercutting its own critical project. It is, in the end, the metatheoretical turn and the tyranny of universalism that is politically stunting for critical theorists and explains the "gap between the promise and the realization of their own ideals."

It might well be argued that this fate of critical theory is represented in the extensive controversy that has surrounded Habermas' *Theory of Communicative Action* and the attempt to find in the structure and logic of speech a kind of immanent transcendentalism. This controversy has now, on both sides of the Atlantic, become an important part of a more general meta-ethical debate about the possibility of a postposivitivist cognitive ethics. Since much of this discussion has taken place in Germany, Fred Dallmayr and Seyla Benhabib have compiled nine essays by German-speaking authors, including Habermas and Karl-Otto Apel (the "foremost spokesmen of communicative ethics"), and representative defenses and critical considerations of their positions.

Dallmayr's concise but comprehensive introduction does a splendid job of clarifing the issues in the controversy, indicating its place in the larger contemporary debate between ethical rationalism and skepticism, distinguishing the more Kantian foundationalist approach of Apel from Habermas' less transcendental formulation, and explicating the positions and arguments of the respondents. Behabib's afterword explores more fully the "provocative interaction" between the Anglo–American and Continental traditions in ethical theory. The focus is especially on the criticisms of universalism in discourse ethics that might be—or have been—urged by various neo-Aristotelian and neo-Hegelian theorists, including the hermeneutical approach of Gadamer, but with a view toward reconciling some of the characteristic oppositions, such as that between universalism and historicity.

Following through the complexities of this metatheoretical debate indicates just how far the aspirations for universalism and the search for the authority of knowledge have been alienated from the original practical concerns of critical theory and political theory in general. The solution, Leonard argues, is to redeem critical theory through (1) an "immanent critique" that puts into relief its aspirations and latent possibilities and (2) a "reconstruction" of certain social theories representing critical theory in practice and making good its aims (to which the largest part of the book is devoted).

Leonard provides a valuable, extended examination of dependency theory, Paulo Freire's "pedagogy of the oppressed," liberation theology, and certain aspects of feminist theory. What these theories have in common, he claims, is that they are "self-consciously tied to particular historical circumstances and practical contexts" and are "part of struggles where people are emancipating themselves," rather than a merely "esoteric and purely academic ideal."

What must be retained from modernist critical theory is its emancipatory goal and its voice rejecting transcendental social truth. While postmodernism provides the required historical or contextual stress, its skeptical antifoundationalism is as dangerous as—and in some sense merely the obverse of—alienated univeralism and makes it difficult to "take a stand that is in solidarity with those who are oppressed." Leonard argues that the critical theories in practice embody these requirements and thereby both vindicate critical theory and provide a model for assessing critical–theoretical endeavors. It is not enough, however, that they are simply critical and practical. If they are to be more than ideologies, they must contain a "metatheoretical self-understanding that preserves the demand for solidarity and the recognition of plurality and difference."

This is a serious book that deserves to be taken seriously. Leonard has taken an important step and confronted the fact that there is no philosophical solution to the problem of theory and practice. The recent literature of academic political theory is replete with work that like Leonard, seeks to defend an idea of critical theory that avoids foundationalism and yet circumvents relativism, thereby reconciling historicity and truth. But in nearly every case, the argument rests on a metatheoretical twist and on an epistemological strategy that avoids the concrete issue of the existential relationship between theory and practice. Although Leonard is offering still another metatheoretical mediation, he is also saying, quite simply, that in themselves, such academic constructions are sterile. Critical theory without a practical dimension is not critical theory. There are, however, he suggests, certain situated engaged theoretical practices that either go far toward realizing the idea of critical theory or point toward such a realization.

The examination of critical theory in political practice is an interesting and distinctive contribution, but Leonard is too modest and too painfully self-conscious about the inherent limitations of his general endeavor. He deeply feels the paradox that his project is an "academic discourse" when he is claiming critical theory must be more and that it is a metatheoretical project—"a theory about theories"—when he is arguing that metatheoretical diversions are a large dimension of the problem.

The problem with metatheory is not that it is inherently inauthentic but that it pretends to be something it is not—either theory or practice. Leonard does not fall victim to this delusion or temptation, but he does ultimately shy away from the issue that at some level, he realizes, lurks behind his discussion. This is the issue of the relationship between academic and political discourse in both its generic and specific dimensions.

Whereas Critical Theory is an institutionalized academic discourse and one that is largely metatheoretical, critical theory in political practice is a stipulative functional category for classifying certain practical or quasi-

practical discourses. Leonard is clear enough about this difference in principle; but in the course of his discussion, the distinction begins to blur, since the latter validates the former and the former is manifest in the latter.

Leonard begins his book with a confessional testimony to his sensitivity to being a privileged Western professional white male while having political commitments that involve "a desire to reach out to those who are engaged in . . . day to day empowerment struggles" and "who now suffer damaged lives." In critical theory he saw the possibility of reconciling the two; yet he realizes that his book is primarily written for, and will be read by, a largely academic audience.

His mediation is, in the end, a conceptual one whereby he underwrites practical critical theory by eliciting from it an "understanding of knowledge and human identity" that accords with the best instincts of academic critical theory. He wants at once to make theory practical and keep it pure. At best, he hopes to provide "analytical insights that might allow some—even if they are 'only' theorists—to begin to bridge the gap between critical theory and concrete, historically contingent political struggles."

There is a good deal of pathos in these reflections. They do not, I think, carry us very far in understanding or dealing with the concrete structural problems inherent in the relationship between the American (and maybe Western) university and politics and how personal political commitment is sublimated in philosophy and professional activity. But they—and as the book as a whole—bring us, both in spirit and substance, to a point where we cannot retreat from the dilemma without conscious repression and bad faith.

State University of New York, Albany JOHN G. GUNNELL

Nietzsche and the Politics of Aristocratic Radicalism. By Bruce Detweiler. Chicago: University of Chicago Press, 1990. 242p. $24.95.

Nietzsche constantly complained about being misunderstood, but he uncharacteristically once embraced George's Brandes's suggestion that his position was an "aristocratic radicalism." Bruce Detweiler agrees that Nietzsche's attempt to formulate a unique modern position in political thought, alongside "aristocratic conservatism" and "egalitarian radicalism," is best captured by that phrase; and he has written a book-length defense of such an interpretation. For the most part, he restricts himself to interpretive issues, quarreling mostly with those like Walter Kaufmann and Peter Bergmann, who argue that Nietzsche was an antipolitical or apolitical thinker, and those like Mark Warren, who argue that Nietzsche's attack on the nihilistic consequences of the Western tradition is most consistent with a politically pluralistic or liberal democratic position. (There is some brief reference to the European literature on these topics but no discussion of the relevant work on similar themes by Michel Haar, Philippe Lacoue-Labarthe, Henning Ottman, or the like.)

Detweiler argues first that Nietzsche's radical *perspectivism* and his attack on the traditional notion of truth are not inconsistent with the possibility of a general political position. A perspectival approach to classifyings and evaluatings entails neither any framework relativism (wherein we could not *understand* other perspectives) nor any surefooted claim that such classifyings and evaluatings must be false. Nietzsche's own position is said to be adopted "in the most tentative of terms," thus allowing "tentative convictions about relative adequacy" when assessing perspectives (p. 27). This adequacy appears to be a function of whether a perspective is "more honest and searching, more comprehensive and more noble" (ibid.). The discussion here is brief. The author only suggests that some evaluative position might be logically compatible with Nietzsche's perspectivism. He does not develop or defend Nietzsche's own position in those terms.

According to Detweiler's Nietzsche, there are only a very few capable of forming, creating, animating, legislating, or commanding such perspectives. Such rare artist-legislators form the center of the political story told here. Politics, as Detweiler's Nietzsche understands it, has a great deal to do with creating the conditions under which such noble types are possible and can thrive and with hastening the ever-accelerating collapse of the antiaristocratic, modern, egalitarian political culture. The strength of the book lies in Detweiler's marshaling of evidence to support the claim that Nietzsche's picture of a postnihilistic aristocracy was a decidedly political one, and in his detailed criticisms of contrary, antipolitical readings.

Thus, on this account, society *as a whole* "should be dedicated to the promotion of the highest type rather than to the universal good or the common goods" (p. 101). This highest type is a modern extension of the "master," or pre-Christian, morality identified in *On the Genealogy of Morals*. Its virtues include a robust egoism; indifference to the effects of one's deeds on, or their general acceptability to, others; a courageous self-reliance in the face of "the death of God" or the impossibility of any sort of cosmic reassurance about the worth of one's deeds; and some ability to integrate, create a structure for, or in some way harness and harmonize the various "drives" contending for power in one's internal life. Such a virtue is characterized as "Dionysean," although the author carefully differentiates Nietzsche's early, somewhat "metaphysical" use of that term in *The Birth of Tragedy* from the "self-forming" ideal that emerged in later works.

The book leaves a large question unanswered—perhaps the central issue in any discussion of Nietzsche's politics. It may be a matter of fact that the historical Nietzsche did claim that the self-forming, heroically courageous, independent individual described in Detweiler's later chapters was the proper telos of all political life and that the historical Nietzsche did vigorously propose political goals, concerned himself with antidemocratic politics, and encouraged a grand politics—all as if such enterprises were central to the very possibility of such a type. But are they? Why would the new "master of the earth," so contemptuous of the herd, want, or even need, to *rule* the herd? Is it some sort of anthropological truth that the everywhere-motivating will to power requires actual political domination? Is it a matter of simple prudence? Since the herd will not leave him alone, he must preemptively create the political world that will allow his survival? What sort of world is that? Why would not the traditional refuge of esotericism suffice?

And none of these possibilities is consistent with the "politics" of Zarathustra, who overflows with *love* of

man and is unable to remain in his cave, with animals. He needs men, even if already almost "last men." Even if the politics are "aristocratic" and the master needs only a certain few, the nature of that need and its implications for Nietzsche's supposed "individualism" remain important and still unanswered questions.

University of California, San Diego ROBERT B. PIPPIN

Compromise and Political Action: Political Morality in Liberal and Democratic Life. By J. Patrick Dobel. Savage: Rowman & Littlefield Publishers, 1990. 215p. $38.00 cloth, $18.95 paper.

The Moral Imagination and Public Life: Raising the Ethical Question. By Thomas E. McCollough. Chatham: Chatham House Publishers, 1991. 191p. $14.95 paper.

These two books would enable the traditions of communitarian and liberal political thought to respond to some standard criticisms leveled against them, particularly the difficulties both have in addressing the most serious practical problems of (American) political life. McCollough searches for a communitarian public ethic that attends to contemporary public policy as well as currents in American history; one that would revitalize the public realm as a place of ethical involvement for ordinary citizens. He endeavors to explain and remedy the current alienation from democratic politics felt by many Americans, paradoxically, when post-Communist nations seeking to renew themselves aspire to emulate many of its features. Dobel writes from the perspective of "responsible political actors"—he himself is chairman of a County Ethics Commission—and would teach them "about how individuals should hold moral beliefs in political life." (p. 3) His treatment of the ethics of compromise seeks to show how and why liberal principles may inform a successful politics. These volumes commendably eschew professional jargon wherever possible and avoid the solipsism of many academic debates. Their authors have written for intellectually curious generalists devoted to bettering our political life.

The non-technical approaches employed display both virtues and drawbacks. The virtues stem from the range of issues discussed and each author's interconnection of them. McCollough weaves together a critique of Lindblom's models of rational public policy formation; sympathetic accounts of a 1968 Duke civil rights vigil and Earth Day-1970; narratives of the American founding; complaints about utilitarianism, liberalism, and pluralism for their contribution to corrosive individualism, amoral scientism, and narrow rationalism; nostalgia for medieval community; reliance on the conceptual categories of Tonnies and Kierkegaard; appeals for deliberative democracy, and a call for Christian-based vision of moral community. Dobel's historical and normative scope is less wide-ranging, but he deftly integrates accounts of the way American politics has dealt with the legacy of slavery; critiques of Aristotle, Machiavelli, Weber, and communitarians; adaptations of Rawls; and analyses of the logic of prudence and justification. The drawbacks stem from both authors' inability to give any one of these topics their due and the inevitable distortions that come from severe simplification. For example, Dobel criticizes Aristotle (via MacIntyre and Beiner) for not being self-critical and McCollough interprets the Constitution as a betrayal of the ideals of the Declaration of Independence—each using only a few sentences to justify these sweeping claims.

But what about their arguments? While each extends and elaborates communitarian and liberal positions, neither has removed their limitations. McCollough makes a good case for the view that major strands in America's political culture and dominant modes of argumentation in ethics and public policy tend to marginalize the idea of community as a commanding ideal which, in turn, favors the political reason and power of elites over ordinary citizens and isolates ethical concerns from our public discourse. But he never seriously confronts the political phenomena which have given our dominant political ethos its persuasive force—namely, the character of conflict in American society. The clearest examples McCollough cites of "the communal point of view" having a beneficial, practical, and coherent effect are its instantiations by branches of government whose linkage to any participatory community is by definition suspect. One is the institutionalization of fluoridation (p. 59) which was primarily promoted by *regulatory* agencies of local and state governments. McCollough's other major example of a successful, communally based public policy is the reportedly successful institution of a *judicially* initiated school busing plan for Charlotte, North Carolina (p. 112f.). Moreover, busing may have worked in Charlotte, but nation-wide it has generated more opposition than approval. Finally, although McCollough's endorsement of the idea of a non-denominational "public church" as a vehicle for rekindling the desire of all citizens to believe in an American moral community is well-taken, his failure to consider the role of Catholic and various fundamentalist churches in the abortion controversy detracts from the authority of his position. McCollough has much faith in a basic linkage between participatory citizenship and the creation of a moral community, but what are we to think about Americans' prevailing ways of demonstrating their ideas of moral community? If we find them uncongenial, how do we determine which moral community is the really good one? In order to embrace a new ethos of community that will inspire and harmonize the American citizenry, we need to know more about how this new ethic can accomodate the conflictual differences that both enrich and divide the two hundred fifty million Americans who hail from all corners of the globe.

Dobel's discussion of political compromise would provide such an ethic. He believes that "compromise" has been given an unnecessarily bad name. It is inherently associated with neither immoral conduct nor *realpolitik*. To the contrary, it is a necessary tool of liberal and democratic politics that can become an instrument for both preserving the moral integrity of political actors and building an ethical political community. Rather than urging us to maintain political community by removing issues rooted in fundamental moral and religious commitments from the agenda of liberal democracies, as does Rawls's current work, Dobel believes that liberalism, via the art of political compromise, can help us accommodate them. So that "compromise" can fulfill the function Dobel ascribes to it, he infuses ethical substance into its ordinarily technical meaning. For Dobel, not any compromise will do. He is wary of how compromise can be used by conservatives (e.g. Burke and Oakeshott) to preserve the status quo and by amoral

politicians to undermine the political efficacy of critical, moral principles. But he stresses that unwillingness to compromise one's principles can encourage coercive, if not violent, behavior in public life and that presupposing a substantive notion of community as a basis for political justification inhibits the moral and political reflection of individuals. He therefore offers the normative idea of a "good compromise" as the key for practical political ethics. It is a "principled compromise" which produces an "integrated solution," awarding each of its parties equal Rawlsian respect while producing a mutually advantageous outcome. Dobel's historical examples of such compromises include the Missouri Compromise, the Civil War Amendments, and Lyndon Johnson's civil rights strategy (beginning in the Senate and continuing through his Presidency). But can one really get from compromise to community? Lasting, worthy compromises typically depend on practical conditions and ethical values which haven't been subject to compromise. If acquiring the art of compromise would improve the conduct of American politics, the acceptance of the liberal principles that can insure its successful practice must already be an accomplished fact. In other words, even as Dobel would seek to demonstrate a new political ethic of compromise, its persuasiveness depends on a satisfactory answer to prior questions about the coherence of liberalism as a political theory and its correspondence to political reality.

Although these authors approach the problems of ethics and politics from fundamentally opposed theoretical standpoints, they share common political goals. Both would have America become more egalitarian, participatory, enlightened, and democratic. Unfortunately, in these days of scarcity, what communitarianism there is the United States seems to subvert rather than promote a democracy of free and equal citizens, while liberalism continues to weaken as an effective political ideal. If we are to understand better how to make compromise a source of community, and ideas of moral community new ideals for public policy, we may need to abandon the meta-ethical positions of liberalism and communitarianism. Freed from these theoretical moorings, we may be in a better position to diagnose the ethical and practical elements of our new world order, which makes these authors' laudable convictions sound like sirens from the past.

Hunter College, CUNY JOHN R. WALLACH

The Nature of the Right: American and European Politics and Political Thought since 1789. Edited by Roger Eatwell and Noel O'Sullivan. Boston: Twayne, 1990. 203p. $29.95 cloth, $14.95 paper.

Reactions to the Right. Edited by Barry Hindess. New York: Routledge, 1990. 193p. $54.50 cloth, $19.95 paper.

The Political Economy of the New Right. By Grahame Thompson. Boston: Twayne, 1990. 216p. $29.95 cloth, $14.95 paper.

The Political Theory of Conservative Economists. By Conrad P. Waligorski. Lawrence: University Press of Kansas, 1990. 260p. $29.95.

Nearly 40 years ago, at the peak of McCarthyism, Richard Hofstadter referred to the American extreme Right as a species of "pseudoconservative" dissent "because its exponents, although they believe themselves to be conservatives and usually employ the rhetoric of conservatism, show signs of a serious and restless dissatisfaction with American life, traditions, and institutions" (*The Paranoid Style in American Politics and Other Essays*, [1979], p. 43). Associated with this curious mixture of conservatism and radicalism and with more violent radicalisms of both the Right and the Left, he suggested, was the "paranoid style" of mind whose main elements included a hyperrational awareness of "a 'vast' or 'gigantic' conspiracy as *the motive force* in historical events" and a crusading "will to fight things out to the finish" rather than compromise with the evil conspirators (pp. 29, 31; emphasis original). Pseudoconservative paranoia erupted during the early 1950s among Taft Republicans, who contended that godless communists had infiltrated the federal government and other key institutions, including universities and the mass media, as part of a sustained conspiracy—most evident in Roosevelt's New Deal—to destroy market capitalism, undermine traditional religious and moral values, and, ultimately, transform America into a cog within the Soviet empire. This fringe movement gained ground through the next two decades, achieving a temporary national prominence during the 1964 Goldwater presidential campaign. Increasing numbers of affluent whites and lower-middle-class fundamentalists apparently became persuaded that the eastern Republican establishment had capitulated to, and/or was too incompetent to defend the American way of life from, communists, socialists, and their possibly unwitting allies, variously labeled welfare democrats, Keynesians, and liberals.

By 1980, with the election of Reagan, the pseudoconservative moment had arrived. During the 1980s, the New Right began the attempt to "repossess" America and the federal government from liberals and their ilk. Their agenda, more or less continuous from Taft to Reagan, may be reduced to three main elements: no compromise with the Soviet empire as long as it subscribes to godless communist or socialist ideals; restoration of traditional religious and moral values by means of suitable amendments and/or judicial decisions (e.g., permitting religious instruction in the public schools) and state and local government sanctions (e.g., against pornography and abortion); and liberation of market capitalism from arbitrary government regulation, again with the help of suitable amendments and/or judicial decisions (e.g., limiting federal taxation, requiring a balanced budget, and limiting welfare expenditures). Moreover, in attempting to carry out this agenda, their uncompromising paranoid style has led to heightened ideological conflict in American politics, despite the fortuitous détente with the Soviet Union. As Patrick Buchanan puts it: "Our political and social quarrels now partake of the savagery of religious wars because, at bottom, *they are religious wars*. . . . Someone's values are going to prevail. . . . The only questions are whose religion, and whose morality, shall we consult?" (*Right from the Beginning* [1988], pp. 337, 342; emphasis original). Perhaps the "kinder and gentler" Bush administration is moderating this divisiveness to some extent, but the question must remain open for now.

Hofstadter's term *pseudoconservatism* underscores the point that the New Right morality is too radical to be truly conservative in the classical sense of Burke. This raises the difficult general issue of the nature of the Right, including what distinguishes it from the Left.

Roger Eatwell, in five chapters of *The Nature of the Right*, suggests that the Right, at least in the period since the French Revolution, includes five styles of thought, not necessarily compatible, that have emerged historically in response to liberalism and socialism–Marxism. More specifically, reactionary and moderate right-wing styles emerged in response to certain eighteenth-century liberal notions of individual liberty and perfectionism; radical and extreme styles emerged in response to nineteenth- and early twentieth-century socialist ideals of social and economic equality; and the New Right emerged during the 1970s and 1980s in response to the perceived incompetence of socialist and left–liberal administrations (pp. 63, 74). Five remaining chapters, by as many authors, illustrate the five styles by discussing, Charles Maurras's royalist critique of French republicanism, moderate British and American conservatism, the "conservative revolutionaries" of pre–World War II Germany, fascist conspiracy theory in post–World War II Europe, and New Right views of civil society in Europe and America, respectively.

Despite his brief remarks about the importance of ideology and "the marketing of political ideas" (p. 26), however, Eatwell never clarifies why these five styles of thought are properly classified together under the right-wing label or why it is useful to think in terms of right and left. His approach is reminiscent of Samuel Huntington's well-known "situational definition" of conservatism, according to which true conservatism is a "positional ideology" involving "the passionate affirmation of the value of existing institutions," whatever their content (*APSR* 51:455, 468). But at least two problems bedevil Eatwell's analogous situational definition of the Right. First, the nature of the Right remains ambiguous until we clarify the content of the left-wing challenge to which it is reacting in the given context. Second (and perhaps more important), Eatwell's extension tends to hide the fact that these five styles of thought are really quite different mixtures of conservative and radical elements. Huntington argued persuasively that radicalism is the opposite of conservatism, the essence of the former being "criticism of existing institutions," whatever their content (p. 458). In his view, neither conservatism nor radicalism per se is committed to normative ideals of any particular content. These isms are merely opposing attitudes toward existing institutions so that depending on the situation (in particular, the nature of existing institutions), each is compatible with many substantive ideals, including those associated with this or that version of liberalism (pp. 460, 472–3). It follows that support or criticism of any normative political theory will, except in limiting cases, involve a mixture of conservative and radical attitudes toward actual institutions. Of course, alternative theories might usefully be classified as moderate or extreme according to whether they imply piecemeal or revolutionary institutional change relative to the given status quo. If existing institutions are basically liberal, for example, then such a classification would call attention to the similarities between different types of moderate views (e.g., progressive conservatism and gradualist liberalism), as well as to the differences between them and radical views that are nonliberal by definition in this context. But any such classification by itself tells us little about the justification of competing political theories and their respective ideals. Moreover, its contextual validity must disappear as existing institutions change. Perhaps for these reasons, it can hardly be expected to correspond to any popular left–right spectrum.

Grahame Thompson, in *The Political Economy of the New Right*, argues that the Reagan and Thatcher governments were in practice constrained to adopt moderate economic policies at odds with their radical official postures. He suggests that New Right political economy involves four distinct doctrines, namely, monetarism (Friedman); supply-side economics; economic neoliberalism, including neo-Austrianism (Hayek); and the public choice approach (Buchanan), with two common features: "celebration of the virtues of the market, and . . . antithesis to the idea of a Keynesian welfare state" (p. 2). Building on earlier work, he concludes that the New Right administrations did not make radical changes on the basis of these doctrines, despite much official rhetoric to the contrary. For the most part, these governments merely introduced some less direct forms of market regulation, tinkered with the broadly Keynesian fiscal and monetary policies of earlier governments, and fostered substantially more inequality. The high economic growth observed in both the United States and Britain during the 1980s resulted not so much from monetarist policies or supply-side incentives to producers, for example, but more from continued government spending and an explosion in consumer demand, facilitated by easy credit and income tax cuts for the rich, during a normal cyclical recovery from the recession of 1979–81. More generally, Thompson seems to be saying that it is unhelpful to deploy labels such as New Right and Keynesian on the presumption that a radical ideological switch might empower us to alter the fundamental direction of the economy. Rather, we should recognize that workable policy ideas are largely ad hoc, piecemeal responses to complex and ill-understood changes in the economy itself.

Like Thompson, Conrad Waligorski is interested in the political economy of the contemporary Right. In *The Political Theory of Conservative Economics* He offers numerous insights into the normative political theory, which he claims is shared in broad outline by many influential economists today, in particular, Friedman, Hayek, and Buchanan. Despite its radical libertarian elements, he describes the theory as conservative for its insistence that human reason is too weak to understand or reform the web of conventions whose spontaneous evolution makes social order possible, its sanctification of certain contingent forms of inequality as inseparable from the spontaneous social order, and its related mistrust of democratic politics and political equality. In his view, the theory is inadequate because it arbitrarily selects certain private property conventions and merely takes for granted the optimality of a spontaneous market order based on them. It seeks, above all, to preserve this market order to the neglect of any individual's fate; and it systematically distorts and impoverishes traditional political ideas of freedom, equality, democracy, justice, and community by forcing them to conform to the market vision. Thus, freedom, for example, is reduced for the most part to following the spontaneous rules of the market. Despite the fact of unequal bargaining power among its participants, the market is noncoercive, "because it is impersonal and spontaneous and its results are unintended, unforeseen, and undirected by any one person" (p. 61). Although defenders of this sort of theory are not without replies to many of his objections, Waligorski nicely brings out some of the tensions

between its conservative and radical libertarian elements. For example, from the Huntington-type perspective, true conservatism defends whatever conventions emerge from the evolutionary process and thus seems unable to fix the content of property conventions in the way libertarians want. These theorists need criteria to tell us why property conventions of a definite content are more desirable than others and why property conventions should predominate when they conflict with other kinds of conventions—for example, political conventions both within government and between government leaders and citizens. Without such criteria, libertarians merely take for granted that certain private property conventions are stable equilibria of the evolutionary process and that various kinds of conventions can in principle coexist in a sort of natural harmony within their respective separate spheres (e.g., market and government) if only ambitious political leaders can somehow be kept within their proper spheres. But the same general form of argument has been used by Michael Walzer in *Spheres of Justice* to defend a version of democratic socialism.

Finally, the Hindess volume, *Reactions to the Right*, offers a critical evaluation of contemporary British socialist thought, with a view to developing serious alternatives to New Right policy ideas. Three chapters together profess the need for a socialist alternative to both radical market socialism and radical democratic socialism—in short, a version of socialism that does not sacrifice traditional left-wing ideals of distributive justice to either unregulated market procedures or democratic procedures. Thus, Jim Tomlinson argues that more reliance on suitably regulated markets may be desirable, with the caveat that much work remains to be done on the design of a suitable "regulatory regime" to constrain powerful corporate bureaucracies (pp. 45–49); and Paul Hirst suggests that parliamentary reform is needed to bring about an "open corporatism" (p. 168). The four remaining chapters consider specific policy approaches in the areas of exchange rates, secondary education, welfare, and race relations. Much of the discussion is familiar in the American context and reminds the reader how deep an ideological gulf can separate moderates from radicals. For example, progressive conservatives and cautious liberals are unlikely to find helpful the radical notion of "institutional racism" discussed by Juliet Cook and Julian Clarke (p. 140). The notion implies that the white community, through its institutions, can be unjustly harming nonwhite minorities if institutional outcomes depart from some statistical measure of equality, even though nobody can point to any racist acts on the part of identifiable individuals.

Tulane University JONATHAN RILEY

The Political Philosophy of Michael Oakeshott. By Paul Franco. New Haven: Yale University Press, 1990. 277p. $30.00.

Most of the obituaries that followed the death of Michael Oakeshott in December 1990 were so inadequate that one must be especially grateful for Paul Franco's exhaustive and philosophically careful study. If one believed the *New York Times*, Oakeshott was a high-tory oracle, a "right-wing guru". If one did marginally better and gave credence to the *London Times* he was, though a philosopher who "stood fastidiously aside" from politics, nonetheless the "articulator" of the "real philosophical foundations of Mrs. Thatcher's policies." As Franco is careful to point out, Oakeshott had insisted as early as his first important Hobbes essay in *Scrutiny* (1935) that "every man, I suppose, has his political opinions. . . . But a political philosopher has something more: he has an analysis of political activity, a comprehensive view of the nature of political life, and it is this, and not his political opinions, which it is profitable for a later and different age to study" (p.). For Oakeshott, then, if political philosophy is to be worth more than the efforts of a fact-gathering *philosophe* such as Bentham ("as a thinker he is negligible"), it must "mediate between politics and eternity," must move from opinion to philosophy. Oakeshott made that clearest in a remarkable essay from 1938, "The Concept of a Philosophical Jurisprudence" in *Experience and Its Modes*: "Philosophical thought and knowledge is simply thought and knowledge without reservation or presupposition. . . . It should be thought of as something which happens at the end, when the concepts of science, or commonsense, or practical life are subjected to the revolutionary and dissolving criticism of being related to a universal context." (Franco helpfully rehabilitates this neglected essay, together with Oakeshott's other writings from the twenties and thirties.)

The stress on philosophy as "a universal, self-complete context" alone able to convert mere opinings into a "fully considered politics" that looks for its "reflection in eternity" suggests (as Franco sees) an Oakeshott rather different from the one made familiar in *Rationalism in Politics*. Indeed, one can wonder whether that brilliantly successful volume, with its insistence on the "sympathetic pursuit of intimations" within inherited historical traditions does not cast a misleading Brukean light on Oakeshott's vision of politics; for the chapter "Political Education" first annihilates two straw men (empirical ideological politics) in a way that reduces one to helpless laughter, then magnifies "our tradition of political behavior" in language redolent of Burke's *Reflections on the Revolution in France*. In much of *Rationalism in Politics*, one sees an Oakeshott wonderfully in command of a satirical idiom more narrowly English and less philosophical than elsewhere. For example "Freedom, like a recipe for game pie, is not a bright idea": the freedom of an Englishman is simply the availability of procedures such as habeas corpus.

Even those who doubted that *Rationalism in Politics* was as deep as it was effective could never have doubted that Oakeshott was, from the 1940s on, the supreme interpreter of Hobbes in this (or perhaps any) century. Franco well brings out the importance of Oakeshott's introduction to *Leviathan* (1946) and "The Moral Life in the Writings of Thomas Hobbes" (1960). With unerring insight, Oakeshott maintained that Hobbes's civil thought was the supreme expression of the philosophical tradition that viewed will and artifice as the heart of politics. Since Hobbes holds that "wills . . . make the essence of all covenants" (*Leviathan* 40) and that covenants alone generate sovereign legal authority, it seems that Oakeshott can scarcely have been wrong about Hobbes's voluntarism. Even if Oakeshott's own thought were to vanish, he would remain consequential as the most intelligent illuminator of "the sole masterpiece of political philosophy written in the English language."

Franco quite properly reserves his main interpretive

efforts for Oakeshott's magnum opus, *On Human Conduct* (1975). That book is divided into three parts: (1) a compressed, dense, difficult account of human agency, intelligent self-determination, and self-enactment; (2) an account of the "civil condition," or *respublica* (above all authoritative *lex*), which citizens are to subscribe to in their pursuit of self-chosen satisfactions, with politics restricted to maintaining and occasionally rethinking *lex* (rather than indulging in a Baconian transformation of the world through joint enterprise); (3) an account of the advent of every significant moral disposition that has appeared in Europe since the fall of the Roman Empire, notable for distinguishing between the state as a *societas* of authoritative rules subscribed to by citizens in their chosen self-enactments and the state as a *universitas*, an "enterprise association" ruled by "teleocratic" managers in pursuit of a "substantive common purpose." In pages of great brilliance and insight (well treated by Franco), Oakeshott urges that the state as *societas* has been most adequately theorized by Aristotle, Hobbes, and Hegel (the half-dozen pages on the *Philosophy of Right* are incomparable) and is clearly contemptuous of the "servile" notion of the state as a "warm" *universitas* providing a "social end" to anti-individuals who have no self-chosen purposes. (Here, Aquinas, Calvin, Bossuet, Rousseau, and Marx are lumped as partisans of teleocratic "enterprise management"; and Montaigne is celebrated as the greatest understander of "the human condition as an adventure in personal self-enactment".) If *On Human Conduct* now receives a more sympathetic reading than it got in the 1970s, that will be in part because Franco shows how extraordinary a work it really is.

If, indeed, there is anything to complain of in this excellent piece of scholarly interpretation, it is that Franco tries a little too hard (mainly at the end) to relate Oakeshott to contemporary debates over liberalism and communitarianism, attempting contrasts with Rawls, Rorty, Dworkin, Sandel, and so on. But Oakeshott's real rapport is with Hobbes, Montaigne, Montesquieu, and (above all) Hegel—as Franco himself proves. If Oakeshott used the vocabulary of Roman jurisprudence (*respublica, lex, societas, universitas*), that was in part to mediate between his own politics and "eternity"—to make himself a part of the long philosophical tradition that he memorably interpreted and faithfully sustained.

University of Wisconsin, Madison PATRICK RILEY

Resurrecting Marx: The Analytical Marxists on Freedom, Exploitation, and Justice. By David Gordon. New Brunswick: Transaction Books, 1990. 155p. $29.95 cloth, $16.95 paper.

Political Identity: Thinking through Marx. By Robert Meister. Cambridge: Basil Blackwell, 1990. 426p. $44.95 cloth, $17.95 paper.

These two books offer radically alternative ways of making sense of Marxism. David Gordon dismisses Marxism as a viable theoretical position in any of its many possible formulations. Robert Meister creatively considers how Marx's work itself can be useful in making sense of contemporary politics, economics and philosophy and ultimately comes to the conclusion "that the Marxian moment is now" (p. 348).

Gordon claims that economists and philosophers in the 1950s and 1960s considered Marxism an "outmoded relic." The "analytic Marxists" (his focus is on G. A. Cohen, Jon Elster, and John Roemer) tried to resurrect Marxism using the tools of analytical philosophy. He presumably uses the same tools to examine their work. The book, though superficially sophisticated, is based on a rather crude argument. Capitalism is not exploitative, Robert Nozick is right, and an abstract model of laissez-faire capitalism can correctly define justice. The analytical Marxists cannot refute the "*prima facie* argument for the justice of capitalism," namely, that "*other things being equal*, . . . a just economic system is one in which people are free to make any economic arrangements they wish, so long as they do not coerce others" (p. 33). Therefore analytical Marxism is "a complete failure" (p. 144). This is hardly the "neutral" approach to the issue of capitalism we were promised (p. 33). Without serious empirical evidence there is no reason to accept his claim that "laissez faire capitalism has just this desirable feature [justice] while socialism, as usually interpreted, does not" (p. 77).

Meister asks how we might understand the relevance of Marx's work for contemporary criticism of existing institutions. His main theme is that it is essential to understand Marx as a critical theorist responding to the political and intellectual climate in which he worked. He makes a compelling case that the present situation is remarkably similar to that in Western Europe following the revolutionary upheavals of 1848 and that Marx's critical method can be used now in much the same way as it was then.

Gordon praises the "analytical Marxists" because they ignore or reject anything that even vaguely resembles "dialectics" or the "bizarre and hermetic terminology" (p. 19) of those who would attempt to make sense of Marxism as a response to Hegelian thought. Meister argues for renewal of Marxian critique as a response to the Hegelian character of modern thinkers ranging from Francis Fukuyama and his "mentor" Allan Bloom (p. 345) through Samuel Huntington and Sheldon Wolin to academics representing the New Left. Meister's explication of Hegel should be required reading for those tempted to take Gordon's view of Hegel's relevance to modern thought.

The differences between the two approaches are clearest in their treatment of Marx's economic theories. Gordon is satisfied to reject the "labor theory of value" largely because it offers no useful account of the prices of commodities. He deigns to note neither that Marx adopted the labor theory from Locke, Smith, and Ricardo nor the implications of this fact. Meister, in his normal scholarly manner, engages in an extensive discussion of what Marx actually said about economics and how this related to his critique of the classical political economists. He uses a novel reading of *Capital* to reject some important elements of Marx's substantive economic views. He shows how Marx's basic method of economic critique is tied to his critique of ideology and politics and concludes that even though Marx made some important substantive errors in his economic thought, his basic method is extremely useful in examining contemporary economic systems and economic theory.

Meister rejects all simplistic notions of what Marx had to say about politics. Gordon hardly addresses the subject except in his simplistic version of a kind of

rational choice notion about justice. Meister argues that Marx's view of the relation of classes and political interests was a complex one that attempted to make sense of why "some historical differences between groups become politically salient, while others, apparently as great, do not" (p. 153). He suggests that the important political question is to decide "when and whether to support ideologically progressive demands of those victims of class exploitation, and how to regard demands of those victims that are not yet ideologically progressive" (p. 153). This analysis is far superior to dogmatic notions about class relationships or to much contemporary discussion of how groups other than classes make sense of progressive politics. His critique of feminist theory throughout the work is especially compelling in this respect. Meister argues that those who are to be guided by Marx's thought must exorcise the ghosts of Rousseau and Hegel and avoid the "revolutionary mystique" of much Marxist analysis, emphasizing what he sees as Marx's more mature political views that developed in the nonrevolutionary context that followed 1848.

Progressive students of politics and political activists who think they can find guidance in Marx must consider Meister's work. They will find much of his analysis at least as critical of the Left as it is of the Right. Those who reject this whole political perspective will nevertheless find much of interest in, for example, Meister's discussion of the concepts of pluralism and corporativism and his lengthy discussion of Tocqueville. Gordon's work may be of value to students of analytical philosophy interested in Marx and to those looking for a model of libertarian polemic through which to eliminate even the last vestiges of socialist theory. Students of political thought, whatever their own political perspective, should read Meister as an example of serious analysis; Gordon can be passed over with little loss.

California State University, Fullerton BRUCE E. WRIGHT

Simians, Cyborgs, and Women: The Reinvention of Nature. By Donna J. Haraway. New York: Routledge, 1991. 287p. $55.00 cloth, $16.95 paper.

Donna Haraway is an original and exciting voice in feminist theory. This book of essays, the author notes, marks her evolution from a "US socialist–feminist, white female, hominid biologist" to a "multiply marked cyborg feminist" (p. 1). Yet I am struck by the consistency over time of the two passions that have motivated her work: understanding science and devoting politically motivated attention to difference–multiplicity (whether produced along the lines of race, gender, class, or sexuality). Both these discourses, she argues, must be understood as social constructions. Her dense, allusive, and playful prose moves back and forth among discussions of biology, primate studies, philosophy of science, literary criticism, science fiction, and her own mythmaking. For social scientists, Haraway's efforts should make it far more difficult to maintain an image of the natural sciences as more objective than the social sciences could ever be.

Haraway, from the earliest essay to the most recent, attempts to denaturalize science. The first three essays in the book focus on theories in biology. In the first, "Animal Sociology and a Natural Economy of Body Politics," she argues that in primate studies one finds a "naturalization of the patriarchal division of authority in the body politic" in such a way that the fallacies of any claim to objectivity are exposed (p. 12). At the same time, Haraway refuses the distinction between bad (ideologically marked) science and good (objective) science. Science, she argues is neither discovery nor invention but, rather, a process through which we "both learn about and create nature and ourselves" (p. 42). She refuses the split between nature and culture and insists that the creation of ourselves is bodily, as well as social.

The other two essays in this section explore similar themes through different literatures. "The Past as Contested Zone" argues that it mattered that some of the scientists who worked on evolution by studying primates were women. They rewrote the stories men had put forward about human evolution and shifted the arguments from man the hunter to woman the gatherer. "The Biological Enterprise" examines how the images of the machine and the market have centrally organized theories of biology. Once again, Haraway shows science to be permeated with, and structured by, the social relations of a particular time and place. But once again, Haraway insists that there is no clear distinction between "objective science and abusive ideology" (p. 67).

Part 2 pushes further the claim that what we regard as science is deeply marked by social relations; and in focusing on the notion of "contested readings," Haraway calls attention to issues of power in both science and social life and to how power structures what will count as legitimate readings or even "real" experience. "In the Beginning Was the Word" and "The Contest for Primate Nature" insist on the fictive character of science as the telling of important public stories that rest on the process of production and reproduction of human life. But because science is not a mechanical reflection of ideology, Haraway is concerned to trace the development of the new stories told by women primatologists, point to questions about who saw what in the field, and document the destabilization of "facts." "Reading Buchi Emecheta" (a Nigerian novelist) grows from Haraway's experience as an interdisciplinary women's studies scholar. Yet her argument here resembles that of the first two essays. Now experience, as well as science, must be recognized as an intentional artifactual construction. Experience—in particular the experience of women—is not immediately given but must be contested for.

All this lays the ground work for "Differential Politics for Inappropriated Others," which makes the most important, original, and indeed brilliant interdisciplinary contributions. In "Gender for a Marxist Dictionary," Haraway argues that an adequate feminist theory must be a theory of racial difference in specific historical conditions and calls for the development of theories of difference whose geometries can break out of binary oppositions. One such dictotomy important for Haraway is the sex/gender (or nature/culture) opposition. While Haraway recognizes the strategic usefulness of the sex/gender distinction, and how gender was developed as a category to problematize what was to count as a "woman," she argues that the categories of sex, flesh, body, biology, race, and nature must be historicized.

In her now classic "Cyborg Manifesto" Haraway argues that boundaries between human and animal, organism and machine, and physical and nonphysical machine have all become ambiguous in the late twenti-

eth century. In this world, feminists need to learn how to read webs of power and form new coalitions. She is beginning to tell her own feminist stories and to play with the possibilities for vision opened by science fiction in a postmodern world.

"Situated Knowledges" represents an effort to pull back a bit from the brink to which she moved in the previous essay. But once again, she shrinks from the dangers of an easy positioning, noting that while she wanted a strong tool for deconstructing science, the combination of radical social constructionism coupled with some versions of postmodernism could lead feminist theory into a "self-induced multiple personality disorder" (p. 186). The problem is how to cling simultaneously to an account of radical historical contingency for all knowledge claims and a commitment to faithful accounts of a "real" world. This is, indeed, an important problem for feminist theory. Haraway calls for partial, locatable, critical knowledges in which what had been the passive object of knowledge becomes an active agent—the world understood in the form of the Native American image of Coyote. The final essay, "The Biopolitics of Postmodern Bodies" returns to the themes of the more scientifically oriented pieces from earlier sections and once again subverts any logic of naturalization. "Science *is* culture," Haraway declares.

While Haraway was trained as a biologist, her work is genuinely interdisciplinary—in dialogue with feminist theorists from a wide variety of disciplines—and represents an important resource for political theorists. She undermines and subverts the image of natural science common among social scientists. In addition, her efforts to develop alternative geometries and strategies for reading social life in the late twentieth century are profoundly valuable additions to interdisciplinary scholarship in the social sciences.

University of Washington NANCY HARTSOCK

Voegelin on the Idea of Race: An Analysis of Modern European Racism. By Thomas W. Heilke. Baton Rouge: Louisiana State University Press, 1990. 161p. $22.50.

Throughout his career Eric Voegelin reminded us of the Platonic insight that political philosophy arises as an active resistance to the forces of human and social disorder that threaten the very possibility of the good life. In a nonideological era, such acts of resistance could draw upon the speculative traditions that were based upon the rational elucidation of an individual's experience of human existence-in-truth. Assuming the universality of such experience, available (according to Voegelin) in the undifferentiated insights of common sense, the topoi of discourse were established by the very structure of existence itself. Thus, meaningful debate and critique were possible because reason was rooted in the community of true existence; and that rootedness, in turn, provided both the ground for, and the rules of, serious discourse.

In Voegelin's analysis this possibility of truly rational debate has been seriously challenged by the emergence of political ideologies. Typically, ideological thought begins by first denying the authority of existence-in-truth and then proceeds by building its speculative structures upon an alternative or imaginary "reality" that is closed to the appeals of common sense. In this situation, debate becomes meaningless because speculation has been cut free from that discipline provided by a common and shared experiential base. Consequently, in an ideological age philosophical resistance to political disorder must take the form of an analysis of both the nature of the particular imaginary "reality" in question and the specific motivations accounting for its creation in the first place.

One particularly useful element of Thomas Heilke's *Voegelin on the Idea of Race* is that it presents to the reader a summary of Voegelin's earliest efforts to confront and thereby resist an important modern ideology. As a German living in Vienna from 1910 to 1938, Voegelin experienced firsthand the reality of an emerging political community in the process of ordering itself according to the fundamentally irrational principles of a racist ideology. In an effort to understand this phenomena, he published two texts in 1933 that have yet to be translated into English: *Rasse und Staat* and *Die Rassenidee in der Geistesgeschichte von Ray bis Carus.* Their publication led to Voegelin's being regarded as an enemy by the Nazis and his eventual flight from Austria following the German occupation in 1938.

At one level, Heilke's account is a fairly straightforward effort to summarize the contents of the two texts. He divides Voegelin's argument into three sections. The first, developed within *Rassenidee,* is essentially a historical account of the idea of race up through the end of the nineteenth century. Of specific importance to Voegelin is the perceived transformation of the race idea from an essentially transcendental understanding of life in general and human life in particular to a fundamentally imminent perspective on both.

The second and third elements of Voegelin's arguments are contained within the single text of *Rasse und Staat.* First, Voegelin sought to explore the status of the scientific claims that accompanied the emergence of modern racial ideology. In doing so, he paid particular attention to the tendency of race theorists to be epistemologically naive about what it was they were doing and thus substitute "speculative constructs" for the real units of theoretical analysis. Second, Voegelin explored the role of race ideas in informing and sustaining the shared web of meanings, beliefs, and expectations that partially constituted the European political communities of his day. The efficacy of such ideas can be partly understood in terms of the felt need for a concept to help order and explain the variety of human types that was becoming increasingly visible as a result of the expansion of Europe's historical and geographical horizons in the nineteenth century.

In addition to seeking to introduce the English reader to an important theme in Voegelin's earliest works and, in doing so, to provide background for a better understanding of his mature work, Heilke also intends to extract "a series of analytical tools from Voegelin's early though that aid in understanding political phenomena in genera" (p. x). Here, however, the volume is less successful. Heilke does, indeed, discuss such Voegelinean themes as archetype, ideas, experience, and mystery—but in a somewhat elementary manner. Part of this may be due to the fact that Heilke is primarily concerned with those analytical tools insofar as they explain the meaning of the text with which he is working, rather than as focal objects of his own analysis. At the same time, however, the elementary nature of this

discussion may also be due to the fact that these analytical concepts were still not fully developed in Voegelin's own thinking. Voegelin's late realization that in order to understand an ideology one should move beyond a review of its statements to an analysis both of its "imaginary reality" and of the motives that explain its creation is anticipated, to some extent, in these early works. (See Voegelin's discussion of the essentially mysterious quality of our human unity and his analysis of Schelling's concept of *Angst*.) Yet it does not receive the focused attention that would come to characterize Voegelin's mature work on political order and human consciousness.

Loyola University, Chicago JAMES L. WISER

Can Modernity Survive? By Agnes Heller. Berkeley: University of California Press, 1990. 177p. $25.00.

Agnes Heller's latest book is a collection of essays organized around the theme of modernity and its prospects. It is an elegant, erudite book, written with Heller's usual clarity and cogency, addressing issues at once philosophical, political, and ethical. Heller—philosopher, prolific member of the Budapest School, and celebrated *intellectuelle engagée*—is a committed modernist and writes with the urgency of an unreconstructed partisan seeking to convince her readers of the risks to modernity and the enormous benefits, primarily political, that come from its actualization. Her essays have modernity as their subject; and they are, in themselves, the kind of urbane, carefully reasoned, politically aware statements of a decidedly modern theorist.

As the title suggests, *Can Modernity Survive?* focuses on a the conditions and possibilities of modern social and political arrangements and their future and advocates their preservation and actualization against threats from within, that is, from the contingency of modernity itself. According to Heller, modernity is not a "totality" but a framework to house a multitude of social orders and ways of life. Hence, modernity does not advocate one "supreme" way of life or set of values. Instead, "modernity does not merely tolerate, it positively cultivates difference." Its hallmark is difference and diversity—in a word, *freedom* to "transform one's contingency into one's destiny." The concretization of freedom is modernity's project, Heller argues, and its precondition: "Freedom is the central modern value and the interpretation and determination of freedom is what modern politics is all about." The question of modernity's survival, then, is primarily a political question, an inquiry into the prospects for freedom.

The same contingency which underlies the modern experiment also threatens to destabilize modernity's accomplishments. As Heller notes, the more we permit diversity, the more people begin to live uniformly. The more industrialization spreads, the more working conditions resemble one another. The "symmetrical reciprocity" of modern social relations results in conformity and conditions of mass culture. Modernity's freedom is a camera obscura, Marx wrote, subverting the moral freedom of the Kantian individual (which Heller clearly prizes). Heller hopes to counter the " 'death wish' at the heart of modernity" by outlining the lessons learned in the Western experiment with modernity and urging their cultivation.

She turns initially, in "Hermeneutics of Social Science," to the constitutive role of the social sciences in the formation of self-knowledge. The reader is reminded of the three functions of an informative social science: to create meaning, not primarily to solve problems; to establish true knowledge, not truth; and to provide understanding, which requires translation back into the language of the subject. A humane, modest activity, social-scientific inquiry, in Heller's view, is a moral and political enterprise. The ethical component of political and philosophical discourse is again strikingly endorsed in "What Is Practical Reason?," where Heller examines the political responsibility of the philosopher—in particular, the moral status of Heidegger, the man and philosopher, and the political status of his philosophy. In other essays, Heller examines the concept of the political (a distinctly modern concept, she maintains), the possibility for an emotionally fulfilling life, the priority of freedom over happiness, and the importance of political rights.

Throughout it all, one looks for criticisms of modernity or discussions of postmodern commentaries on modernity: its Eurocentric bias, its reliance on the state, its support of technology, its destruction of "subaltern" cultures. But for Heller, the criticism seems to be that the modern was not instituted thoroughly enough. Moreover, the postmodern is not a critique or a commentary on the modern but the culmination of the modern: "Postmodern is not what follows after the modern age, but what follows after the unfolding of modernity. Once the main categories of modernity have emerged, the historical tempo slows down and the real work on the possibilities begins." The possibilities of modernity are, it seems, just beginning with the emergence of the postmodern. With postmodernity, we are promised more of the same, only better. Thus, the answer to "Can modernity survive?" is a guarded and qualified *yes*. Modernity can survive if we improve on it.

Macalester College LESLIE J. VAUGHAN

History and Modernity in the Thought of Thomas Hobbes. By Robert P. Kraynak. Ithaca: Cornell University Press, 1990. 224p. $28.50.

When once asked what he understood himself to be, Leo Strauss is reputed to have answered, "an historian." Whatever the valence of Strauss's use of the term, Kraynak here has offered a reading of Hobbes that is in certain respects sympathetic to Strauss and provides the long-overdue service of assessing Hobbes's theory of civil history and its bearing upon the rather abstract and ahistorical political theory with which readers of *Leviathan* are familiar.

Notwithstanding the precariousness of life in the hypothetical state of nature, Kraynak convincingly argues that Hobbes's extensive reading of civil history led him to the conclusion that the struggle for survival that marked the early stages of civil history had been supplanted by a struggle of opinion—an age of "doctrinal politics—that was far more dangerous (chap. 1). Echoing Rousseau, Hobbes would have it that vanity, the wish to think highly of oneself—in short, the very thing that separates us from the beasts—makes human struggle so unlike the struggle in the animal world. Contra Marx,

human struggle in the civil, doctrinal age is not *really* over resources.

The Hobbesian project, synonymous here with the project of modernity, purports to offer the only antidote for the intellectual vanity at the root of doctrinal politics—to deny the authority of all vain and ambitious men who claim wisdom for themselves. Hobbes's question—"Is it possible to overcome the passion of intellectual vanity and the historical tendency of the human mind to trust in the words and wisdom of authority?" (p. 96)—was answered with a resounding *yes*. And his success in undermining such authority marks the beginning of the illegitimacy of the modern age.

Kraynak offers a careful reading of Hobbes interwoven, as the book proceeds, with an Aristotelian counterpoint at times overtly incredulous at the audacity of Hobbes's project and, by extension, the modern project. Chapter 5 is particularly unforgiving of Hobbes, exposing, as it does, his supposition about the self-reliant mind and its method of introspection. Dialectic gives way to introspection. The search for reality gives way to an inquiry into states of mind and their cause. "Still protected," however, "by the philosophic and academic traditions of the West, [which hold] that a science of nature and natural justice is possible" (p. 197), the modern mind does not yet become the "existential mind." However, having dissevered itself from the tradition, the latter is but a short step away. The self-reliant mind is a fiction that can end only in the subjective morass of solipsism, notwithstanding its initial posture, Kraynak seems to intimate.

All authority now suspect, sovereign power must serve the function of arbiter (p. 171), of arresting vanity and the claims of authoritative wisdom. This antidote to doctrinal politics is purchased, however, at a high price. It violates the classical insight that "the 'cave' of the political community—its authoritative opinions and traditions fashioned by the poets, priests, and legislators—could be transcended only by the privileged few philosophers" (p. 188). Now, in effect, not only are the few who can bear the absence of authoritative opinion allowed to do so (the ancient insight) but everyone is forced to live in accordance with this verity. Indeed, the structure of political life, the need for a sovereign who is merely an arbiter in a world bereft of authority, reflects it.

The conventional wisdom about Hobbes, of course, is that he (perhaps along with Machiavelli) inaugurates modern political realism. Yet Kraynak would turn this view on its head. Because Hobbes disregards "the powerful human tendency" to trust in authority (p. 130), he is *less* realistic than were Aristotle and the scholastics. They at least knew that the human mind is not self-reliant, that it must trust in the authority of others. Because this tendency is a permanent feature of the human condition, Kraynak seems to intimate, Hobbes's edifice, the modern edifice, is built upon a foundation of sand.

Several objections do come to mind. First, it is to be wondered how the intellectual vanity Hobbes thought so necessary to combat may be overcome by a kind of resolute acceptance of the tendency to trust authority. Only human beings, Hobbes reminds us, have the privilege of absurdity—the privilege of going awry that possessing language bestows. Whatever may be said of the theoretical inadequacy of Hobbes's formulations, the suspicion against intellectual vanity has been burned into the modern mind on his account, and this has been for the good. Would Kraynak want us to return to the scholastic debates that had human authority on their side, if nothing else? To be sure, Kraynak is not oblivious to this problem and so countenances a kind of humility at the end of his book (p. 216). But the question remains whether the temptation of vanity can be vitiated without, as it were, the necessary illusion that "knowledge" based upon the authority of the one propounding ought rather to be called *belief*. Human beings are prideful creatures, as Hobbes over and over again reminds us. The pretensions of wisdom ought to raise suspicion not because wisdom does not exist but because while it can raise us almost to the level of the angels (following Locke), it can also lower us to a level below the beasts.

Second, the question needs to be posed whether *civil* history is the proper horizon within which to view Hobbes's thinking. History is crucial—this is Kraynak's important contribution; but Hobbes places civil history within the larger horizon of *biblical* history. Reason can know the former; only revelation illuminates the latter. Reason may conclude that there is a need for an antidote to doctrinal politics; but revelation confirms that the right pattern of political association is the Mosaic covenant wherein the sovereign has the right to command obedience and the right to interpret—attributes that the Leviathan, not incidentally, possesses. Reason and revelation, as Hobbes insists, do not conflict. By focusing only upon reason and the civil horizon of history within its purview, only half of the picture emerges. Hence, the embarrassment of the attributes of the Leviathan cannot be satisfactorily accounted for.

In other words, like Strauss, Kraynak understands Hobbes's edifice to be operating without superhuman support (p. 179), without revelation. The historian in Kraynak has made an important contribution to our understanding of Hobbes. The criticism to be made is that in the end, he is not enough of an historian, does not accord biblical history its due. Hobbes's science may be fully illuminated without recurring to biblical history, but his politics may not.

George Washington University JOSHUA MITCHELL

The Political Philosophy of Montaigne. By David Lewis Schaefer. Ithaca: Cornell University Press, 1990. 407p. $41.50 cloth, $13.95 paper.

All praise to the scholar who in his youth resists the enduring attractions and current temptations to publish and persists with his studies and rewritings until they and he reach a maturity from which the world of serious inquirers will benefit! This volume is the fruit of David Schaefer's long consideration of Montaigne. For years, in essays, panels, and other scholarly endeavors, he has argued for a serious consideration of the thought of Montaigne—its consistency, its power, and its pertinence to the foundation of the modern liberal order (including the state), to the academic political science practiced in the liberal university that is the image of that state, and to that suburb of the university called the French department. Schaefer's book presents to Montaigne studies a Montaigne that is more political and teacherly than private and self-savoring. To political science it thrusts forward a Montaigne who is the

cunning consistent founder of liberal modernity. To the diverse and divided university of the present day, it may disclose a thinker whose thoughts are too unified to fit in any one department and too significant to be dubbed interdisciplinary.

It could not be these good things without being both exceedingly detailed and somewhat comprehensive. Since Montaigne is divers, wavelike, and savory to the point of seeming casual, inconsistent, and even self-indulgent, the majority of the book is devoted to detailed, careful, even suspicious readings of important chapters from the *Essais* (esp. 1.23, 31; 2.11–12; 3.1, 4 & 9—with by far the greatest attention paid to 2.12. "The Apology for Raymond Sebond," which previous generations took to be central to Montaigne's achievement and understood to be skeptical (e.g. Sainte-Beuve). Schaefer's fine readings should be read along with the chapters, slowly. Thus, the book includes a careful treading of Montaigne's footpaths, by stile and gate, and an attempt to see his whole design, as if in one high-altitude topographic survey. In the latter, which concludes the readings, Schaefer emphasizes the "intermediary" service of Montaigne between founder Machiavelli and his liberal tamers (Locke, etc.). Here Schaefer hangs from the lion and fox of Machiavelli studies, Leo Strauss and Harvey Mansfield.

Naturally, such an ambitious book and its achievements raise questions for the reader, the author, and the rereader (for the book is worthy of such). Granting for a moment that Montaigne teaches covertly, why ignore the amiable face of the teacher? Isn't the secret of the depth in the surface as well? And granting that what Montaigne teaches tends to found something like the liberal order (which I am partly disposed to believe by my own researches), did not Montaigne teach it, as well, by the liberal, free, gentle self he presents the unsuspecting reader? When aiming at one goal, isn't it best to arrive by divers means? By argument and by example? In any case, can there be a good book without examples or a great book on human things without some great examples?

Is Montaigne such? Of all authors of great books, he is perhaps the one readers would most like to meet. Is he trustworthy, as well? Who is Montaigne? The characterization of Montaigne as Machiavelli's student (and esteemer of Tiberius) implies that he might not be one who has suffered. Certainly, Pascal might agree: not recognizing suffering or joy, the liberal man is overcome by the one and cannot rise to the other. Tocqueville might add that without the virtues it disdains to inculcate, liberalism will give way to tyranny. What would Montaigne reply? When he acknowledges himself no Socrates, (let alone Christ), Montaigne certainly accords with Machiavelli's lowering of the standards—but not wholeheartedly, for Socrates (at least part of him) shines in the *Essais* as never in the *Prince* or *Discourses*. Could it be that Montaigne differs quite a bit from the unarmed Machiavelli whose single bite at once maddens the clever and arms them to the teeth?

It is the merit of this book that it provokes such questions (and many others). This worthy fruit of the author's long consideration of Montaigne, along with the author's honest acknowledgment that many things baffle him utterly, or elude his firm grasp is the strongest reason to look forward to his further consideration of Montaigne—the generous founder of an order that cannot acknowledge him but whose citizens will always benefit from reading him thoughtfully. David Schaefer is a member of the liberal university who makes it surpass the liberal order in richness, nobility, and intellectual honesty—which it must do if that order is to retain its characteristic virtue, liberty.

Dartmouth College MICHAEL PLATT

The Limits of Government: An Essay on the Public Goods Argument. By David Schmidtz. Boulder: Westview, 1990. 197p. $38.50 cloth, $12.95 paper.

Why must governments exist? The common philosophical response to this question is some variant on the public goods argument. A public good, approximately, "is a good that can be produced only by collective action, but [whose] production benefits people regardless of whether they join in the collective effort" (p. 1). All profit from passable streets and secure borders; government exists to ensure that all likewise pay. If streets and the common defense were left to the market, they would probably not be produced. If their funding depended on voluntary action, some would presumably contribute, but others would take the free ride. Thus, coercion compelling participation in the production of public goods is essential—hence, the state. It is "not that people consent to government, but rather that government does something that needs doing" (p. 10).

David Schmidtz has produced a literate and at times charming essay on the (rather deep) limits of the public goods argument. Successive chapters deal with the legitimacy of property and property rights and the famous Lockean Proviso, the emergence of the right to punish, various versions of the prisoner's dilemma as applied to the public goods argument, and a theory of morality (enshrining reciprocity as the highest moral virtue) in a cooperative society.

Coercion is required for the production of public goods—hence, the state is necessary—only under two conditions: (1) if the good is nonexclusionary (all may partake whether they contributed to its production or not), then some would choose not to contribute because they can enjoy the good for free; (2) some essential goods may be hopelessly underfunded and thus never produced if their production relied on voluntary contributions. This is known as the *assurance* problem. If a noncoercive solution to the free-rider and assurance problems could be found, then the public goods argument in behalf of the state would fail. In the language of game theory (and of this book), this amounts to defining a prisoner's dilemma where cooperation is a dominant strategy.

Schmidtz argues that assurance contracts can solve the assurance problem. In essence, the assurance contract is a money-back guarantee. Contributors get their donations back if the total donations are insufficient to provide the good. Evidence from experimental economics tends to show that voluntary contributions to the collective good increase under assurance contract conditions (chap. 6). The free-rider problem is less easily solved, owing mainly to the difficulty of distinguishing between free-riders and "honest holdouts" (people who do not value, or would not profit from, a particular public good): "The real point of coercion is to force even honest holdouts to pay" (p. 84). And therein lies the legitimation rub: By what right does government expro-

priate the property of honest holdouts or (what amounts to the same thing) force people to buy that which they do not value?

Thus, the public goods argument is unsound: "Its descriptive premise is that coercive public goods production makes each person better off from the person's own perspective. The argument's normative premise is that when coercion does this, coercion is permissible. The descriptive premise is, by and large, false"—precisely because honest holdouts in fact exist (p. 84).

The problem with the coercive production of public goods, of course, is that overproduction is stimulated (as opposed to the underproduction characteristic of a free-market situation). Coercers can (and do) finance their own projects "without regard to whether the income they are spending is their own" (p. 89). There is evidently no limit to how costly overproduction can be, at least not if the recent history of public spending is taken seriously; but there is a definite limit to how costly underproduction can be, "for the worst that can happen is that the good is not produced at all" (p. 90).

The concluding chapter, "The Morality of a Cooperative Society," is an effort to develop a more adequate normative basis for the public goods argument. Not belaboring the details, a reworked version of the public goods argument "can justify more than a minimal state, perhaps, but not a great deal more. The justification of big government requires a different kind of argument" (p. 160).

The public goods argument lies at the heart of much contemporary political theory. *The Limits of Government* is a sustained and persuasive attack and is thus required reading for theorists throughout the social sciences.

Tulane University JAMES D. WRIGHT

Reason and Democracy. By Thomas A. Spragens, Jr. Durham: Duke University Press, 1990. 281p. $39.50 cloth, $17.95 paper.

In *The Irony of Liberal Reason* (1981), Thomas Spragens charted the various miscalculations and subsequent disappointments that attended the early marriage of liberal hopes to the epistemological projects of Descartes and Locke. The irony was that because liberals expected too much of reason, they were forced to settle for less (e.g., instrumental rationality and value noncognitivism). Lacking any philosophical resources for countering the equation of liberal reason with a calculative ideal, liberalism seemed well suited to one or another form of power politics but ill suited to any more generous vision of politics. *Reason and Democracy* shows us that we can and should do better. It is an argument presented in two parts. The first part briefly retells how postpositivist developments in the philosophy of science and language enable us to recapture the pre-Weberian belief that rationality can be used as the standard of a good society. The second and larger part of *Reason and Democracy* aims at showing us how the values and ideals of human reason conceived as a certain type of praxis provide us with a clearer statement of the importance and meaning of liberal democracy.

Spragens's account of how reason came to be reduced to a model of means–ends calculation and his critique of that model will be familiar to those who have either read his earlier work or been following the arguments for more contextual (practical/communicative/discursive) conceptions of reason. As Spragens reads these developments, they essentially involve a shift away from the notion of reason as a logic and technique of neutral observation and toward an appreciation of reason as a formative process of inquiry that constitutes an act of "collective prudence" or judgment. Recognizing that rationality itself has a practical dimension "enlarges the range of rationality, jettisons demands for and claims of certitude as a hallmark of reason, shifts the focus of norms of rationality from content to process, and substitutes a paradigm of a communal dialogue for the paradigm of a logical soliloquy" (p. 110). Publicity, communication, fairness, toleration, rights of participation, duties of veracity and responsibility, the discipline of reasoned discourse: each plays a role in science understood as a rational enterprise. The bulk of *Reason and Democracy* deploys these norms of rational practice in order to characterize and justify liberal democracy. It is Spragens's contention that democracy is at its best when it most resembles human reason at its best: "If we had to choose a single normative standard for the understanding and evaluation of liberal democratic political systems, the one that would get us the farthest would be neither liberty, nor fairness, nor neutrality, nor utility, nor pluralist bargaining, but would instead be the ideal of rational practice" (p. 255).

This contextual turn, in which standards of good practice are uncovered in some activity when it is done well is a familiar pragmatic move in contemporary political theory. Charles Anderson's *Pragmatic Liberalism* (1990) argues that pragmatism gives liberalism a method and that liberalism gives pragmatism purpose. Benjamin Barber's *Strong Democracy* (1984) echoes John Dewey in its thesis that political rationality is best understood as a type of social intelligence that is expressed in democratic processes of common deliberation. *Reason and Democracy* is consistent with these and similar works, but it is in some ways a more ambitious book. Spragens uses the ideal of a rational society to measure the contributions of liberal, communitarian, republican, and other theories to the project of democratic rationalism. There is room in his account of reason and democracy for liberal tolerance, a constrained common good, strong democratic talk, and a modified range of republican virtues. Since one is more used to theories that seek to defeat, rather than reconcile, competing doctrines, it is refreshing to follow Spragens as he looks for what is right in the work of various authors. But this generosity is not without its price. Controversies are suppressed, and differences smoothed out, in ways that permit Spragens to talk, for example, about the contributions of both liberals and communitarians to the ideal of a rational society. But if these differences reveal something about politics that we might otherwise miss, then we might not want a theory sufficiently capacious to accommodate both. At times, *Reason and Democracy* is too roomy. Conceived as a rational enterprise, the good society is "a complex form of social practice that protects rights but does not enshrine them in a vacuum, that protects and promotes equality but is not mindlessly egalitarian, that seeks justice but does not abstract it from the common good" (p. 238). Statements such as this leave many lines undrawn and what might count as a rational balance unclear.

Reason and Democracy could be read as a caution against the demand that reason reach as far as my last criticism implies that it should. Spragens might fairly

remind his readers that it is precisely because liberals once mistakenly imagined that reason could make all of the difference in politics that we lost sight of the difference that it could make. Because it so clearly recaptures a political and moral role for reason in liberal societies, this book is a worthy sequel to Spragens's earlier work.

University of Florida ALFONSO J. DAMICO

The Idea of Political Theory: Reflections on the Self in Political Time and Place. By Tracy B. Strong. Notre Dame: University of Notre Dame Press, 1990. 196p. $28.95.

Any book with a title such as this one risks disappointment, in part owing to its big promises, in part owing to the difficulty of saying anything new on a topic so thoroughly surveyed and debated. But Strong's book is remarkably fresh; he reformulates traditional questions by looking at the ways they locate and constitute the self in the several kinds of worlds—aesthetic, moral, theoretical, political, and linguistic. This allows Strong to ask a traditional question—What is "the political" such that it could be an object of theory?—without providing a traditional answer. Traditional answers fail us not because they are "wrong" but because they take for granted precisely what we now experience as most problematic—a self, a personal identity that is intact and autonomous. "The political," Strong argues, is "that form of human activity in which the answer to the question 'who am I?' is also the answer to the question 'who are we?' " (p. 34). This way of putting the issue is perhaps deceptively simple. It is not, for example, to ask how we might reconcile differences to pursue common projects. Rather, it is to ask what kinds of problems map the terrain within which the "I" and we" are simultaneously constituted, an approach that owes more to Nietzsche and Wittgenstein than to Locke and Hobbes.

Most of Strong's book examines the implications of viewing "the political" in this way. Most importantly, a political terrain comes into being intersubjectively, through human interactions. This alone means that politics lacks qualities that we might wish it to have. It is imperfect, not entirely controllable, and does not admit of certainty. And yet we are always tempted to believe otherwise just because perfection, control, and certainty are necessary to self-identity. Yet if we give ourselves over to these temptations, we in effect cut ourselves off from the "we" and produce for ourselves a world with fewer possibilities and experiences.

One of the more remarkable aspects of Strong's book is that he shows how these insights are embedded in the canon. Augustine's view of the self as constituted by *will, vision,* and *memory,* for example, provides Strong with a way of analyzing three kinds of antipolitical temptations—topics of the middle three chapters. Vision is future-oriented, motivated by a desire for perfection and represented in its purest form by dramatic art (chap. 2). The will is present-oriented, seeking control over the "otherness" of the world and represented in science (chap. 3). Memory is oriented toward the past and tempted by the identities of "truth" that would, in effect, relieve the self of its dependence on contingencies and securing its identity in a permanent order (chap. 4).

Much of Strong's argument is devoted to these "temptations." On the one hand, they typify approaches to political theory that seek secure identities for the self. They do so, however, at the cost of obscuring political dimensions of existence, thus depriving humans of awareness of, and responsibility for, the political dimensions of their experience. On the other hand, politics can and must invoke these identities since "the political" draws on the problems and relations that these identities locate. It is precisely this overdetermination that makes politics a realm of contingency and resistent to metanarratives. These several kinds of relations exist in tension, yet each is necessary to constituting persons. This is why political theories that seek to ground themselves in any one of these realms become partial and lose their ability to provide a political articulation of the self.

If we are to understand political theory in a way that articulates our postmodern condition—one that Strong describes as an interrelated crisis of personal identity and political authority—then it must be about "boundary maintenance" between these necessary but irreconcilable realms (p. 2). Without holding these realms in tension, without allowing each its particular "mode of validity," the self is dispossessed of the situated identities through which it can become a political agent. Political theory's mode of validity consists in articulating these relationships while holding them in tension. One implication of this approach (to which Strong devotes chap. 5) is that the authority of political theory must be parochial, that is, relative to time and place. As to the problem of ethical relativism that seems to follow, Strong notes that the problem is conceived in a way that cannot produce a politically intelligible answer: universal commitments have a purchase on action only when selves are already constituted as moral agents by their relations to the "we." And this is a political, not an ethical, problem.

There are things one could quibble with in this book. For example, the catalog of "temptations" that constitute the self seems somewhat contrived or perhaps incomplete. For example, although Strong discusses religious, moral, and linguistic identity, they do not figure into the temporal framework of self-constitution that he borrows from Augustine. In addition, some of Strong's claims to originality depend on ungenerous readings of approaches to which he is indebted, such as phenomenology and hermeneutics. And sometimes the argument is awkward in ways that it need not be. For example, a little reading in Habermas—a master of arguments about distinctive modes of validity—would have saved a strained attempt to explain why political theory cannot be "true," but only "truthful" in articulating relations between "I" and "we" (pp. 118–20). The point is well taken but comes across badly because Strong uses *truthfulness* to cover two related but distinct modes of validity, namely, sincerity and legitimacy—in this way collapsing reflexive validity with respect to the self and acknowledged validity with respect to others.

Nonetheless, this is an important book. One has a sense that an approach to political theory that has been developing in bits and pieces for several generations (one might call it "critical postmodernism" even though Strong does not use the term) is now catching its stride. Strong's book speaks in a confident voice devoid of jargon. It puts forward positive, not merely critical, claims and visions. It is devoid of the whiny nostalgia for past paradigms—paradigms appropriate to other times and places—that mars so much contemporary political theory. And Strong convinces us that critically postmod-

ern approaches can help us attend to a broader range of experiences, can induce a deeper spirit, and can provide more appropriate responses to suffering and contingency than their fading alternatives.

Georgetown University MARK WARREN

Moral Foundations of Constitutional Thought: Current Problems, Augustinian Prospects. By Graham Walker. Princeton: Princeton University Press, 1990. 189p. $25.00.

This book presents a normative critique of recent constitutional scholarship. It might best be described as a "second-level" analysis; that is, Walker has far more to say about those who write about the Constitution than about the Constitution itself. Indeed, he succeeds in writing a book about the Constitution without even mentioning a single decision by the Supreme Court or any other court.

The author's thesis is that constitutional scholarship has reached "normative impasses" grounded in the various philosophical foundations of the leading writers and that a fresh perspective is needed if we are to make any progress in "clarifying the fundamental and inescapable questions confronting constitutional thought" (p. 5.) Ambitiously but with considerable skill, Walker reaches back some fifteen hundred years to Augustine to provide this fresh approach.

The argument of the book is divided into two parts. First, the author explains the normative impasses of contemporary constitutional scholarship by dividing the field into four quadrants created by a horizontal axis separating teleologists from deontologists and then bisecting that axis with another that divides realists from conventionalists. A moral realist for Walker is one who "regards the good, whatever it may be, as a reality whose existence is independent of human artifice." Conventionalists affirm that the good is "a contingent human artifact" (p. 24).

With his quadrants established, Walker distributes prominent authors to their assigned places: Sotirios Barber and John Courtney Murray to teleological realism, Walter Berns to conventional teleology, William Rehnquist and Michael Perry to conventional deontology, and Ronald Dworkin and David A. J. Richards to realist deontology. One can only imagine the howls of protest and the shrieks of delight that will greet these placements. Surprises await the Senate Judiciary Committee when it discovers Laurence Tribe and Robert Bork sharing the rather cramped quarters of conventional deontology along with Rehnquist, Perry, Lief Carter, John Hart Ely, and Rogers Smith.

In what is surely the best part of the book, Walker develops a powerful argument against all conventionalists by showing how their position can ultimately be reduced to a "nihilist skepticism" that undermines serious moral analysis. One might question whether Walker has the right players in the right slots, but his argument against conventionalism is clear and unerring.

His attack on the realists is more problematic. Walker maintains that realists threaten to reduce law to morality. Because they have a substantive notion of the good, he maintains, they have no principled way to confine the sphere of law: "The deep logic of contemporary moral realism seems to suggest that law is coextensive with morality" (p. 59). All this is problematic because Walker fails to recognize the principled limitations on law in the jurisprudence of the prince of realists, Thomas Aquinas, and such twentieth century Thomists as the late John Courtney Murray, whose work Walker misconstrues.

Problematic or not, however, the criticism of the realists brings Walker to the second major point in his argument, namely, that Augustine comes to the rescue. Although Augustine is clearly a realist, his belief in man's fallen nature considerably tempers his expectation of improving human affairs by law. For Augustine the institution of government is itself the result of sin and, therefore, Augustine is in no danger of succumbing to what Walker sees as the realist's temptation to achieve authentic moral perfection by law.

Walker's presentation, serious and thoughtful throughout, is often persuasive and at times convincing. Yet this fine book is not without its shortcomings. The primary problem is Walker's somewhat confusing treatment of the theological basis of his argument. He tells us we need not share Augustine's faith in order to learn from his teachings on law but fails in his effort to disengage Augustinian political science from the faith of Augustine. As a matter of fact, Walker dedicates some 30 pages to a concise summary of Augustinian theology. With a polite bow to secular orthodoxy, Walker graciously grants the reader permission to pass over these pages; yet clearly, he hopes his readers will not avail themselves of his liberality.

The heart of the Augustinian relevance to Walker's argument is Augustine's clear-headed view of man's imperfection that keeps him from looking to law to perfect man morally. Walker assures the reader that for all his hard-headedness, Augustine is no cynic. This is surely correct, but only because of Augustine's faith. No serious Christian can be a cynic, and Augustine is a very serious Christian. In his unconvincing effort to uncouple Augustinian jurisprudence from the faith of the saint, Walker would give us *Hamlet* without the Prince of Denmark.

Virginia Polytechnic Institute JOHN A. ROHR

After Ideology: Recovering the Spiritual Foundations of Freedom. By David Walsh. San Francisco: Harper, 1990. 296p. $24.95.

As communism collapses and Marxism–Leninism is repudiated around the world, we have the sense that a distinct period of history has come to a close. Some may call it the age of totalitarianism and trace its course in the twentieth century through the rise and fall of fascism and communism. Others may look at the period from 1789 to 1989 and see the series of convulsions as a great experiment in political utopianism. Others may go even further and view the entire modern period from the Renaissance to the present as a completed event, with the twenty-first century marking the end of modernity and the dawn of the postmodern age. However the lines are drawn, one cannot deny the need for an explanation of these momentous events, as well as a moral compass to guide us through the next century or millenium.

While most political scientists might feel overwhelmed by such intellectual challenges, we can be grateful that David Walsh has not shied away from

them. In his new book, Walsh attempts to understand the whole modern age as a dramatic spiritual crisis and to propose a solution. His thesis is that modernity is an age of ideology, driven by a misguided revolt against God; his solution is a carefully balanced synthesis of freedom and Christian faith.

Following the lead of Eric Voegelin, he traces the roots of modernity to a change that occurred within Christianity. Somewhere in the late Middle Ages, the "gnostic" spirit of blaming God for the fallenness of the world overtook certain Christian thinkers (beginning with Joachim of Fiore) and led them to seek the realization of the Kingdom of God in this world and eventually the reformation of the fallen world by purely human effort (pp. 99–116). From this spirit, the ideologies of the modern world were born. They are secularized or deformed versions of Christianity, driven by rebellion against God and the God-given limits of the natural order. They are captivated by visions of realizing heaven on earth (among other things the end of suffering and the perfectly harmonious society). Whatever form they may take—socialism, communism, fascism, genetic engineering, Freudian psychology—ideologies are intellectual constructs whose purpose is to transform human nature and conquer the natural world (pp. 10–12). In Walsh's judgment, the effort to remake the world through ideologies has been more of a nightmare than a pleasant dream; and the manifest failures have led to their demise in the present day.

While this analysis and critique is not new, Walsh develops it in a way that is fresh and in many respects profound. He explores the inner logic of the modern crisis of ideology by using what he calls the "existential" or "experiential" insight of four unusual figures: Camus, Dostoyevski, Solzhenitsyn, and Voegelin. Although the selection of these four may seem idiosyncratic, Walsh justifies his choices with the claim that each felt the modern spirit of revolt against God and overcame it through a kind of personal cleansing or purification of the soul—an actual religious conversion to Christianity or an acceptance of the need for God.

To support his claim, Walsh presents detailed analyses of the literary and philosophical works of each author. Camus is undoubtedly the most difficult to fit into this scheme; but Walsh makes a plausible case that Camus's novels *The Plague*, *The Rebel*, and *The Fall* are the works of a man anguished by the theological problem of innocent suffering and longing for an affirmation of the goodness of life—a quasi-Christian existentialism that yearns for redemptive grace without ever receiving it (pp. 142–48). With less straining, Walsh presents Dostoyevski and Solzhenitsyn as men who went through socialist phases in their lives and truly overcame the revolt against God. The most delicate question in both cases is whether they actually embraced the Christian faith—experienced real conversions—or merely equated the profound need for God in their lives with the real existence of God. To Walsh's credit, he presents the evidence as fairly as possible (pp. 148–84); but I am not sure how close one can get to understanding these tortured Russian souls! In the case of Voegelin, the situation is also complicated. Walsh quotes Voegelin's statement, "History is Christ written in large letters" but also notes Voegelin's "distinct coolness toward Christianity," implying that Voegelin's interest in Christ is that of an intellectual historian rather than that of a heartfelt believer (p. 201).

Despite the complicated nature of each thinker's relation to Christianity, the overcoming of the modern spirit of revolt in each case permits Walsh to draw lessons for modern politics. His conclusion is that the modern idea of freedom embodied in liberalism and constitutional government is salvageable from the wreckage of ideologies if it is established on Christian principles. In other words, liberalism is not just another ideology, because it has no inherent metaphysical grounding or messianic pretenses. Its notion of free individuals responsible for their actions does not require the doctrine of rights or even a metaphysical teaching about freedom of the will. Rather, Walsh argues, its best grounding is Christianity, properly understood.

But here is where Walsh fails to satisfy the critical reader. What does he mean by Christianity, and why is he so sure that it is compatible with liberal freedom? Walsh makes a very interesting case that Solzhenitsyn's Christianity is actually compatible with a certain kind of constitutional monarchy and local *zemstoy* or citizen assemblies (p. 232). On the other hand, Walsh fails to convince that Dostoyevski's faith and politics are compatible with western liberalism (pp. 211–12). Walsh's own position remains unclear, because he speaks repeatedly of "philosophic Christianity," which sounds like Thomism, and of a "Christian philosophy of existence," which could be either Augustinianism or the phenomenological Christianity of recent Catholic thinkers such as Henri de Lubac (pp. 4–7, 212–28). Since Walsh denies that he is in any way a reactionary, one may infer that he is not promoting a Thomist or Augustinian revival. The statement of his political program bears this out (pp. 272–78); for it closely resembles the position of that great nineteenth-century synthesizer of Christianity and liberal democracy, Alexis de Tocqueville. Thus, through a fascinating tour of four strange and mysterious human souls, Walsh has led us back to familiar institutions but with new insight about how they might be grounded in the experience of transcendent order.

Colgate University ROBERT P. KRAYNAK

The Politics of Being: The Political Thought of Martin Heidegger. By Richard Wolin. New York: Columbia University Press, 1990. 231p. $34.00.

As the intense debates—first in France, then in Germany—showed, the revelations about Heidegger's deep involvement with the Nazi movement published by (the often unreliable) Victor Farias and, more significantly, Hugo Ott, made it imperative that Heidegger's work be reread with a sharper eye for its political dimensions.

Richard Wolin, undoubtedly inspired by his earlier work on the French Heidegger debate, followed this imperative. The result is a very lucidly written and persuasively argued brief for the claim that Heidegger's involvement with National Socialism "was rooted in the innermost tendencies of his thought" (p. 66). Implicitly, Wolin thus argues against Richard Rorty's conviction that Heidegger's politics is explained purely in terms of his "pretty nasty character." Rorty's judgment that the relation between a writer's books and his life are purely contingent is shown simply not to stand up against overwhelming contrary evidence. In the stringency of

his thought Wolin surpasses even Habermas. The latter, in an attempt to salvage the "autonomy of thought" of *Being and Time*, a book that had enthralled him in his own youth, claims that after 1929 a "process of ideologizing" or *Verweltanschaulichung* of Heidegger's pure theory destroyed its initial integrity (*Texte und Kontexte* [1991], pp. 58, 60). Wolin disputes Habermas' contention that Heidegger is wrenched by an internal struggle between pure philosophy and the popular German conservative ideology of cultural decline. Instead, Wolin can show that from the start, Heidegger's most fundamental philosophical premisses issued necessarily in his peculiar reading of National Socialism. Consequently, Wolin's basic argument generally is in agreement with the writings of Otto Poeggeler, Alexander Schwan, and Karl Loewith.

But Wolin's argumentation is very much his own and offers some new insights. He begins with two facts: that Heidegger himself claimed an "essential" relationship between his philosophy and National Socialism and that this was true despite the absence in his work of an articulated practical philosophy. The answer lies in Heidegger's philosophical attempt to eliminate altogether the traditional division between theoretical and practical reason. Heidegger accomplishes this, however, by essentially equating basic ontology with history and politics. Ontology, or the history of the dispensation of being, issues in a kind of "politics of being." In the process, politics and history as more or less independent realms perpetrated by more or less free and responsible human agents entirely disappears, as does the traditional concept of "political judgment." Certainly, Heidegger himself, with his totalizing, politically indiscriminate visions of being, showed no political judgment.

A few stages in Wolin's interpretation merit special attention. He convincingly shows that the "pessimistic" philosophical anthropology of *Being and Time* and the ubiquity of existential fear and death easily merged with the deeply conservative revolutionary worldview of the German *mandarin intelligentsia* (Fritz Ringer's term) of the 1920s. Heidegger's predilection for Oswald Spengler's popular theme of the decline of the West, as well as his extensive preoccupation with the work of Ernst Jünger prepared him for his interpretation of Hitler and National Socialism. The latter, as Luc Ferry and Alain Renaut emphasized, was to provide Heidegger with the antimodern response to the challenge of global technology allegedly not provided by modern democracy. Heidegger's self-admitted "big blunder" was to join a movement that did not live up to the momentous historical task he had assigned it. Thus, Heidegger never relinquished his conviction of the "inner truth and greatness" of National Socialism even in the face of Auschwitz, the holocaust, and the other evils it perpetrated.

Another point Wolin makes is of even greater significance. It had been argued by both Habermas and Leo Strauss that Heidegger's practical failure to become the "führer of the Führer" had not only led him to his preoccupation with Nietzsche and the notion of Western history as the unfolding of nihilism but, finally, to history as the fateful dispensation of being itself. Wolin agrees that this step led to the total elimination of human agency and politics and to the fateful equation of truth with the disclosure of sheer temporality. But in addition, Wolin shows that this acceptance of "existential criteria" for political truth is profoundly exacerbated by Heidegger's "equiprimordiality of truth and error" (p. 118). Ultimately, that is, Wolin traces Heidegger's incapacity for political judgment to his incapacity philosophically to distinguish truth from untruth. Briefly, Heidegger's notion of truth lying in the unconcealment of being contains a necessary concealment in the act of unconcealment. Thus, the errancy resulting from the partial concealment of being is part of the primordial essence of truth as unconcealment; and thus, truth disables us ever to escape errancy or to distinguish the two. Consequently, Heidegger always rejected the notion of an ethics. His notion of truth and its necessary errancy also made nonsense of his attempt personally to mythologize himself as the fated voice of being.

Lastly, Wolin's otherwise admirable treatment of the subject suffers from one internal inconsistency. He begins his study with a sense of caution: he only wants to focus on Heidegger's political philosophy, not his philosophy per se. He is not persuaded that the results of his study will transfer to Heidegger's essential philosophy except in a "highly qualified and tentative fashion" (p. 15). But as Wolin gets going, it becomes increasingly clear that he is challenging the most quintessential elements of Heidegger's thought—like his fundamental notion of truth as unconcealment. What, if not his concept of truth, is Heidegger's philosophy per se? Wolin ends his book by speaking of the "bankruptcy" of Heidegger's later thought. He even doubts "the underlying cogency of his conceptual framework in its entirety" (pp. 168–69). And he agrees that Heidegger "betrayed" philosophy. Wolin's proving considerably more than he planned unfortunately leaves the relation between a "first philosophy" and practical politics in a state of confusion. Nevertheless, I have been persuaded by Wolin to believe Karl Jasper's early dictum that Heidegger's way of thinking is "essentially unfree, dictatorial and uncommunicative."

University of Colorado, Boulder HORST MEWES

Justice and the Politics of Difference. By Iris Marion Young. Princeton: Princeton University Press, 1990. 286p. $45.00 cloth, $10.95, paper.

The universe John Rawls created has once again expanded. This study by Iris Young brings together a series of essays that have as their common focal point the attempt to transcend the distributive paradigm of justice most famously associated with Rawls's theory of justice. Starting with a critique of that paradigm, Young advances a theory of oppression—which she then expands upon in chapters on welfare capitalism, the biases inherent in the idea and practice of neutrality, the discipline of bodies, difference feminism, affirmative action, city life, and (finally) international justice.

Young's study has the great merit of being simultaneously lucid and politically passionate. Attempting to confront the theory of justice with the harsh realities of the ongoing injustice wrought by the systemic inequalities of the late capitalist world order, she is relentless in her pursuit of the ideal of justice. She is also conversant with the most recent developments in contemporary critical and poststructuralist theories, making good use, for instance, of insights from Foucault to refute the

notion of power as a distributed good and Kristeva's idea of abjection to advance an explanation for racism, sexism, and homophobia.

Young moves between the worlds of poststructuralism and critical theory smoothly, and she is consistent in her effort to make political theory useful for the expansion of just possibilities. The book's merits are many; yet, in the end, I was troubled by it. Although it qualifies even its strongest conclusions regarding the need to remedy unjust situations, at its core, this book—by virtue of its very power as a statement concerning the extent of injustice—gives expression to the limits of the idea of justice as a political end in the contemporary world.

I do not wish to condemn or dismiss what seems to me to be an excellent work of scholarship and care, and I know that many of the few questions I can raise go beyond the scope of the book; yet it seems to me that in *Justice and the Politics of Difference*, Young has inadvertently closed, rather than opened, debate concerning justice and difference by defining injustice in such a monolithic way.

Her pivotal chapter is entitled "The Five Faces of Oppression." Arguing that it is necessary to develop a theory focusing on how the contingencies of group identity may engender oppressions, Young makes a key leap. Taking issue with the notion that to treat people as members of groups, not as individuals, is the best way to advance justice, she suggests that "it is foolish to deny the reality of groups." (p. 47). She then, however, assumes that through state policies to address pernicious group distinctions, progress toward justice will be made. As malign as the neglect of rights has been in the United States and other capitalist societies, there is no reason to assume—and much reason to deny—that such an alternative approach would help lessen the group oppressions she identifies. Moreover, once Young gets down to specifics, minor and major injuries are practically indistinguishable. The accumulated effect of her analysis of the extent of injustice will be for her less-sympathetic readers, either despair or, more likely, dismissal.

For Young, the reach of injustice is so thorough and so multifarious that there is scarcely a person who does not suffer it. Of course, this may well be true. I have no difficulty in accepting the ubiquity of injustice in the world, but wonder which injustices may be remedied through the use of policy and which not. Young fails to ask this. More troubling, in this book rarely is a positive assertion regarding a goal of justice (e.g., the need for group participatory democracy) assessed in reference to the injustices and miseries that would accompany it. Indeed, despite her own cautions, Young embraces all sorts of intrusions by state power into the lives of people in order to ameliorate conditions that might better be left alone by such a form of power. Having embraced Foucault's discussion of power in her critique of liberals, she abandons it when it comes to her quasi-socialist program.

If the Rawlsian juggernaut is finally reaching its terminus, it may well be that *Justice and the Politics of Difference*—as resistant as it is toward Rawls and as powerfully as it asserts a need for difference within a community—may be one of the more interesting variations his work has inspired. Yet it may also provide reason for rethinking the politics of justice from other, perhaps more transgressive, perspectives. In the meantime, it will be interesting to see what Young does next. A mind so engaged will not rest where this work concludes.

Amherst College THOMAS L. DUMM

AMERICAN POLITICS

The Exemplary Presidency: Franklin D. Roosevelt and the American Political Tradition. By Philip Abbott. Amherst: University of Massachusetts Press, 1990. 233p. $35.00 cloth, $13.95 paper.

Looking Back on the Reagan Presidency. Edited by Larry Berman. Baltimore: Johns Hopkins University Press, 1990. 332p. $45.00 cloth, $13.95 paper.

The Bush Presidency: First Appraisals. Edited by Colin Campbell, S.J. and Bert A. Rockman. Chatham: Chatham House, 308p. $25.00 cloth, $16.95 paper.

The Nixon Presidency: Power and Politics in Turbulent Times. By Michael A. Genovese. Westport: Greenwood, 1990. 265p. $42.95.

These politically wide-ranging books cover most of the modern to the postmodern presidencies. Although the topics and approaches are diverse and numerous, the books have one thing in common: the quality of the scholarship is extraordinary. Abbott's book, *The Exemplary Presidency*, is by far the most profound and innovative not only of those reviewed but of the more recent scholarly literature. It provides a theoretical framework that elevates presidents to a higher level of historical interpretation and political thought than previously assumed by scholars.

For years, participants, students, and presidential scholars have consistently shown that Americans value brilliance of thought but that a deep and comprehensive philosophical outlook has escaped them. Abbott challenges this conceptual framework and, instead, asserts that the "American presidency as an institution . . . is in a fundamental sense exemplary" (p. x). To Abbott, presidential statements are models for political thought. Presidents are put in a position to articulate political philosophy, and they "do speak in the language of political philosophy" (p. 7). For these reasons, the presidency should be studied as a structure for the creation of political philosophy. From this vantage, Abbott attempts to demonstrate "how complex this process of imitation can be and . . . how much eloquence and profundity it requires" (p. x).

This philosophical approach has significance not only because it is rarely used but because, traditionally, presidents have centered their presidencies on the actions and thoughts of former presidents. As Abbott reaffirms, contemporary presidents carry with them "the successes and failures of past presidents as burdens to be born, or as opportunities to be explored" (p. 14). Furthermore, Abbott provides convincing evidence of how FDR constructed his political and philosophical outlook from those of Hamilton, Jefferson, Jackson, and Lincoln. These constructs (more vividly exemplified by Lincoln's) asserted that there was a core of beliefs that defined us as Americans and that through imagination and understanding of history, presidents have rallied their thoughts and policies around these core beliefs.

Despite the author's consistency in using these philosophical contructs, it seems he at times stretches Roosevelt's imagination to fit his conceptual framework. It is also difficult to believe that as a political pragmatist, FDR was sufficiently well-grounded in philosophical matter to appreciate and apply Hamiltonian and Jeffersonian political philosophy. These shortcomings do not prevent the author from providing scholars and students of the presidency with a research framework for years to come.

The political nature and modus operandi of Richard Milhous Nixon have inspired the production of a wealth of information on this controversial figure. One wonders why Michael A. Genovese has written yet another account of the power and politics of the Nixon presidency. Yet the author claims (with justification) that it is the least understood of modern administrations. In other words, there is no "comprehensive overview of the Nixon presidency" (p. 1). Accordingly, Genovese tries to piece together the many puzzles that characterize Nixon and his presidency. To unravel this mystery the author begins this well-researched study with a brief biographical sketch of Nixon's early years and his rise to political office followed by a study of Nixon's personality; style of decision making and management; and handling of domestic, economic, and foreign policy. He concludes with an examination of the dissolution of Nixon's presidency. Genovese concludes his study with "an analysis of the impact and legacy of Richard Nixon on the presidency and the United States and tries to place Nixon into a broader comparative perspective with other presidents" (p. xii).

Considering the hidden and obscure nature of Nixon's presidency, Genovese presents his strengths and weaknesses in an objective manner. Although I agree with the author's conclusion that "fundamentally, Nixon has no central philosphical beliefs," (p. 238), there are some contradictory characteristics of the Nixon presidency that fit Abbott's conceptual framework outlined in *The Exemplary Presidency*. Because of Nixon's psychological motivations (which included childhood poverty and provincial status), he relied on FDR-style tactical skills. More important, Nixon's political disposition is both Rooseveltian and Jacksonian. His distrust of the Eastern establishment, his hatred of power derived from privilege, and the concept of the "silent majority" are all fundamental elements of the Jacksonian political philosophy. In spite of the author's heroic efforts to provide an impartial analysis of Nixon's domestic, economic, and foreign policy, the tone of the book is strongly affected by Nixon's unique personality—ultimately, his "fatal flaw."

The last two books are edited volumes covering the two most recent presidents. *Looking Back on the Reagan Presidency* consists of a series of conference papers written by a group of distinguished scholars. These Brookings Institution conferees review the impact of the Reagan administration in five general areas: foreign policy, economic and fiscal policy, institutional changes, elections, and congressional relations. Although the authors have many divergent points of view, it seems clear that they are in agreement that the administration's impact includes "a detachment from policy; an ideologically derived commitment to specific policy priorities; and the captivating, disarming personality of the man himself" (p. ix).

The impact of the Reagan administration on both domestic and foreign policy seems stronger than anticipated. Moreover, his ability to continue to govern was based on what Thomas Mann calls "a protective rather

than an expansive domestic agenda after the first six months of his administration" (quoted p. 26). Yet there were failures. Considering the fiscal rhetoric of this conservative administration, the increase in size of the federal deficit and its impact on future generations proved to be quite embarrassing. Furthermore, his inability "to achieve a realignment with the Republicans as the new majority party in this country" (p. 28) was another unfulfilled goal. In foreign policy, the reaction is mixed. In "The Middle East," Robert Lieber argues, "there has been somewhat greater latitude for the use of military power and for unilateral action than appears to be the case in the late 1970's" (p. 51), while Robert Pastor concludes that against Reagan's policies in Latin America "the judgment will be harsh, and not just by critics" (p. 47).

A very important feature of this volume is the significant use of quantitative data to explain the impact of the Reagan legacy. Walter F. Murphy's "Reagan Judicial Strategy" and Gary C. Jacobson's "Meager Patrimony" illustrate the urgent need to use quantitative data in the study of presidential politics.

The Bush Presidency, edited by Colin Campbell and Bert A. Rockman literally provides the first serious assessment of the Bush administration. Like the Reagan study, *The Bush Presidency* is a comprehensive assessment of the administration's performance. Both deal with similar topics—foreign and domestics politics—and both involve a group of distinguished scholars. Nevertheless, *The Bush Presidency* uses a different approach and arrives at rather different conclusions. The authors "describe events that have been relevant to various aspects of the Bush Presidency" (p. xii), ranging from Bush's leadership style and conduct of foreign policy to his relationship with Congress, the voters, and the bureaucracy. The central theme, however, is based on Bush's leadership style and effectiveness given the complexity of our system of government, the weak party system, and a divided government. It is an evaluation of performance as it relates to personality and style. As with Nixon and Reagan, the evaluation of Bush's performance is a mixed bag.

It is still too early to specify Bush's presidential exemplars definitively. But like his immediate predecessor, Bush focuses on FDR as an exemplary president. Thus, the Bush presidency, too, is analyzable within Abbott's framework, further underscoring the pervasive influence of FDR, as Abbott argues when he states that "to the extent to which the exemplary presidency of Franklin Roosevelt represents the exemplary presidency in general as a mode of governance, future presidents will indeed always remain the children of FDR however much they seem to reject their father" (p. 201).

University of Texas, El Paso ROBERTO E. VILLARREAL

Ethnic Identity: The Transformation of White America. By Richard Alba. New Haven: Yale University Press, 1990. 374p. $35.00.

The American Kaleidoscope: Race, Ethnicity, and the Civic Culture. By Lawrence H. Fuchs. Hanover: University Press of New England, 1990. 618p. $40.00.

Social science inquiry on race and ethnicity has spanned a continuum ranging from broad theoretical treatments to rich case studies of particular ethnic communities. Decades of such investigation notwithstanding, researchers and policymakers continue to ask fundamental questions about ethnicity in the United States. Furthermore, these questions are framed at all levels of analysis from nation–state to individual. What sustains a national identity—a sense of membership—in the wake of growing ethnic diversity? What determines the degree to which different ethnic groups align themselves with a national identity? What does it mean to an individual to be a member of a particular ethnic group? How does this meaning change over time and across space?

Two new works grapple with these questions, one casting its net wide across the entirety of U.S. history, the other focusing deeply on the ethnic experiences of a single community at a single point in time. Together, Lawrence Fuchs's *American Kaleidoscope* and Richard Alba's *Ethnic Identity* advance ethnicity research by providing cogent analyses of ethnicity as a profound influence on U.S. public policy and as an ever-changing element in the construction of personal identities among individuals.

Two ambitious themes frame *The American Kaleidoscope*. Part 1 assesses ethnicity as a major factor in the development of U.S. national identity. Fuchs reviews how the state has managed ethnic diversity over time and what conflicts have arisen from these strategies; part 2 places ethnicity in its current policy context in areas as varied as education, employment, civil rights, residential settlement, and political participation.

For Fuchs, the basic demographic process engendering struggle in the creation of U.S. national identity was from the start, is now, and ever shall be international immigration (a topic Fuchs is well equipped to address as past director of the U.S. Select Commission on Immigration and Refugee Policy in the late 1970s). Fuchs traces the influence of immigration and immigrants upon elite and folk visions of Americanness from colonial settlement to the present day. In doing so, he draws a useful distinction between two kinds of "ethnic pluralism" evident in the historical record: a voluntary pluralism, characteristic of European ethnics, and a "coercive pluralism" (or enforced separatism) characteristic of the African, Mexican, Asian, and Native American experiences. The policy mechanisms of exclusion differ for each of these nonwhite ethnic groups, from slavery to enforced labor migration and repatriation—mechanisms that Fuchs outlines in admirable detail. He then argues that the powerful logic of the U.S. civic culture, with its promotion of participatory democracy and equal opportunity, gradually undercut support for separatism and exclusion of nonwhites, replacing such approaches with a popular conceptualization of ethnicity closer to the voluntary pluralism ideal—in which ethnic diversity exists and has meaning for people but no longer means necessarily constrained life chances for nonwhites.

In keeping with his portrayal of a U.S. civic culture where laws and civil rights forge the ties that bind (as opposed to ethnically based claims of membership), Fuchs devotes considerable attention to the intersection of policy and ethnicity. He interprets the policy record on ethnicity as the demolition of state-sanctioned separatism protecting white interests and the simultaneous emergence of state-sanctioned pluralism promoting the interests of ethnic minorities through such initiatives as affirmative action and bilingual education. Fuchs con-

cludes that many of the xenophobic fears behind restrictive immigration policies and English-only movements are doomed to political failure, since they violate the civic precepts of fairness and freedom. He ultimately takes a page from William Julius Wilson's *Truly Disadvantaged* (1987) in his assessment of current policy trends in ethnicity; that is, Fuchs charges that race-specific attempts to redress historical wrongs have done little to improve the lot of most nonwhites, particularly those of the urban underclass. His prescriptions call for a move beyond race-specific policies to race-neutral, class-based antipoverty initiatives aimed at all those in need.

The theme of a nation whose civic culture accommodates, and eventually transforms, the meaning of ethnic diversity finds corroboration in *Ethnic Identity*, Richard Alba's exhaustive survey analysis of one white ethnic community in upstate New York. With carefully constructed hypotheses derived from both psychological and sociological literatures, Alba seeks the meaning of ethnicity in the daily lives of individuals whom Fuchs might call the inheritors of the voluntary pluralism tradition. Alba's analyses dissect aspects of ethnic experience as varied as ethnic food consumption, ancestral language use, ethnic endogamy, and settlement in ethnic neighborhoods. He concludes that the traditional anchors of ethnicity for white Americans—undivided ethnic ancestry and attachment to ethnic social structures like churches, neighborhoods, and community groups—are giving way to more individualized, symbolic expressions of ethnic identity. In other words, the Alba survey respondents *feel* ethnic (reporting their ethnicity as an important element of their identity) even when they do not *act* ethnic in the sense social science has historically understood, that is, by marrying ethnic fellows, speaking ancestral languages, or participating in ethnic social organizations. From this research, Alba envisions the emergence of a new "European American" ethnic category, identifying, in a general sense, with an immigrant experience in the first 150 years of American nationhood and restricting its expressions of this identity to symbolic practices that do not much influence daily life.

In this sense, Alba acknowledges and corroborates Herbert Gans's *Middle American Individualism* (1979). However (Alba insists), at least two caveats should be attached to Gans's view that such "symbolic ethnicity" implies a trajectory toward the deterioration of ethnicity as a meaningful concept. First, Alba notes that ethnic identification appears as significant among younger cohorts as those closer to the immigrant experience and that it is more vigorous among more highly educated respondents (although he admits that his cross-sectional data do not allow him to trace such identification patterns over time). Second, Alba speculates that ethnic identification among whites may persist, at least in part, as a function of the demographic momentum of other ethnic populations (e.g., Latinos and Asians), who promote and perpetuate their own ethnic identities. Regrettably, Alba does not explore the possible political, economic, or social implications of such reactions on the part of white ethnics—a thesis worthy of continued study.

While the theme of ethnicity undergoing transformation emerges from both of these works, the volumes could hardly be more different in tone, style, or methodology. Fuchs draws upon copious historical, archival, and secondary data sources spanning three centuries, while Alba methodically probes the responses of 524 survey respondents in a single community in an effort to uncover precise patterns of ethnic ancestry and associated trends in ethnic identity. Not surprisingly, the differences in approach lead to different points of critique.

Fuchs admirably weaves together a massive amount of information. However, no tables are presented, leading to some confusing narrative summaries of empirical evidence. More importantly, Fuchs's effort to cover so much ground results in his drawing upon evidence that is, at times, highly selective. This is particularly evident in his treatment of the politics of language, where he relies disproportionately on newspaper accounts of ethnic conflict over bilingual education in the southwestern United States and on the autobiographical essays of Richard Rodriguez, whose dramatic social commentary is often invoked as a rationale for policy reforms few U.S. Latinos actually support, like the dismantling of bilingual programs. Such evidence is emphasized at the expense of many widely available scholarly efforts to evaluate bilingual education more systematically, such as Cummins's *Empowering Minority Students* (1989). In contrast, Fuchs's treatment of immigration policy is most compelling. He sounds a much-needed, well-documented note of reassurance to those who fear an onslaught of foreigners, deftly illustrating the contribution of international migration to the social and economic fabric of the United States.

Alba's micro approach cuts a narrower path through questions of ethnic identity, adhering closely to the classic social science treatment of a research question. Theories are reviewed, hypotheses formulated, and data analyzed and discussed in meticulous detail. Alba, however, makes little attempt to place his findings in any sort of political or public policy context. Several provocative questions emerge from his analyses. For instance, does the transformation of ethnicity among whites into an amorphous, symbolic identity imply reduced significance of political mobilization around issues of ethnicity for these groups, or might such mobilization simply take an equally amorphous "whites-verses-nonwhites" tone? Does the changing nature of white ethnic identity undercut white support for race- or ethnicity-specific public policies or for immigration policy reform? In what ethnic terms do whites and nonwhites in the Alba community view each other, and what are the implications of these views for political conflict and/or coalition building? With his unwavering attention to his white-ethnic-European-American sample, Alba thoroughly highlights one pattern in Fuchs's American ethnic kaleidoscope. The next task will be to understand how this pattern influences the potential social and political patterns of other groups in times of rapid ethnic compositional change.

University of Arizona SUSAN GONZALES BAKER

Purchasing Power in Health: Business, the State, and Health Care Politics. By Linda A. Bergthold. New Brunswick: Rutgers University Press, 1990. 213p. $37.00.

According to most published accounts, corporate America has not played a proactive or concerted role in health care reform. Corporate interest has been traced to the inflation in hospital expenditures and the economic

recession of the early 1980s. Corporations responded by implementing cost containment measures (e.g., cost sharing, HMOs, PPOs, and wellness programs) for their own work forces, not by shaping national health policy.

Linda Bergthold challenges this account in her book, *Purchasing Power in Health.* She sets out to demonstrate that business not only involved itself in health reform as early as 1969 but also developed as an organized interest group in health care politics.

Bergthold's agenda is ambitious. She seeks to combine case studies with survey evidence in order to illustrate business activity at federal, state, and local levels with a view to documenting widespread business participation in health care policy formation and implementation since 1969. She suggests that business participation in health policy formation is evident in three types of interventions: institutional mechanisms for change, such as coalitions and lobbying groups; advisory bodies, such as policy committees; and direct legislative participation by business leaders. At the federal level, she describes the genesis and activities of the U.S. Chamber of Commerce, the Business Roundtable, the Washington Business Group on Health, and the Dunlop Group. At the state level, she analyzes the role of business in California's MediCal legislation and Massachusetts' hospital prospective payment system, both enacted in 1982. She also compiles survey evidence from three studies of state business coalition activity and health policy involvement to support the conclusions from these case analyses. Finally, at the local level, she briefly discusses the prevalence, origin, and process of business–health coalitions.

This broad agenda is perhaps too ambitious. The author has understandable difficulty interweaving the case studies with survey data collected by others, as she also does interrelating the activities of business leaders at the different geographic levels of analysis. As a result, the wide array of business leaders, groups, and events described here do not cohere.

Equally troubling are several weaknesses in Bergthold's argument. One is her failure to justify 1969 as the starting point of business involvement in health policy. According to her argument, 1969 witnessed a blending of economic and political motivations for business participation, "the beginning of a decline in corporate profits accompanied by a national involvement of both business and government in reorganizing and rationalizing health care" (p. 22). While the economic motivation makes sense, Bergthold never confronts evidence that such motivations were not keenly felt until much later, in the early 1980s. For example, she never explains why business dragged its feet during the 1970s on such health reforms as offering HMOs to employees or why a 1980 survey concluded that "corporations were neither greatly concerned nor strongly motivated to do much about their health care costs" (Sapolsky et al., "Corporate Attitudes toward Health Care Costs," *Milbank Memorial Fund Quarterly* 59). Conversely, one can argue that economic motivations actually developed earlier in some cities due to a "strategic orientation" among business elites to control health care cost escalation for their communities. Such motivations surfaced in Minneapolis during the late 1960s and early 1970s with the formation of the Twin Cities Health Care Development Project. This coalition inexplicably receives no mention in the book. The political motivation for business participation, on the other hand, is never explained.

A second, related problem is the book's thesis that business supported or created institutional mechanisms during the 1970s to implement their health policy agenda. There is insufficient evidence here of a coherent business agenda for health policy in the making during this period. Some of the institutional mechanisms Bergthold describes did not exist during the seventies; others were neither created, nor dominated, by business (as the author admits). Oftentimes, Bergthold's account of business's role in policy formation is too vague: "Business exercised its power instead in more subtle ways, stemming from the structural power of business in society" (p. 83).

As a result of these problems, the reader cannot reject the alternative hypothesis that business is a late entrant to health policy development. Nor is the reader convinced that until recently, business has been anything more than a follower of government initiatives to control health care spending. Most of the case study and survey evidence dates from the 1980s. Moreover, the case findings from California strongly suggest that business was used by state democrats as a lever to sway Republicans and counteract providers.

Despite these problems, Bergthold's synthesis is informative in several respects. The book traces both the historical development of several major business coalitions (primarily at the federal level) and the roles played by key corporate and political leaders, as well as rich case studies of their involvement in specific pieces of health care legislation. The book also insightfully identifies some of the inherent conflicts that can undermine business coalition activity, such as business's multiple roles as "purchaser," "insurer," and "hospital trustee." Finally, the book points to the growing interaction, or "fusion," of public and private power in the formation of health care policy. This fusion of interests constitutes the major coalition in Bergthold's book. The business–government alliance is now successfully confronting the hospital–medical industry over such policy issues as payment and quality review.

Business power in health care purchasing and policy is clearly on the rise. Bergthold provides some excellent historical background on key business coalitions and associations that will likely play a major role in future reforms and sketches some profound issues emerging from business involvement. The lack of integration and convincing evidence regarding the timing of business involvement should not obscure these important contributions.

University of Arizona LAWTON R. BURNS

Long-Wave Rhythms in Economic Development and Political Behavior. By Brian J. L. Berry. Baltimore: Johns Hopkins University Press, 1991. 241p. $49.50 cloth, $19.95 paper.

In the ranks of analysts who find long-wave arguments attractive, rarely a move is made without some trepidation. There are certainly more popular, more paradigmatically sanctioned, and therefore more prudent courses of action within academia. Sometimes, however, the data literally grab you by the throat and seem to leave few choices other than to take cyclical explanations seriously. Brian Berry describes such an experience as his initiation into the much-contested

arena of long-wave rhythms. Examining the relationship between economic growth and urban migration patterns in the United States since 1790, Berry found that accelerations and decelerations in economic growth, once smoothed by decadal averaging, took the form of half-century fluctuations accompanied by similarly patterned bursts and declines in the pace of urban migration. Once hooked, Berry felt compelled to explain this relationship and was thereby seduced into exploring the long-wave literature.

But Berry's book is not about processes of urban migration. There are two other facets of the long-wave conversion process. One is that whatever topical interest with which you begin, an interest in long-wave explanations forces an analyst to come to grips with the root causes of long waves. The phenomenon is not unlike an explorer of a river's tributary branch becoming obsessed with finding the river's source. A second by-product of delving into the long-wave literature is an initial reaction of dismay at its intellectual disorder. A compulsion to set things right and bring order to a confused and confusing set of arguments and evidence is one possible outcome. As Berry describes the subfield "It is a difficult land, populated by tribes with different cultures and languages, separated by their world-views and by the boundaries they defend" (p. xiii). What he claims that he wants to do is to provide readers with a better map of this difficult land than has previously been available.

Berry's reference to the difficult terrain of long-wave analyses is more accurate than is his claim to have executed a superior mapping of its difficulties. Rather than explore all the hills and valleys (one viable way to map the landscape), Berry has decided to take a major shortcut or two. Instead of surveying all the explanatory paths, Berry chooses to focus on the route that he finds most efficient for explaining why economic and other types of long waves exist.

He also chooses to do it in what comes across as a highly inductive exercise. One of his most serious complaints about the literature he is addressing is the poor quality and paucity of appropriate data analysis. Thus, we are bombarded with 137 figures to take us through a series of exercises demonstrating behavioral regularities, often with the aid of an unusually user-friendly form of chaos mapping.

The type of behavioral regularity that Berry finds most useful to explain long waves is a roughly 25–30 year Kuznets cycle in infrastructural investment. Two of these Kuznets cycles are embedded within each Kondratieff long wave. The typical movement can be seen as beginning in a long-wave depression, moving to a period of inflationary growth (in the first Kuznets), and followed by a phase of stagflation, a period of deflationary growth (in the second Kuznets), and the next long-wave depression. These alternations in growth and prices set up longer-term fluctuations in technological change. Established technologies saturate their markets in the first and inflationary Kuznets. New technologies emerge in the second and deflationary Kunzets.

Berry advances an interesting argument that may hold up to further scrutiny. His argument, which centers on a specific causal dynamic frequently absent in long-wave analyses, is certainly testable and also has novel qualities, especially in terms of the alternating Kuznets sequence. But much more work remains to be done. What Berry demonstrates is that there is supportive evidence in United States economic data for the sequencing that he describes. Which processes drive what other processes still needs to be worked out theoretically and empirically. Moreover, it also remains to be seen just how uniquely American his economic sequencing is. If other economies are subject to long waves without the dependence on paired Kuznets, what might that suggest for Berry's preferred explanation?

Berry might have pursued some of this further modeling. Instead, he devotes chapters to speculations about such related topics as war, critical elections, mass psychology, demographic implications, stock market indexes, and climate. The chapter on critical elections and realignment processes is useful in the sense that the relevant political science literature usually stops short of linking the political outcomes explicitly to the underlying, long-term, economic fluctuations. Yet these chapters fall well short of doing justice to the complexities of their subject matters. In some cases, as exemplified by the chapter on war, Berry makes some of the same types of weakly grounded, loose statements that he criticizes other long-wave analysts for. Hence, these auxiliary chapters are at best suggestive.

This book does not provide a comprehensive analysis of long-wave rhythms in political economy. It does advance a provocative argument for why there might be long-wave rhythms. It generates persuasive empirical support for the strong argument that is advanced. Both are welcome contributions to a growing, increasingly sophisticated, and genuinely international research program.

Indiana University WILLIAM R. THOMPSON

Governors, Legislatures, and Budgets: Diversity across the American States. Edited by Edward J. Clynch and Thomas P. Lauth. Westport: Greenwood, 1991. 191p. $42.95.

This is a study of how the budgetary process works in practice, at both the executive and legislative levels, in 13 American states, with the emphasis on events during the 1980s. The chapters are written by experts in each of the states, an approach that is often necessary in comparative state studies and that works well in this volume. The editors have provided reasonable guidelines; and these are usually followed by the authors, so that similar topics are covered in each chapter.

Each of the brief state chapters is tightly packed with detailed information. To make this a valuable comparative study, there needs to be a concluding chapter, synthesizing the findings and explaining what practices and trends are common to the states, the differences among them, and why these exist. The editors' final chapter is designed to answer such questions; but they do so too briefly, in only seven pages, emphasizing primarily legislative–executive relations.

Several common patterns and trends stand out. The process of budgetary review has grown more sophisticated in all the states, partly because of the use of computers. In most states, the governor has gained more direct control over the executive budget. Most state legislatures have developed a more competent and better-staffed committee structure for budgetary review.

There are differences among the states in whether the balance of power in budget making is shifting to the executive or the legislature. Clynch and Lauth conclude

(correctly, I believe) that there is a trend toward greater balance, a shift away from domination of the budgetary process by either branch. There are a few states, however, such as Texas and Mississippi, where staffs of both the governor and legislative committees prepare budget estimates, and the legislature pays more attention to its own committee recommendations.

It is also clear from this book that no matter how well organized and staffed the budgetary process seems to be, a serious financial crunch will play havoc with the system, often leading to special sessions and deadlocks, particularly when there is a partisan division of government. The chapters include several examples of this, particularly during the early 1980s. Texas and Kentucky illustrate well the impact of severe economic setbacks on budgets. Even more dramatic examples have occurred in the 1991 legislative sessions, since this book was written.

Unfortunately, this book tries to cover too much too briefly. The state chapters, which average only 11 pages each, describe the formal executive and legislative budget-making process in some detail. But often, the political realities are skimmed over too briefly. For example, the California chapter barely mentions the constitutional requirement that the budget receive a two-thirds vote in each chamber, a reality that has enormous political implications. The Illinois chapter tells us only briefly about the governor's aggressive use of the amendatory veto and its impact on the whole budgetary process.

Some of the chapters provide interesting detail about the political aspects of budget making. In Connecticut a strong partisanship pervades the budgetary process. In Texas, the election of a Republican governor changed the political dynamics of the process, while in South Carolina a governor aggressively challenged the traditional legislative control of budget making. Florida illustrates the importance that conference committees can play.

The editors, in their concluding analysis, might have given more attention to the political dynamics of budget making, such as the importance of divided government and the problems raised when economic problems require severe budget cutting. It is important to understand how the political battles over state budgets have been affected by the shifting of federal responsibilities to the states; the public clamor for more services and fewer taxes; and the continued inability of state governments to control the state's economy (at least in the short run), which makes rational budgetary planning impossible.

University of Kentucky MALCOLM E. JEWELL

Contested Ground: Collective Action and the Urban Neighborhood. By John Emmeus Davis. Ithaca: Cornell University Press, 1991. 356p. $45.00 cloth, $16.00 paper.

Over the past 10 or so years, urban scholars have undertaken a reconstruction of urban theory based on the political and economic restructuring of the last several decades. This project has been interdisciplinary in its scope, and the resulting insights have effectively refined and expanded our understanding of the processes behind urban growth and change. Yet while new insights and understandings have been generated at the level of political and economic structures, insufficient attention has been paid to theorization below the urban level at the smaller scale of the neighborhood. In *Contested Ground,* John Emmeus Davis, an urban planner by training, makes a significant theoretical contribution to illuminating this poorly understood yet critically important area by moving our understanding beyond well-worn broad generalizations about the class-based nature of community action. The central aim of this book is to begin the process of developing a theory of domestic property interest group formation based on an empirical exploration of the conditions under which territorially based collective action occurs. Toward that end, Davis advances a theoretical framework that combines a static model of property interest groups with a dynamic model of group formation and intergroup conflict. In short, Davis examines objectively observable and subjectively held interests and conditions in order to explain why and when consciousness and organization combine to produce a collective action that is fundamentally tied to place and property.

Contested Ground is divided into three parts with a total of 13 chapters and two appendixes. In part 1, Davis provides a comprehensive and critical treatment of the Marxist and Weberian perspectives on locality-based collective action. Davis's objection to the Marxist perspective is that it tends to misrepresent all struggle as class struggle. He argues that such an approach is theoretically impoverished and insists that urban collective action is most often based on complex territorial, not class-based, interests. Davis is also dissatisfied with the Weberian attempts at theorizing housing classes and contends that an emphasis on equity as the only interest that is causally significant severely limits the explanatory power of such an approach. Part 1 is completed with a careful and detailed elaboration of an alternative theory of territorially based collective action derived from an ideal typology of material interests and a three-stage model of housing classes based on *propertied*-versus-*propertyless* categories. Davis argues for 10 distinct property interest groups distinguished along the axes of tenure, function, and interest. His stage model of group formation is based on the interaction of levels of "housing consciousness" and "housing organization" that translate into three, potentially progressive, phases of intergroup conflict.

In part 2, Davis tests his theoretical framework empirically by way of a longitudinal case study of collective action in Cincinnati's West End community. The West End, like many inner-city areas of older industrial American cities, has gone through several waves of investment and disinvestment and was the object of large-scale urban renewal efforts in the 1960s and limited gentrification in the 1980s. Its population composition over the years has, for the most part, paralleled national migration and immigration trends. It is currently home to about thirteen thousand residents (many of them African–American), who hold varying tenancies. The case study of this diverse and currently divided neighborhood spans the years between 1800 and 1985 and is based on participant observation, documentary sources, and interviews with activists and representatives of locally important institutions. Davis expertly weaves theory and raw materials, providing a fascinating account of solidarity and fragmentation among West End neighbors, as well as a compelling argument for his interest-based framework. In part 3, Davis refines his theoretical framework based on the evidence from the case study and provides discussion of the usefulness of

his approach for practicing urban planners and community organizers.

Contested Ground is an important advance in an evolving urban theory that is drawing upon, and contributing to, developments in social theory. Davis has done a great service for all of us by developing a theoretical approach to locality-based collective action that makes connections to theories of the production and reproduction of urban space. The case study of Cincinnati's West End invites comparisons and further theory testing—perhaps a focus on the impact of group interests such as gender or ethnicity as they crosscut domestic property relations. At both the theoretical and the empirical level, this is a competent, convincing, and stimulating piece of scholarship that should be on the bookshelf of every urban scholar.

University of Arizona SALLIE A. MARSTON

What Older Americans Think: Interest Groups and Aging Policy. By Christine L. Day. Princeton: Princeton University Press, 1990. 240p. $24.95.

Demographic Change and the American Future. By R. Scott Fosler, William Alonso, Jack A. Meyer, and Rosemary Kern. Pittsburgh: University of Pittsburgh Press, 1990. 280p. $29.95 cloth, $14.95 paper.

The needs of the elderly in the coming century are important today not only because we care about older persons but because we also care how we ourselves will age; how we will relate to our families; and how, and with what values, our children will grow up. High on the list of concerns is to determine how we will meet the increasing health care requirements of a rapidly aging society and pay for the benefits all of us expect from our society. A recent report projects that "demographics, living arrangements, disability and income will be the major determinants of the future needs of the elderly" ("The Needs of the Elderly in the Twenty-First Century").

We can anticipate large numbers of disabled elderly, an increase in the number of elderly persons who are institutionalized, and an increase in the numbers of elderly individuals living alone. And there will be significant numbers of subgroups of old persons who are at risk financially. Most importantly, we have enough information to make it possible for us to be constructively involved in planning for this future, to enable us to influence our future.

It is highly likely that the domestic public policy debate of the next few decades will be focused on the impacts of changing demography on our society. This debate will be considerably enriched by the introduction of these two books. Both are well written and provocative and deal harmoniously with the companion issues of changing demography and the opportunity for constructive societal changes through the use of special interest groups.

Both books adequately help us separate from reality such myths as that what is good for the elderly is necessarily opposed to what is good for the society at large and that all old people will rise to defend any policy that benefits the elderly regardless of its content, value, or cost.

Day's book illustrates in a convincing way that collectively, "old people are no more supportive of government benefits for the elderly than are younger adults." Older people are as varied as are their political beliefs, social status, income, sense of health and security, and residence. People join age-related interest groups for many reasons, sometimes totally unrelated to their own political beliefs. This may include joining an organization for add-on benefits of membership that may include reduced rates for purchases.

The critical task for us as a society, to which this book contributes significant insight, is to learn to understand older persons (or people of any age) on the basis of their individual characteristics and needs. Although needs may be grouped collectively for the purposes of generating public policy, they should not be used to generalize about characteristics of the elderly. If this book does no more than drive home this significant point, it will have made an important contribution to the literature. How interest groups represent a particular segment of the society, how they are constituted, and some cautions about interest group participation represent a significant part of this book.

The book ignores the significant advocacy role of special committees on aging of both the House and the Senate, as well as concern about aging policies on the part of city, county, and state legislatures. All of these comprise very effective advocates, especially at the federal level where it intersects with their funding priorities.

Fosler and his colleagues' presentation uses demographics to highlight societal changes but clearly cautions that demographics, although seemingly quantitative and objective, may be misleading and misrepresent a changing phenomenon because they are "artifacts, stick figures attempting to represent fluid multidimensional realities." Unfortunately, this multiple presentation by several authors has resulted in repetition of content that is burdensome.

With the expectation that there may be one million centenarians in the United States by the year 2050, the authors argue for the need to understand the political constituency of the aging segment of the population and to evolve public policies that respond appropriately without denying the needs of other groups. It is a challenge to project an older population that includes the destitute, who need the assistance of society; those who are near poverty and also need help; members of a middle class, who expect to maintain their economic status through social security and other redistributive policies; and retirees well able to maintain their high standard of living as a result of their accumulated financial resources. The most difficult task involved is to balance the diverse needs of older Americans against the unfulfilled needs of many other segments of our society and to minimize the tension created by competition for limited resources.

Some important ideas that are different but in harmony are projected by the authors of these two books. One argues for such an alliance of advocacy groups as the program called Generations United, which has as its goal to minimize intergeneration disputes. The other volume emphasizes the significance of worldwide demographic changes on economic and social events in the United States and therefore argues for addressing the interdependence of changes across the life span. How we deal with our elderly has impact not only on the person who receives—or is denied—service but, from a wider perspective, on the total society, because it affects

people of all ages and backgrounds. What may be required is the replacement of special interest political advocacy groups with a national advocacy effort on behalf of all in need, irrespective of age.

University of Arizona THEODORE H. KOFF

Military Organizations, Complex Machines: Modernization in the U.S. Armed Services. By Chris C. Demchak. Ithaca: Cornell University Press, 1991. 202p. $31.50.

Cornell University Press has added another gem to its fine series on national security affairs. Chris Demchak's *Military Organizations, Complex Machines* is an excellent analysis of the interaction of technology and organization in the employment of modern weapons. The specific technology examined is the M-1 Abrams tank, and the hosting organization described is the U.S. Army; but the attempt is to provide broader lessons for militaries and organization theory.

Demchak is at her best in the conceptual chapters, where the problem of organizational complexity is effectively explored. Here, the influence of the emerging Berkeley school of organizational analysis is clearly visible and fully acknowledged. We learn that complex systems are vulnerable to a series of surprises because of their large number of internal relationships, each of which can be disrupted in unknown and undesirable ways. These nasty surprises ("rogue outcomes") plague the systems, forcing their managers into a continuing quest for coping strategies. Limited resources frustrate the managers' abilities to respond quickly and successfully to the inevitable rogue sets cascading through the systems. As downtime grows, the capacity of organizations to meet external challenges weakens dangerously.

The M-1 tank was designed to reduce the army's battlefield uncertainties. It was intended to be faster, tougher, and more lethal than the Soviet tanks it might someday encounter at the Fulda Gap. To do this, the M-1 was to utilize the latest technologies, including a gas turbine engine, laser-assisted targeting, and advanced gun–turret stabilization techniques that would allow it to fire accurately while maneuvering. The army's procurement agency, beset with its own political and organizational uncertainties, promised Congress that the M-1 would also be more reliable and cheaper to maintain than predecessor tanks, thus justifying its higher purchase costs. In order to achieve this, much of the repair was to be pull-and-replace after diagnosis by specially developed automatic test equipment. Of course, not all of these promises were kept. Although it has excellent combat capabilities, the M-1 has poor fuel economy, relatively short range, and a cranky disposition and is a nightmare to maintain.

The bulk of the book focuses on the army's attempt to deal with the nightmare. Demchak, a reserve officer, apparently had the needed access to the operators and the required technical knowledge to understand the maintenance adjustments initiated. In short, what was expected to be simple became very complex indeed as field units and an overly lean support network strove to cope with the operating uncertainties of the tank. Demchak offers some—but not enough—comparisons with the introduction of other complex weapon systems into the air force and navy to make convincing generalizations of this experience.

Nevertheless, Demchak is ready to draw lessons, particularly as they relate to wartime operations. Complex procedures instituted to cope with the surprises of complex technologies, she argues, will increase the operational uncertainties in the rear, as well as on the battlefield, raising the risk of catastrophic organizational failure. Instead, she believes, we should cautiously weigh the pros and cons of adding technical complexity to organizations with difficult, dangerous tasks like the military. Simplicity, if not the main guideline, should be at least a major option.

The Gulf War, which occurred as the book's final editing was being completed, is dismissed as exceptional. There, the M-1 was shown to advantage, as was nearly every other of our very complex weapon systems. But that war revealed how the American military actually copes with the risks of complexity. Two full complements of tanks for the units assigned were sent, one after the other. There were spares for spares. It is only junior officers and organizational theorists who worry about scarcity. Senior officers know that the organizational goal in that war and all others is to avoid American casualties and, like American politicians, believe the promise that costly, complex technologies offer for achieving that goal. Complexity, it turns out, is the only acceptable option.

Massachusetts Institute of Technology HARVEY M. SAPOLSKY

Civil Rights under Reagan. By Robert R. Detlefsen. San Francisco: Institute for Contemporary Studies, 1991. 237p. $24.95.

Liberals would argue that the title of this book is an oxymoron. Certainly, there have been laments that Reagan's reconstruction of the federal courts has led to the diminution of civil rights, successfully implementing a part of the "Reagan agenda." Robert Detlefsen joins the laments about the Reagan administration. Surprisingly, his complaints focus not on the effectiveness of the Reagan administration regarding civil rights, for he argues that the Reagan administration was unsuccessful in that effort. He focuses, rather, on Reagan's failure to dismantle affirmative action policies in employment and busing in education. He notes that Reagan failed despite the fact that both policies are strongly opposed by the American public. The puzzle he addresses, then, is how a highly popular president failed to abolish a set of highly unpopular programs.

The obvious answer to this puzzle should be that the American political system is notoriously resistant to change. Factors like the influence of interest groups, the importance of bureaucracy in implementing policy, and the American tendency to compromise lead us to expect that radical change is difficult to achieve. Detlefsen recognizes this but argues that the real answer lies elsewhere: "[A] *civil rights elite* in America . . . has developed an ideology of civil rights that necessarily demands the creation of a jurisprudence in which different kinds or levels of rights and privileges are allocated to individuals on the basis of their membership in particular social groups, defined according to racial, ethnic, and gender criteria" (p. 13; emphasis mine). This civil rights

elite has prevented the abolition of unpopular civil rights policies. Through an analysis of the opinions in several important legal cases dealing with affirmative action and school desegregation, as well as of federal civil rights agencies, Detlefsen attempts to show that administration efforts to implement reform were thwarted by judges and federal officers committed to a "civil rights ideology."

One could take issue with the thesis of this book. It is debatable that Ronald Reagan "failed" to change civil rights policy. This would only be true if one used the absolute criteria posed here. Moreover, it reflects a judgment that is focused solely on the short term. The author notes that three important cases in 1989 clearly indicate the current Supreme Court's willingness to undermine affirmative action policies. But even if one were to accept the author's focus, he is less than successful in making the case.

First, legal precedents constrain judicial decisionmaking. Although political scientists like to act as if judges have the power to do whatever they want, the limits placed on them by precedent are meaningful. As a result, judicial policies must change slowly. It also takes time to replace justices and judges on the bench with those who have the proper ideology. Despite a record number of appointments to the federal bench, Reagan could not change the federal bench overnight.

Second, despite the emphasis that Detlefsen places on the importance of a civil rights elite, his own account suggests that others outside that elite were either hostile, or uncommitted, to change. For example, he notes that efforts to abolish the use of quotas in hiring by the Civil Rights Commission were opposed by those in the business community. He also notes that those appointed to civil rights positions by the Reagan administration were often at odds on civil rights policy. Finally, Reagan himself was ambivalent, providing very little leadership on the issue.

Indeed, one of the interesting questions not addressed in this book is how these civil rights policies have managed to survive in the face of public hostility. The check on unpopular policies is not the courts but the voters, who presumably exercise their protest through the ballot box. Jesse Helms's 1988 campaign is only one piece of evidence that this issue is ripe for exploitation. To say that Reagan has failed is premature.

Washington University, St. Louis LIANE C. KOSAKI

No Escape: The Future of American Corrections. By John J. DiIulio, Jr. New York: Basic Books, 1991. 301p. $22.95.

Courts, Corrections, and the Constitution. Edited by John J. DiIulio, Jr. New York: Oxford University Press, 1990. 338p. $32.50.

The Scale of Imprisonment. By Franklin E. Zimring and Gordon Hawkins. Chicago: University of Chicago Press, 1991. 244p. $29.95.

Jails: Reform and the New Generation Philosophy. By Linda L. Zupan. Cincinnati: Anderson Publishing Co., 1991. 201p. $14.95 paper.

Each of the works reviewed here agrees that America's prisons and jails are overcrowded. Taken as a whole, these works demonstrate the tremendous legal, administrative, and political consequences of this obvious fact. In *The Scale of Imprisonment* Zimring and Hawkins look for causes of this phenomenon. DiIulio, in *No Escape,* surveys the broad landscape of prison overcrowding and offers suggestions for its management, while in *Courts, Corrections, and the Constitution* he pulls together authors of diverse points of view to assess in detail the impacts of court intervention into prison management. Finally, Zupan, in *Jails,* presents a very narrow focus on the issue of overcrowding by examining one heralded solution to its effects, if not its causes.

The implications of the problem lead Zimring and Hawkins to search for causes. In an impressive review of a broad and diverse literature, they suggest that no real causes have been found. In reviewing social and historical process theories, they find no consistent evidence pointing to a cause. Nor do intentional policy choices, such as mandatory sentencing, have a causal effect. They similarly report that neither variations in crime rates nor various policy changes have an impact on prison population sizes. Public opinion, politics, economic forces, and even drug convictions are ruled out. The absence of discoverable causes compares well with DiIulio's premise that there is "no escape" from the problem. Moreover, Zimring and Hawkins are in agreement with DiIulio that remedy plans have not succeeded as policy. While DiIulio does find that intensive supervision probation is effective, it can only be used for a small portion of the incarcerated. He bases this on the assumption that 95% of the prison population is "dangerous" and belongs there. This assumption is not uniformly shared by policymakers or analysts.

Zimring and Hawkins, painting with an even broader brush, suggest that none of the remedy policies has succeeded in lowering prison populations. Despite this minor disagreement, DiIulio argues that the only way to solve the problem is to manage it. Overcrowding can be overcome. The difference between prisons, then, is the management. Accordingly, he offers several principles by which to manage. However, these principles run the risk of being seen as "proverbs" or the "one best way" to manage a prison. As the Zimring and Hawkins data suggest and the case studies in *Courts, Corrections, and the Constitution* demonstrate, the contours of the problem change with the context. It is difficult, therefore, to imagine one set of immutable principles.

DiIulio also argues the need to limit court intervention in prison administration. While he concludes of his edited book that on balance, the results of judicial intervention in prison administration have been positive, he presents as a basic principle for judges that they must *decide things and order things incrementally* (borrowing from Lindblom's analysis). DiIulio reaches this conclusion by analyzing case studies, arguing that too vast changes made too quickly result in chaos. He takes a decidedly harsher tone in *No Escape.*

Other contributors present a more mixed view. Three views of the Texas prison case (Ruiz) suggest that while there was some disorder and violence, this was overcome and the results of intervention were more or less positive. The case of the New York City jails (particularly the Tombs) is also seen as a success, though Zupan does not even address the role of the court as part of the success. Rather, her work suggests that the new generation philosophy was responsible for the limited success.

The authors in *Courts, Corrections, and the Constitution* provide a glimpse of a taxonomy of variables and a framework within which to view judicial intervention.

Feeley and Hanson discuss the impacts of reforms and the institutional and organizational atmosphere resulting from intervention. To this we may add the state's (Useem) and city's (Storey) political climate. Differences between trial court and appeals court case handling are of significance (Talarico and Chilton), as are differences between the power of federal and state judges (Useem). The impact of due process reforms has discoverable limits (Rhine), but policy change can be implemented with a "fixer" and a powerful judge (Storey).

What solutions to prison overcrowding seem to emerge from this broad landscape of contingent variables? Zupan suggests that the new generation of jail design and management represents one possibility. Her evidence is mixed on the capacity of design and management reform to ameliorate the spin-off problems (violence and staff burnout). However, she argues that this solution is understudied and offers some hope of alleviating the symptoms if it cannot cure.

Zimring and Hawkins suggest discovering a political economy of incarceration. For them, the problem is the "free lunch" that incarceration offers in the public choice model. The sentencing authorities (the courts) do not have any costs associated with sentencing. Hence, other variables cause prison population fluctuations independent of the true costs of incarceration.

DiIulio offers a partial resolution of cost problems by suggesting the nationalization of the prisons, having them administered by the Federal Bureau of Prisons but paid for by the states. He also suggests, as more achievable solutions, that guidance be offered to judges to make them more effective. This is important if, as Bradley's contribution suggests, Republican judicial appointees are no more and no less likely to be interventionist-minded. DiIulio also suggests that good management would go a long way to solving most of the difficulties associated with overcrowding.

It is interesting to reflect that the same argument can be made regarding judicial intervention. As Ekland-Olson and Martin note in their contribution, the pattern of resistance by management to court orders in Texas was the true source of harm. If so, then intervention can be dealt with, as well. While none of the authors suggests an analogy, the model of bankruptcy courts for managing the assets of major corporations comes to mind. Rarely is this economic bankruptcy model questioned. Yet in a case of "constitutional bankruptcy," the federal courts are seen as incapable. The evidence weighs in favor of intervention based on these readings.

Each of these works will be useful to a broad variety of scholars. *Courts, Corrections, and the Constitution* will be of great interest to students of policy, implementation, state and local politics, public administration, public law, and criminal justice. It is well written and exceptionally well organized. In contrast, *No Escape* is more likely to be useful to public administration and criminal justice scholars. Like the edited volume, it is highly detailed but less balanced in its presentation of the evidence. Both are brilliantly written and are highly recommended.

The Scale of Imprisonment has an exceptionally well designed literature review of interest to public policy, criminal justice, and public law scholars. Its careful review, analysis, and critique of research is stimulating and inventive. This book, too, is highly recommended as well.

Jails is far narrower in scope than the other three. For those with a specific interest in jail design and problems related to management at the micro level, this work will prove useful. It does not address issues most policy or implementation scholars would find of interest. However, students of criminal justice policy may find this useful, depending on their focus. It relies heavily on the social psychology of jails and is accordingly devoid of interesting content for most political scientists or students of public administration. It is a thorough and painstaking assessment of this new theory in jail design and management. While it ignores many interesting theoretical issues (e.g., it fails to demonstrate empirically any essential link or dependency between design and administration), it is recommended for those with interests in this limited area.

California State University, Chico GREGORY D. RUSSELL

The Constitution and the American Presidency. Edited by Martin L. Fausold and Alan Shank. Albany: State University of New York Press, 1991. 323p. $49.50 cloth, 16.95 paper.

Martin Fausold and Alan Shank adumbrate the theoretical issues on which this symposium volume's arguments converge. Following their remarks are essays by historians and political scientists that explore and explicate various aspects of the constitutional development of the presidency.

The historical selections in part 1 focus on how disparate presidents from Thomas Jefferson to Harry Truman augmented the constitutional authority of the executive. Ralph Ketcham suggests that Jefferson's presidency combined the Aristotelian ideal of good government and a classical republican emphasis on civic virtue with creative executive leadership. Robert Remini illustrates how Jackson, through his domination of other institutions of government and his control of the nation's agenda, "reconstituted" the office of the presidency. Michael Les Benedict shows that Lincoln, even though he established "precedents for presidential power that remain potent today," was ultimately cognizant that "inherent presidential powers" neither constricted nor superseded the constitutional authority of Congress (pp. 55–56). William Harbaugh links Theodore Roosevelt's expansion of presidential prerogatives with the unprecedented growth and concentration of "private economic power" that obtained in the latter part of the nineteenth century. In his portrait of the presidency, Ellis Hawley describes and contrasts Hoover's and FDR's policy responses to the Great Depression. He also considers how the resolution of constitutional questions precipitated by FDR's "managerial solution" for this economic crisis affected the constitutional evolution of the presidency. Hawley concludes that the strictures imposed by the constitution were "altered to accommodate" the "administrative state" and the "modern presidency" inaugurated by the New Deal (p. 104). Finally, Donald McCoy illustrates how Truman's broad interpretation and vigorous assertion and defense of his constitutional authority facilitated the extension of presidential power.

Written by political scientists, the chapters in part 2 underscore salient constitutional issues and questions

provoked by the accretion of presidential power in domestic and foreign affairs over the last half-century. Louis Fisher argues that the Budget Act of 1974 entails complex legislative procedures that favor presidential control and decrease presidential responsibility in the budgetary process. He contends that this piece of legislation has culminated in irresponsible budgetary decisions that the Gramm–Rudman–Hollings Act of 1985 could not control and that an item veto will not correct. Louis Koenig describes in detail how even though the founding fathers "left open the question of which branch was to have the decisive voice in foreign affairs," presidents have expanded their powers to realize considerable autonomy in the foreign policy arena (p. 173). Similarly, Richard Pious provides ample evidence to show that the War Powers Resolution of 1973 has failed to curtail presidential autonomy in military affairs. Donald Robinson emphasizes that the presidency, as it has developed over last half-century, "poses a dire threat to constitutional government" in the United States (p. 212). To meet that threat, he proffers several constitutional reforms designed to restore salutary restraints on executive power. In a carefully reasoned argument, Jeffrey Tulis challenges the conventional viewpoint concerning the scope of executive power implied by the "original constitution." Applying a novel and rather evocative interpretation of the constitution, he advances the thesis that the political order engendered by the New Deal did not represent a revolution in constitutional principles. Ultimately, he argues, large government and the modern presidency are really "implicit in commitments ratified two centuries ago" (p. 134).

Theodore Lowi's afterword provides a lucid and exceptionally trenchant analysis of the constitutional development of the presidency as it is portrayed in the other essays. He underscores the analytical weaknesses of the "heroic/developmental theory" of the presidency that links the discrete historical studies of this book. He also repudiates the ideological uses of the heroic model exemplified in both liberal and neoconservative analyses of the presidency. He opts, instead, for a "radical constitutionalist position." In Lowi's view, this model is constitutional because it accentuates the primacy of the separation of powers and checks-and-balances and "radical because it is willing to sustain some losses, if losses are necessary to sustain constitutionalism" (p. 241).

Overall, the essays in this work represent informed, judicious, and even provocative treatments of constitutional issues and questions involving the presidency. This collection, however, is not devoid of minor shortcomings. Some of the historical studies tend to be a bit too episodic. Thus, these essays do not always make clear how the precedents established by the presidents they depict shaped and constrained choices and actions of succeeding executives. In addition, concern for the president's expanded role in foreign and military affairs dominates the political science component of this work. More attention could have been given to the constitutional constraints that encumber executive action in the domestic arena. Still, this work offers a broad range of splendid studies that will be extremely valuable for anyone interested in understanding the constitutional authority of the executive.

Weber State University GARY LEE MALECHA

Water Resources Management: In Search of an Environmental Ethic. By David Lewis Feldman. Baltimore: Johns Hopkins University Press: 1991. 247p. $38.50.

Thirst for Growth: Water Agencies as Hidden Government in California. By Robert Gottlieb and Margaret FitzSimmons. Tucson: University of Arizona Press, 1991. 286p. $35.00.

These are two very different books that take a somewhat similar case study approach to their subject. In *Thirst for Growth*, Gottlieb and FitzSimmons have provided a very thorough and readable history, description, and analysis of six major water agencies in southern California. The first five chapters provide histories of the Metropolitan Water District of Southern California, the San Diego County Water Authority, the Upper San Gabriel Valley Municipal Water District, the City of Burbank Public Service Department, the Imperial Valley Irrigation District, and the Kern County Water Agency. Chapters 6–10 examine various political and management issues, including the isolation of water managers from public scrutiny; the role of consultants, the press, and community groups in water agency management; the increasing role of water markets in water management; and various management techniques designed to improve "efficiency," such as conjunctive use and peak pricing.

Gottlieb and FitzSimmons describe a water industry preoccupied with growth and expansion and insulated from the public (which it cares very little about) that pursues policies of supply augmentation, rather than the politically riskier demand reduction (a strategy consistent with the growth orientation of the agencies) and is more concerned with keeping costs down (e.g., by inexpensive and less efficient purification methods) than with pursuing policies that best ensure the public health.

This is a good book. Those unfamiliar with water management issues (e.g., water marketability and conjunctive use) will find the book a valuable introduction to these and other subjects and the problems of implementing new ideas in water management. All readers will enjoy the case studies of the agencies examined and the descriptions of the bureaucratic and institutional cultures that have played such an important role in the development of southern California.

The authors argue that their work uniquely contributes to our understanding of water management by showing that "local agencies arise out of local initiatives, rather than from a general logic of water development" and that "it is local initiatives, not more widespread constituencies, that stimulate and support federal investment" (p. xviii). Although it does not detract from the value of their work, the authors are not the first to make this observation.

In the concluding chapter, the authors make several suggestions for reform. These include increased public input and public participation, a move away from the growth orientation of agencies in the not-too-distant future, more attention by water agencies to equity and quality of life issues, and redirection of agency efforts away from expansion and toward a greater focus on water quality. These are all good ideas. It would seem that given the geography of southern California, a shift away from a growth orientation is inevitable and that water quality problems will necessarily demand more attention. As concerns quality-of-life issues, it would

seem that the growth orientation of the agencies in this study have already largely dictated the terms of that debate in southern California.

David Lewis Feldman also uses case studies in *Water Resources Management*, examining the Garrison Diversion Project in North Dakota and the Blue Ridge Pump Storage Project in North Carolina and Virginia. Feldman suggests we adopt a new environmental ethic in water resources management, as well as in natural resources management generally. Using a combination of regional resource management, increased public participation, and acceptance of the assumptions of deep ecology, Feldman posits an ethical system for human intervention with nature. Beginning with the premise that "the conferring of water rights upon a few groups and the placement of authority for policy in the hands of an engineering elite have produced a water policy both undemocratic and unresponsive to environmental concern" (p. 54). Feldman proposes that water be managed through a system of regional, self-sustaining authorities (based on the hydrogeographic regions developed by the Water Resources Council). Management would be guided by equal access and appreciation for the interdependency of natural resources (i.e., ecosystem balance), historical patterns of development, and Pareto optimality. Water managers would allocate water on a principal of "universal equity," taking into consideration (among other things) the rights of other living creatures. Such a system would "require self-restraint and an abandonment of self-interest" (p. 197).

Although I enjoyed the book and would use it in my water resources seminar to stimulate discussion, I was sometimes left wondering where the water resources world that I was familiar with fit in. Although most people would agree that water resources policy has often been skewed to the advantage of certain well-financed or well-organized interests and has, at times, ignored the concerns of the less powerful, and although it is not difficult to agree that the ditch-dam-and-dike mentality of water resources development in the past has ignored ecosystem sensitivity and more benign ways of managing our environment, some of Feldman's solutions strike me as politically unfeasible and others as practically undesirable. Concerning the former, I see little chance that the current political actors in the water policy arena would be willing to turn their powers over to regional authorities. I suspect we may have to deal with the current set of water management institutional arrangements for the foreseeable future. On the other hand, there is some evidence that water resource agencies are becoming more cognizant of the externalities associated with their activities. We can largely thank the National Environmental Policy Act and environmental organizations for that.

Both of these books are valuable contributions to the water resources management literature. Feldman has applied certain aspects of political theory and environmental ethics to water resources management in a new way. The result is an interesting resource management ethic that, while not entirely new, deserves additional attention. Gottlieb and FitzSimmons give us excellent case studies, which provide insight into the history and politics of regional water agencies. Although they do different things in quite different ways, I would recommend them both.

Northern Arizona University ZACHARY A. SMITH

The Ideology of Apolitical Politics: Elite Lawyers' Response to the Legitimation Crisis of American Capitalism, 1870–1920. By James C. Foster. New York: Garland Publishing, 1990. 167p. $44.00.

The Justice Broker: Lawyers and Ordinary Litigation. By Herbert M. Kritzer. New York: Oxford University Press, 1990. 233p. $35.00.

Social critics and political scientists have long been fascinated by American lawyers and their role in public life. Tocqueville's references to the "aristocratic propensities" of lawyers in America and his observation that "in the United States . . . lawyers, as a body, form the most powerful, if not the only, counterpoise to the democratic element" are cited in study after study in which scholars have probed for the sources of the remarkable prestige and influence enjoyed by lawyers in this country. Political scientists, in particular, have been curious to know (1) why lawyers behave as they do and (2) why they have had such a profound impact on American politics and society.

Herbert M. Kritzer's *Justice Broker* is a careful study of the day-to-day work of attorneys who handle ordinary (i.e., small) civil cases. It is mainly concerned with question 1 and is based on data collected by the Civil Litigation Research Project (commissioned by the Department of Justice) in 1979–80. Kritzer contends that our understanding of the work of ordinary civil litigators can be improved by adding a new theoretical perspective to the traditional image of the lawyer as a "professional." This new perspective conceives of the lawyer as a "broker"—a person hired to act as an intermediary between his client and the courts or, more generally, between his client and all components of our system of justice. The image of the lawyer as a broker contrasts with the traditional image of the lawyer "as an alter ego [of the client], which is the notion typically used when talking about the professional" (p. 12).

The applicability of the brokerage model to the study of lawyers was suggested by its usefulness in the study of local politicians who function as intermediaries between their constituents and larger social and political systems. However, the model does not fully describe the relationship between lawyer and client. Kritzer confesses that at the outset of his study, he expected the image of the lawyer as broker to supplant the image of the lawyer as professional but that his research persuaded him that neither image by itself was sufficient to account for the experience of ordinary civil litigators. Hence, he did not discard the professionalism framework that has dominated modern sociological analysis of the legal profession but decided, instead, to supplement (or "overlay") this framework with the brokerage framework. His claim, which he defends persuasively, is that exclusive reliance on either framework will produce a distorted picture of what ordinary civil litigators ordinarily do.

Kritzer's data reveal that a typical civil litigator's professional life is complicated by a wide variety of concerns, commitments, and responsibilities. Although the lawyer often furthers the client's interests in a more or less direct way, various aspects of the lawyer's self-interest frequently come into tension, or even conflict, with the client's interests. In such circumstances, the image of the lawyer as the alter ego of the client is obviously inadequate. Kritzer concludes that "in some ways the broker image is better for explaining the work

of lawyers in ordinary litigation, while in other ways the professional image is better" (p. 19).

Kritzer's study is a model of competent social-scientific inquiry into a complex social phenomenon. It is dispassionate, thorough, analytically rigorous, and judicious in generalizing and drawing conclusions from the data. Although Kritzer is methodologically self-conscious, he lets the data call the tune and refrains from manipulating his findings to vindicate a preordained theoretical agenda. In reading *The Justice Broker*, one senses (and shares) the author's satisfaction in achieving a more complete understanding of the complicated lives and roles of ordinary civil litigators.

Kritzer's valuable book stands in sharp contrast with James C. Foster's polemic against American lawyers. *The Ideology of Apolitical Politics* is as tendentious as Kritzer's study is fair-minded. In Foster's book (which is mainly concerned with question 2) a political agenda, not the data, calls the tune; and it is a strain that we have all heard before.

Foster's agenda is avowedly neo-Marxist. He means to prove that "in this society, *lawyers are the liberal guardians of capitalism*" (p. 1; emphasis original). He understands his project as "a reworking of Tocqueville's views on the legal profession" (p. 3).

Foster draws heavily on influential works on the legal profession by Arnold Paul and Jerold Auerbach, as well as on the writings of scholars associated with the critical legal studies movement. The general theoretical framework is informed by Gramsci's understanding of "hegemony," Therborn's notion of "mechanisms of subjection," and Habermas' idea of the "legitimation crisis." Unfortunately, Foster makes virtually no effort to engage serious critics of the thinkers on whom he relies. The result is a string of undefended assertions supported by little more than citations to the works of controversial leftist academics.

Here is Foster's basic story. American capitalism, which he defines as "a social formation characterized by private ownership and/or control of productive property used for profit" (p. 1), is legitimated by liberalism, which he understands as a legalistic political ideology that portrays human relationships fundamentally in terms of rights and rules. The cherished ideal of the rule of law is merely a mask for the rule of lawyers. Lawyers are "mongers of rights and rules" and "professional liberals largely in capitalists' employ" (p. 146). Law in liberal society is nothing more than politics masquerading as something apolitical. The prestige and influence of lawyers derives from their strategic role in serving the interests of their capitalist masters by sustaining "the ideology of apolitical politics."

Foster seeks to persuade readers of the accuracy of this story by recounting the activities of "conservative" and "progressive" lawyers from 1870 to 1920, "a period of social discord 'close to outright class conflict'" (p. 9 quoting Arnold Paul). During this period, the American Bar Association was organized, together with bar associations in 47 states (p. 45). Foster lays special stress on speeches given by elite lawyers at meetings of these associations throughout the country. He purports to show how "legal conservatives" (fundamentally "Hobbesian" in grounding civil government in rules based on fear of political chaos) and "legal progressives" (essentially "Lockean" in rejecting political absolutism and basing government on rights) sought in different ways to legitimate capitalism by sustaining the ideology of apolitical politics.

If one imagines reading a study of atheists by Rev. Jerry Falwell, one will have a pretty good idea of the tone of Foster's analysis of legal elites. Having already decided what they were *really* up to, Foster works and reworks the data to make them validate his neo-Marxist story.

Foster maintains that by sustaining the ideology of apolitical politics, lawyers continue to obscure the vision of the great majority of Americans who are victims of capitalist exploitation. He admits that his analysis does not square with "the way most Americans perceive their lives," a regrettable fact that merely confirms for him Therborn's thesis that "ideology is a powerful mechanism for maintaining power" (p. 149). Hence, Foster declares that "if Americans could see existing social reality clearly we might freely imagine a more just . . . alternative and act to bring it into being" (ibid.). Recognizing that "such a development would spell the end of the(ir) world, lawyers, as well as the capitalist interests they represent, . . . employ all [the] ideological mechanisms of subjection to ensure capitalism's survival" (ibid.).

Someone interested in understanding contemporary lawyers and their work is likely to learn a great deal from Kritzer's study of civil litigators. Someone interested in a sober critique of the role of lawyers in American political and economic life is unlikely to gain many insights from Foster's book, though as an example of fierce polemical writing, it might be hard to beat—at least until Rev. Jerry Falwell writes that book about atheists.

Princeton University ROBERT P. GEORGE

Islands under Siege: National Parks and the Politics of External Threats. By John C. Freemuth. Lawrence: University of Kansas Press, 1990. 200p. $25.00.

Islands under Siege focuses on two case studies of Washington-level policymaking as it affects national parks. One concerns legislation and regulations affecting tar sands leasing in and near the Glen Canyon National Recreation Area, the other the evolution of efforts under the Air Quality Act to protect parks from visibility impairment.

The analysis is low-key, appropriately viewed as storytelling, and contains none of the didactic partisanship that mars much popular and professional discussion of national park issues. Freemuth has also abjured the theoretical and methodological heavy-handedness that render much social science literature impenetrable to citizens, undergraduates, and resource managers. That is the book's greatest virtue—the narratives introduce the general reader to tar sands leasing and visibility issues, both in general and as they affect national parks. Freemuth's careful and informative introduction to visibility is illustrative: it weaves technical concepts such as scatter, absorption, and their relationship to particular airborne chemical pollutants with the political jargon of SIPs, BARTs, integral vistas, and regional haze. Moreover, the author has included several useful appendixes, including a summary of major National Park Service legislation and a brief introduction to different types of units in the national park system. The book is a solid contender for a resource-policy-, legislative-process-,

public-administration-, or interest-group-oriented reading list and a valuable point of entry for non–political scientists interested in either the cases or the national parks.

Because of that clear priority, the volume may disappoint specialists. It is not ground-breaking for either the discipline or for students of the public lands. In the absence of clearly stated and consistently treated hypotheses, the author's discussion lacks edge and the conclusions are somewhat uninteresting. For instance, "It is obvious that both the political process and chance played a much larger role in this issue than rational land-use planning" (p. 49). Again, "It is hardly surprising, then, that the agency in question will interpret the law to correspond to that agency's norms and values" (p. 55). Finally, "Once an area is under Park Service administration, concern for overlooks and park visitation can drop off the preservationists' agenda." (p. 83).

Freemuth alludes repeatedly to issues of major theoretical and practical significance. The role of science in public management is traditional fare for the political scientist and has been the focus of public concern, most intensely in the national park context, since the Yellowstone and Yosemite fires and Alston Chase's popular volume, *Playing God in Yellowstone*. Freemuth does not invoke the literature or enter into the debate. Similarly, he does not build on the organizational behavior literature, which features such standard gems as Herbert Kaufman's *Forest Ranger* and Ashley Schiff's *Fire and Water* to compare the Park Service to the U.S. Forest Service. Nor does he comment on Bureau of Land Management–Park Service relations, central in the tar sands case, or Environmental Protection Agency–Park Service relations, central to the visibility issue. Both the extensive literature, which includes the more recent work of John Baden, Terry Anderson, and Gary Liebcap (all with a public lands focus) and the comparisons inherent in the two cases are missed opportunities in Freemuth's slim volume. And although he seems primarily concerned with the quality of interest group advocacy in behalf of national parks ("If we can come together in our love for our parks, then the external threats issue might be resolved," p. 143), he does not talk very much about interest groups either in theory or in context.

A sharper focus and analytical framework would not have threatened the author's commitment to entry-level comprehension. And although the appendixes include a brief and useful discussion of some of the classic studies in Park Service policy (Runte's *National Parks*, Foresta's *America's National Parks and Their Keepers*, and Ise's much earlier *Our National Park Policy*), he has omitted essential references such as Sax's *Mountains without Handrails* and has not made use of this literature in structuring his discussion. The volume suffers, one might argue, from being a bit of an island itself.

As a result, numerous central issues that ought to be raised are not. Most importantly for public lands scholars, the context of Freemuth's islands–external threats treatment of the parks is rich with significant questions. The island concept featured in the title is a familiar but not obviously appropriate metaphor for a national park. Similarly, the notion of external threats is problematic. In theory, since the classic case of *Camfield* v. *United States* 1897 and certainly since Joseph Sax penned his major article "Helpless Giants" in a 1976 issue of *Michigan Law Review*, there has been clear basis for asserting that the federal government has considerable authority to protect its property from threats arising on adjacent lands. It is not clear, then, why Freemuth insists without discussion both that the external threats issue "is a relatively new one" and that the federal Government has "no regulatory control over development outside [park] boundaries" (p. xii, 9).

Finally, along with discussing approaches to problems such as visual impairment, Freemuth ought to have helped us ponder why external threats have become the focus of so much recent discussion of national park degradation. Many would assert that the external threats over which, ostensibly, the Park Service has inadequate control are not a patch on the problems that the agency itself causes with proconcession and pro-tourist-oriented policies and/or bad science. Clearly, external threats and islands in themselves are political notions that ought to be analyzed, rather than taken at face value. Loving national parks does not necessarily make one love the National Park Service—or should not. That said, Freemuth's carefully told and easily penetrated stories are still a good place to begin an evaluation of the issues he presents.

University of California, Berkeley SALLY K. FAIRFAX

The Boundary Waters Canoe Area: Wilderness Values and Motorized Recreation. By James N. Gladden. Ames: Iowa State University Press, 1990. 166p. $19.95.

The Quetico–Superior boundary waters region provides the canoeist a peak wilderness experience paddling pristine lakes and portaging canoes and "duluth" packs between lakes—plus some great fishing. This region consists of two major management units, the Boundary Waters Canoe Area (BWCA) within the Superior National Forest in northeast Minnesota and Quetico Provincial Park across the international boundary in western Ontario, totaling 2.225 million wilderness acres, plus the new, small Voyageurs National Park at the western end of the BWCA. As a great reserve that is unique in North America, the Quetico-Superior area is a mecca for eastern and midwestern wilderness lovers and serious canoeists.

James Gladden describes the battles over management of the BWCA portion of the area. The 1964 Wilderness Act specifically incorporated the EWCA as a flagship unit of the national wilderness preservation system. However, the BWCA's status in the system, since its reservation as a roadless area in 1926, represented an anomaly because there was a long history of motor use in the area. From 1956, local residents and resort operators fought to retain rights to continue preexisting motorized uses (motor boats and later snowmobiles) and regarded provisions in the Wilderness Act as guarantees of their uses.

For preservationists, motor use within the Quetico–Superior area is anathema. The minimum criterion of a statutory wilderness in the United States is that it must be roadless precisely because motorized transportation is antithetical to the concept of wilderness. Gladden points out, interestingly, that wilderness canoeists's notions of technological purity are somewhat self-deceptive. Motors are deemed a philosophically incorrect intrusion of modern technology into the solitude of the park. Yet the equipment used by the wilderness enthu-

siast is hardly the primitive gear of the French voyageurs. In any case, preservationists and local promoter interests fought a running battle into the 1980s over the size of the BWCA management unit that would be closed to motorized use. The pivotal point in this story was the passage of the BWCA Wilderness Act of 1978.

Gladden's case study focuses on two major interest group protagonists—the preservationist Friends of the Boundary Waters Wilderness and the promoter Boundary Waters Conservation Alliance—and the difference in the resource use values of the two groups. The former are characterized as romantic preservationists for whom BWCA wilderness canoeing is a physical test in an area that provides none of the supports of modern technological society. On the other side, the locals in Ely, Minnesota regard nearby federal lands as their land, to be used as they wish and with whatever energy-consuming conveniences they prefer, and resent the intrusion of "outsiders," federal bureaucrats, and "politicians" (i.e., Congress) into their freedom of choice.

Unfortunately, Gladden's book is only a mildly interesting case study, though one that would be useful to scholars of natural resources policy and students who wish to study decision making in a major public lands issue. A case study can certainly be the foundation of a fine book. But a good case study should include one or more special traits. The case study could provide factual examples for some general theory of political processes. The classic example of this genre is Graham Allison's *Essence of Decision* (1970). Or a good case study could provide an insightful description of the particular policy process or use state-of-the-art social science methods to document its argument. Finally, a truly fine case study should be strongly written.

By these tests, Gladden's book is unsatisfying. First, his stab at a philosophic framework is a flop—a reference to an article by Riley Dunlap from the 1980 *American Behavioral Sciences* that borrows Thomas Kuhn's metaphor of paradigms to restate concepts of biocentricity and anthropocentricity that are well understood to be fundamental to philosophies of resources conservation dating back to the nineteenth century. Beyond that, Gladden's analysis of the values of the protagonists is limited to minor caveats: the prowilderness coalition ignored the history of motor use prior to the Wilderness Act, for example, while the locals had no conception of motors' disturbance of the wilderness experience of canoeists and cross-country skiers.

Second, Gladden's description of the BWCA policy process oversimplifies the protagonists by focusing exclusively on the pro- and antimotor coalitions and adopts a simplistic definition of pluralist politics as the result of the competing preferences of the two coalitions. Gladden particularly skips over the trials of the Forest Service, the agency that manages the BWCA. The BWCA Plan was the Forest Service's most controversial land use plan of the 1970s, with 1,013 comment letters—a foretaste of the mailbag wars of 1980s national forest planning. Moreover, Gladden's descriptions of both the legislative history of the 1978 BWCA Act and the subsequent litigation are one-dimensional and flat.

Third, the study presents generalizations about the values of the two coalitions but no systematic evidence or analysis about the character of the constituent groups. What, in particular, is the variance among group members' attitudes, and how well do the range of members' attitudes and beliefs fit the generalizations Gladden attributes to the coalitions? Or what are the sources of group members' values? Wilderness lovers seem to come from a different sociopolitical subculture than snowmobilers and motorboat users.

Finally, Gladden's book does not read well. I can assure readers that participants in local public lands politics are passionate, articulate, fascinating folks. But Gladden's wooden prose just does not convey either the fire in the belly or the sophistication with which the participants fight out the issues. Nor, ultimately, does it do justice to the beauty and majesty of the Quetico–Superior region itself.

Quetico Provincial Park PAUL J. CULHANE

The Cynical Society. By Jeffrey C. Goldfarb. Chicago: University of Chicago Press, 1991. 299p. $22.50.

In *The Cynical Society*, Jeffrey Goldfarb argues that "the single most pressing challenge facing American democracy today is widespread cynicism" (p. 1). He believes that the political parties, the mass media and culture industries, the educational system, and movements of both the Left and the Right contribute to this problem. Yet Goldfarb also believes that there is potential for a genuinely democratic culture in the United States, and he calls for a renewal of democracy as a counter to the cynical society which has emerged in recent years. In carrying through this project, Goldfarb offers challenging theoretical perspectives on cynicism, a sharp critique of contemporary politics and culture, and a wealth of illuminating insights into our contemporary malaise.

In an illuminating discussion of cynicism as a philosophy, Goldfarb indicates that it originated in Greek philosophy with Diogenes who utilized cynicism as an instrument of critique of the dominant society from the position of the powerless. Later cynicism, however, developed into a philosophy of resignation and became increasingly cynical in the modern sense of the word. In the contemporary era, Goldfarb believes that cynicism has functioned largely as an instrument to defame criticism of the existing order and programs for social change. In addition, he believes that it has become a mass attitude that compounds powerlessness and resignation to the existing order. For Goldfarb, cynicism is thus a block to progressive social change both on the intellectual and mass-political level.

Like Daniel Bell, Goldfarb appears to blame the ills of the present age on the sphere of culture. He writes, "The major problem of mass society is its cynical culture" (p. 51). Further, "we must recognize that mass culture is a response to the underlying tensions in democratic ideals and practices" (ibid). Goldfarb's position, however, is more subtle than those neo-conservatives who would blame the ills of the polity on contemporary culture alone, while defending the economic and political system. Goldfarb recognizes that it is the control of both politics and culture by a corporate elite which uses the state and culture to advance its own economic interests which is the problem with contemporary culture and politics and not merely contemporary culture. Yet unlike Marxists who simply posit that the capitalist class controls culture and politics and leave it at that, Goldfarb analyzes the complex web of mediations that constitutes contemporary culture as cynical. He notes that corporate control of culture puts commercial inter-

ests before the interests of enlightenment, the promoting of democratic debate, or producing a quality culture. But he recognizes that there are contradictory impulses within contemporary culture between democratic and commercial forces and that, often, genuinely democratic and commercial considerations are interwoven.

For example, Goldfarb interprets the rise of public education, the development of public lecture circuits in the 19th century, the mass publication of books, and public libraries as democratic developments that attempted to educate the public so that individuals could participate within public life. But he also notes that commercial concerns compromised the public lecture circuits once agents began organizing lecture tours (pp. 56ff) and that publishing has also become overly commercial in the present age, putting profits before public enlightenment and reasoned debate (pp. 58ff). Above all, however, Goldfarb sees the centrality in contemporary culture of a commercial media system as the key constituent of a cynical culture and impediment to development of a democratic culture.

Yet Goldfarb does not analyze the role of media ownership or the political economy of the media as contributing to the increasing commercialization of media culture. During the 1980s, in the wake of Reagan's deregulation policies, major corporations took over the mass media and have arguably used them to promote a conservative agenda. Goldfarb, however, does not discuss the consequences of corporations like GE taking over NBC or the Time-Warner merger which, in my view, pose threats to democracy both from excessive concentration of state, corporate, and media power and by failing to adequately inform the public or debate issues of national concern.

Most of Goldfarb's book, however, is an ideological critique of contemporary culture and politics rather than a structural analysis of the U.S. polity and economy. In general, Goldfarb is very good in his cultural analysis and ideological critique and weaker in analyzing economic, political, and social mediations that constitute the current crisis of democracy in the United States and what he calls the rise of cynical culture. Goldfarb provides an adequate but not particularly novel analysis of the rise of the New Left and New Right and criticizes both for falling prey to ideology and using cynical appeals to attract the masses to their ideological politics. His strategy is to argue that both the New Left and the New Right are equally cynical in their politics, as are contemporary Republicans and Democrats. But this ploy collapses Bush and Dukakis, Republicans and Democrats, and Left and Right into one cynical essence, overstating similarities while occluding differences. A more adequate analysis, therefore, would analyze the specific modes of cynicism and attempt to carry through a more concrete and specific discrimination between the dominant modes of politics and discourse today.

Such an analysis, I believe, would reveal Bush and the Republicans to be especially cynical in their politics. Bush is arguably one of the most cynical politicians that has ever emerged into prominence, sacrificing a career of moderate Republican centrism to the hard-right ideology of the Reagan era and then fluctuating from moderate to rightist conservative positions during his own presidency, failing to articulate any coherent position. Indeed, there are deep suspicions that Bush has consistently lied to cover over his activities during the Iran/Contra affair and other scandals of the Reagan era and I would argue that Bush regularly engaged in lies and hypocrisy throughout the prelude, duration, and aftermath of both Iran/Contra and the Gulf war, two of the most cynical episodes in U.S. political history. In addition, Bush's promotion of Clarence Thomas as the best man for a position on the Supreme Court and his naming his crony Robert Gates as the best man to head the CIA despite a dismal record of failure during his tenure in the CIA represent rather extreme cynicism. Thus I believe that Bush requires a separate study of his politics of cynicism and that Goldfarb's attempt to balance discussion of Bush's cynicism with the Democratic Party's cynicism represents apolitical liberalism which fails to make key discriminations.

The University of Texas at Austin DOUGLAS KELLNER

A General Theory of Crime. By Michael R. Gottfredson and Travis Hirschi. Stanford: Stanford University Press, 1990. 297p. $39.50 cloth, $12.95 paper.

Theories about crime abound. There are too many to summarize here, but they may be characterized in terms of their political values. Conservatives tend to see the cause of crime as rooted in individual deficiencies. Wilson and Herrnstein's *Crime and Human Nature* (1985), for example, argues that although there is no criminal gene, traits that are to some degree inheritable, such as intelligence and temperament, affect the likelihood that people will engage in crime. Liberals, on the other hand, tend to believe that social and economic factors are the major causes of crime. Gottfredson and Hirschi fall into the former camp, for they argue that people who lack self-control are likely to commit crime. These people tend to have unstable marriages, friendships, and job profiles and few skills and to be self-centered and insensitive to the suffering of others. The major cause of low self-control, they say, is ineffective child rearing. Parental concern for the welfare or behavior of the child is a necessary condition for successful child rearing. When this is absent, then the child will have low self-control.

The authors refute sociological and psychological explanations of crime and argue that self-control is "the only enduring personal characteristic predictive of criminal (and related) behavior, regardless of other dimensions of their personality" (p. 111). Because low self-control develops early in life (before age three), neither rehabilitation nor incapacitation policies will work. Instead, they assert, "effective and efficient crime prevention that produces enduring consequences would thus focus on parents or adults with responsibilities for child rearing" (p. 269).

Gottfredson and Hirschi devote a large portion of their book to refuting biological, psychological, economic, and sociological positivism (part 2) and also trying to discount some of the things that seem to contradict their theory (part 3). For example, crime is committed primarily by males. Does this mean that they have lower self-control than females? (chap. 6). Also, the incidence of crime peaks in the age range of 15–18 and then declines. Does this mean that somehow people are able to learn self-control as they mature? This would run counter to their notion that self-control is learned very early in life. Most difficult for their theory is white-collar crime—crime committed by people who have gained

high positions by exhibiting a great deal of self-control. How can this be explained by their theory? Their answer is that their theory predicts low rates of offending by those in high positions. They reject research that says white collar and corporate crime are extensive and cost the nation far more in money and lives than street crime. In this regard they agree with J. Q. Wilson's *Thinking about Crime*, which also argues that street crime is the real threat to the social contract. The credible evidence, the authors say, shows that white-collar crime does not cost huge sums of money. But it is hard to reconcile this with recent events such as the savings-and-loan fraud, Wall street insider trading and the Housing and Urban Development scandals.

Their theory, they say, is "meant to explain all crimes, at all times, and for that matter, many forms of behavior that are not sanctioned by the state" (p. 117). This, of course, is an extremely ambitious claim. It is almost an attempt to create a theory about life itself, since crime is such an inherent and important part of life. No wonder, then, that they have to perform wonderous mental gymnastics in order to explain away much of the research that is incompatible with their theory.

Although theories about crime that see its cause as a deficiency in individuals, rather than society, are becoming popular, particularly among conservatives, such theories have a long history. But I agree with Emile Durkheim's *Rules of Sociological Method* (1964) that crime is socially constructed and cannot be blamed on an imperfectly formed mind.

Arizona State University DENNIS PALUMBO

Transforming Free Speech: The Ambiguous Legacy of Civil Libertarianism. By Mark A. Graber. Berkeley: University of California Press, 1991. 347p. $39.95.

Today, Americans believe that freedom of speech is a basic human right that plays an essential role in the functioning of our democratic political processes. Free speech has not always enjoyed such a highly preferred place in our society. There have been periods of considerable repression of political dissidents. But (insists Graber) most Americans—including most academics—wrongly assume that the strong contemporary position is based upon a uniform tradition of free speech interpretation dating back to the framing of the First Amendment.

In fact, Graber argues, there have been two differing constitutional conceptions of free speech. A conservative interpretation dominated American discourse from the Civil War until World War I. For the remainder of the twentieth century, a liberal interpretation based upon the functional relationship of free speech to democratic government has been prevalent. *The Transformation of Free Speech* explicates that paradigmatic shift and discusses its consequences.

What Graber labels the conservative libertarian defense of free speech was based upon natural rights theory. Most political scientists are aware how natural rights theory was used to create a constitutional order that provided a strong protection for private property. Graber emphasizes how that same matrix of ideas could encompass free speech. A theory of inalienable human rights, Graber shows, was used by some conservative commentators to argue on behalf of the free speech rights of political dissidents against the nationalistic claims surrounding World War I.

At first, the liberal, social science paradigm offered little comfort to civil-libertarians. When industrialization reshaped the socioeconomic infrastructure of American society, intellectuals found it necessary to talk about the need to reconcile competing group interests, rather than the need to protect individual rights. In championing the right of the people's elected representatives to reconcile competing interests, they had no rational basis for immunizing free speech from the instrumental calculus. This led to a series of judicial standards for interpreting the First Amendment, particularly during the early Cold War period, which hardly seemed to recognize the existence of a constitutional right of free speech. Justice Felix Frankfurter's opinion in *Dennis* v. *United States* 1951 is the leading example of this failure of liberal theory.

Gradually a persuasive defense of free speech emerged. First, following John Stuart Mill, civil-libertarians maintained that free speech is essential for formulating sound social policy. Second, they argued that their understanding of the relationship between free speech and self-government was embedded in the First Amendment and uniformally subscribed to by all major American thinkers since the founding. Both elements of this argument enabled liberals to urge strong judicial protection for all political ideas in the name of American democracy.

Graber—perhaps unduly—maintains that Zachariah Chaffe, Jr. was the galvanizing figure in this development. Be that as it may, Graber clearly shows how the new construct, while historically inaccurate, became the prevailing constitutional interpretation. Given the structure of his argument, he is also able to note its fragility. *Brandenburg* v. *Ohio* 1969 is the leading case. Yet as Justice Douglas's concurrence noted, the Court's opinion was still another balance test; and in times of great stress, judges have generally permitted asserted national interests to outweigh a dissident's rights to free speech. The current conservative Supreme Court's instinctive deference to executive power and its strong concern for social order also makes the survival of the prevailing civil-libertarian interpretation problematic.

More important to Graber is the fact that Chafee and his disciples sharply distinguished between political speech (which warranted strong judicial protection) and socioeconomic matters (which were subject to all regulations governmental officials believed reasonable). Political ideas and campaigns sponsored by the wealthy were thereby immunized: "Elected officials have been constitutionally barred from equalizing participation in the marketplace of ideas by limiting the economic resources persons can bring into that forum"; and "disparate economic holdings are affecting the capacity of elected officials to deliver programs responsive to the needs and sentiments of a majority of Americans" (pp. 186, 189).

Here Graber's failure to tie his constitutional arguments more directly to political theory creates problems. The role of money in contemporary American politics does not pose an insuperable problem for civil-libertarians working within their own paradigm. That is why some commentators have begun to address the issue (as Graber recognizes, pp. 213–15). Rather, Americans are constrained by their more general beliefs. The Western liberal tradition has never favored an egalitarian approach to property. Plainly, the conservative adherents

of natural rights theory would never have argued for a limit on property rights in order to advance free speech. Similarly, liberal adherents of the welfare state are willing to regulate the economic market but not to supplant it. They, too, believe that people are entitled to benefit from the fruits of their labor.

Graber's concluding recommendations are based upon Michael Walzer's *Sphere's of Justice*, but they could just as readily be grafted upon the existing paradigm. They are eminently sensible. A liberal electoral majority might well enact them and a liberal Supreme Court might well sustain them. But it is doubtful whether that would satisfy Graber. He apparently desires a truly major transformation, one based upon communitarian theory. Graber neglects to explain why, after more than two hundred years, Americans are likely to abandon a political order based on liberal political theory for a communitarian vision of society. American conservatives and liberals both appear to believe that Madison's arguments in *Federalist* 10 are conclusive on this point.

This book reflects the best aspects of contemporary free speech scholarship, and the worst. Graber provides a persuasive account of how civil-libertarians came to adopt the prevailing constitutional paradigm and an illuminating description of that construct's strengths and weaknesses. Unfortunately, Graber's discussion of free speech conceptions is not closely tied to more general democratic theories. The absence of that broader political theory framework reduces both the force of Graber's criticisms of the prevailing liberal paradigm and of his admittedly modest prescriptive suggestions.

State University of New York, Albany MARTIN EDELMAN

Impossible Jobs in Public Management. Edited by Erwin C. Hargrove and John C. Glidewell. Lawrence: University Press of Kansas, 1990. 213p. $29.95 cloth, $14.95 paper.

Amid the swirling controversies over appropriate public responses to problems such as AIDS and the mentally ill homeless, psychologist Erwin Hargrove and political scientist John Glidewell offer a stunningly simple observation: some public tasks are impossible. The contributors to *Impossible Jobs in Public Management* show how and why jobs grow to become impossible. Yet theirs is not a counsel of despair; for the main focus of the book is to examine how top agency officials cope with impossible tasks. Much like the successful organizational innovators profiled in Lewis's *Public Entrepreneurship* (1980) and Doig and Hargrove's edited *Leadership and Innovation* (1990), officials who are "professional–political entrepreneurs" seem to stand a better chance in their struggles with so-called wicked policy problems.

The analysis proceeds in two steps. In part 1, Glidewell and Hargrove draw on both fields to explain why some public tasks become impossible and to sketch appropriate coping strategies for top agency officials. They introduce four dimensions along which public jobs can be arrayed. Jobs falling at the *difficult* end of all four continua are those classified as *impossible*.

Six impossible jobs are identified for detailed examination in part 2. DiIulio focuses on commissioners of corrections, Miller and Iscoe on state mental health commissioners, and Lynn on social welfare executives while Lawlor writes of the challenges the AIDS crisis poses for public health officials, and Byers examines the task of special masters in monitoring court-ordered prison reform.

These part 2 essays vary in the extent to which they rely on the conceptual discussion in part 1. Yet all do an excellent job of elaborating on the impossibility of the particular tasks they focus on. Less uniformly compelling are the analyses of, and prescriptions for, officials' coping strategies. Especially thoughtful and informed by the cases they examine are the DiIulio, Lawlor, and Lynn essays. In contrast, Miller and Iscoe conclude with a list of recommended strategies for state mental health commissioners that reminds one of the infamous "proverbs of administration." Still, none of the essays claims that there is one best way to cope with impossible tasks; and most underscore the importance of the fit between individual managers and the political and policy situations they confront.

Impossible Jobs in Public Management may be valued most because it so runs against the current of much contemporary public discourse. It is a useful corrective to the bureaucracy bashing that has grown in volume and intensity over the last two decades. The contributors argue convincingly that an important explanation for government ineffectiveness is that some policy tasks inexorably trigger irreconcilable controversy over the goals that should guide public response (e.g., Should imprisonment rehabilitate, deter, punish, or isolate criminals from society?). Meanwhile, these same tasks typically involve clients (convicted criminals, welfare mothers) whom citizens both understand as needy and perceive to be unworthy. Given weak policy technologies and the absence of acknowledged experts to guide policy, the political system veers almost drunkenly between commitments to, and attacks on, public action.

That top agency officials might have an impact in these difficult policy arenas would be a useful datum in ongoing debates over what public agencies at all levels of government can and should do. At this point, however, perhaps the major limitation of the analysis surfaces, namely, that *better* and *worse* coping by agency managers are not clearly defined. Possibly, "better" copers might be those who can foster some improvement in one or more relevant policy indicators. Yet if the task is indeed impossible, this is an inappropriate evaluative criterion, since either such improvement cannot be achieved, or else it will be merely transitory or produce unanticipated negative consequences, or it will reflect how little a manager could have accomplished. Ultimately, most of the contributors fall back on another standard of success—the length of tenure of the top executive. One who stays longer must be coping better than one whose term is shorter. Whether longer tenure is desirable from the standpoint of clients, constituents, or the political system as a whole is far less apparent. Indeed, many would view the acceptance of such a standard as part of the problem in public management.

Nonetheless, *Impossible Jobs in Public Management* is a useful and thought-provoking book. Its examination of specific policy areas offers compelling accounts of the conflicts and frustrations associated with many of the tasks public agencies undertake. Scholars of public bureaucracy may well lament the lack of systematic attention paid to internal agency dynamics, and agree with DiIulio that "broad contextual variables" are probably

more important in explaining agency performance than is the top executive (quoted p. 68). Even so, Hargrove and Glidewell's argument for the key role of agency entrepreneurs is worth both careful consideration and further scrutiny.

Virginia Polytechnic Institute and State University KAREN M. HULT

Restructuring Justice: The Innovations of the Ninth Circuit and the Future of the Federal Courts. Edited by Arthur D. Hellman. Ithaca: Cornell University Press, 1991. 385p. $42.50 cloth, $13.95 paper.

Caseloads of the U.S. circuit courts of appeal have grown dramatically over the years. The traditional congressional response has been to add judgeships to the existent courts, rather than split the busiest circuits and allocate the new judgeships among them. But in 1975, the congressionally established Hruska Commission concluded that this traditional solution was growing less and less viable because at some point, ever larger appellate courts become unwieldy and undermine justice. (For example, inasmuch as the circuit courts, with rare exceptions, decide cases by randomly drawn, shifting panels of three, the possibility of contradictory rulings and precedents increases along with the total number of judges on each court.) The Hruska Commission thought that no circuit court should have more than nine judges. Beyond that point, the circuit should be split, with each of the new circuits having its own circuit court of appeals.

The commission recommended that Congress split the two largest circuits: the Ninth and the Fifth. Congress did divide the Fifth Circuit; but the Ninth, under the aggressive leadership of recently elevated Chief Judge James Browning, fought back and to this day remains intact (although bills to split the Ninth continue to be introduced). Browning's major strategy was to inaugurate a host of reforms designed to prove that despite its huge size, his court could surmount the problems thought by the commission to be inherent in large appellate courts.

This book enters the Hruska–Browning debate over the size of appellate courts. The Ninth Circuit, the book convincingly argues, is an especially appropriate research setting; for it is by far the largest of all the circuit courts, whether measured by caseload (about one-sixth of the total caseload of all 12 circuit courts), number of judges (more than three times as many as the U.S. Supreme Court), or the vastness of the far-flung area it serves (California, Oregon, Washington, Montana, Idaho, Arizona, Nevada, Alaska, Hawaii, and the territories of Guam and the Northern Mariana Islands). The focus is on the Browning era (1976–88); for that is when the most far-reaching reforms occurred (e.g., his attempt to minimize contradictory rulings by increasing communication among panels and by utilizing staff to spot possible contradictions before written opinions were finalized). The authors earn high marks for grounding their research in, and integrating it with, the substantial body of scholarly literature published prior to 1990.

The most consistent theme of *Restructuring Justice* is that the Browning reforms worked well and demonstrate that the Hruska Commission was wrong when it concluded that large appellate courts are necessarily plagued with problems of efficiency and fairness. In fact, the Hruska Commission's alternative of splitting large circuits is singled out for special criticism on the ground that that would only maximize contradictions in federal court precedents; for while even the largest circuit court can work to reduce contradictions among its own rulings, there is no similar coordination among the U.S. circuit courts of appeal (except, of course, for the relatively few cases of circuit conflict eventually resolved by the Supreme Court).

Although this book seems quite objective, I could not help but wonder about the possibility of subtle bias. The editor and several of the authors express great admiration for Judge Browning, to whom the book is dedicated "in recognition of his tireless service to the more effective administration of service." The volume was sponsored "to pay tribute to Judge Browning"; Browning and other Ninth Circuit judges "reviewed drafts for accuracy" (pp. xiv–xv). I should hasten to add that some of the authors present findings suggesting that the Browning court was, indeed, plagued with some of the problems spotlighted by the Hruska Commission.

The editor and several of the authors correctly note that this study has implications far beyond the Ninth Circuit. As the caseloads of both state and federal appellate courts continue to grow, the lessons learned in the Ninth Circuit may help Congress and state legislatures more wisely assess the merits of "circuit splitting" versus adding judges to the old courts. On balance, I think those lessons are well analyzed in *Restructuring Justice*.

Illinois State University GEORGE C. KISER

The Politics of Obscenity: Group Litigation in a Time of Legal Change. By Joseph F. Kobylka. Westport: Greenwood, 1991. 206p. $45.00.

When changes in the membership of the Supreme Court bring about a departure from established doctrine, how do groups active in litigation adapt? In answering this question, the author of the present study attempts to give some order to, and find a conceptual framework for, the study of group participation in litigation. The raw material he has selected for investigation is a list of 119 obscenity decisions handed down by the Supreme Court between 1957 and 1987.

Miller v. *California* 1973 was a watershed decision, marking a turn from the libertarian (or at least permissive) approach to sexually oriented material by the Warren Court. The Burger Court was willing to "leave the determination of obscenity to local authorities and thereby diminish the oversight role of the federal courts in this area of the law" (p. 7). The winners in obscenity decisions before *Miller* were organizations that (like the American Civil Liberties Union) championed unrestricted free speech. After *Miller* these organizations were on the defensive, and groups dedicated to the protection of public morals were in the ascendency. The author has hypothesized that the about-face in *Miller* should affect the litigative activities of groups and provide a basis for theorizing about the dynamics of group resort to the courts.

Kobylka criticizes the existing literature on group litigation on several grounds. It is "plagued by a lack of theoretical depth," focuses on disadvantaged group actors, does not examine behavior in times of legal flux,

and "lacks an awareness of the dynamics . . . of that litigation over time." Isolated case studies "underplay the relationship between group behavior and the general context in which the groups operate" (p. 15).

Kobylka's investigation finds that 24 groups litigated obscenity questions during the 30 years covered by the study. The groups fell into two categories: *purposive* groups interested in public goals and *material* groups with an economic stake in affecting the outcome of the Court's work. Groups on both sides of the obscenity issue used various strategies (sponsorship of litigation, monetary support, and participation as *amici curiae*) to advance their interests. From an inspection of group activity, Kobylka found that the changing legal doctrine after *Miller* affected groups in different ways. Some, like the ACLU, decided to pursue other goals; others continued in much the same mode as before; and a third group, threatened by the decision, mobilized resources for damage control.

In the process of analyzing group responses to *Miller*, Kobylka has isolated primary and secondary factors that affect group decisions to litigate. Those internal to the group include associational type and intensity of commitment, relationship of leadership to members, and the nature and clarity of organizational goals. Those external to the group include the rules of the legal game (legal context), the nature of threats to each group, activities of other groups, and the general sociopolitical climate.

This is a carefully researched book that makes an important contribution to the literature in the field. However, the study does not attempt to quantify group use of different litigative strategies (one suspects that most of the participation is via amicus curiae briefs) and does not investigate activity in the lower appellate and trial courts. After *Miller*, trial court decisions became increasingly determinative of outcome. Kobylka's study is narrowly focused on cases decided at the Supreme Court level.

The notes attached to each chapter whet the reader's curiosity with tantalizing hints about political maneuvering and the details of specific campaigns, but the account leaves out much of the richness of this anecdotal material and concentrates on generalizing about the empirical data. One minor point that I wish had been followed up relates to the campaign against obscenity by radical feminist groups. There is no indication that antipornography feminists have attempted to influence policy by court action. Certainly, they comprise a politically powerless group. Existing theory would predict some such participation.

North Carolina State University EVA R. RUBIN

Supremely Political: The Role of Ideology and Presidential Management in Unsuccessful Supreme Court Nominations. By John Massaro. Albany: State University of New York Press, 1990. 272p. $49.50 cloth, $16.95 paper.

This book has redeeming features. Its detailed examination of presidential management in Supreme Court nominations in the modern era is a contribution to scholarly understanding of judicial politics. Massaro's "development of a common framework of analysis" (p. xi)—a focus on both the timing of High Court vacancies and party control of the U.S. Senate as indicators of presidential success or failure for nominations—will prove helpful to students of the judiciary.

His chapters on the unsuccessful Johnson–Nixon era nominations of Abe Fortas, Clement Haynsworth, and G. Harrold Carswell are useful, especially in his use of archival materials that offer some fresh insights into presidential and senatorial motivations at key stages of the confirmation process.

The work is flawed by its conclusion that when the Senate's October 1987 rejection of the Bork nomination is compared to the refusals to confirm Fortas, Haynsworth, and Carswell, "continuity rather than change in the factors accounting for the Senate's negative action is evident" (p. 191).

Massaro does not recognize that the Bork confirmation struggle represented both a qualitative and quantitative transformation in the politics of judicial confirmations. He completely neglects the impact of high-dollar paid political advertising (primarily, but not exclusively, financed by Norman Lear's controversial People for the American Way) on Bork's confirmation process.

The unprecedented mobilization of more than three hundred activist organizations of the Left in opposition to Bork is given passing reference here; and the underfunded late mobilization of several dozen conservative groups in support of the nominee is completely neglected (save for one reference on p. 186 to Coalitions for America, the umbrella organizing group that tried to rally the jurist's supporters). In short, the most distinctive features of the Bork confirmation battle get short shrift. This weakness is surprising, because Massaro's book appeared after the publication of Ethan Bronner's *Battle for Justice* (1989).

Even though Massaro's analysis of the Bork battle has some merit (he helps place the public pronouncements and private motivations of key senators in the context of other rejections of nominations in recent decades), there is a tendency to accept at face value the formal statements of key liberal senators.

This presents a particular problem for sound analysis of the Bork fight. Massaro accepts Howell Heflin's charges that Bork was extreme in his philosophy as a seriously held opinion. No mention is made of Heflin's scandalous use of religious grounds when he justified his vote to fundamentalist Alabamans. Similarly, moderate Democrat Dennis DeConcini's protestations about the nominee's sensitivity to women's rights are taken seriously although readers are not told about an explicit threat that DeConcini would face primary opposition in 1988 if he supported Bork's confirmation.

Massaro believes that Abe Fortas had helped to build "one of Washington, D.C.'s most prestigious law firms" and "had earned a reputation as one of the nation's finest and most powerful attorneys" (p. 219). Robert Bork's biographical sketch offers no such plaudits (p. 254). It is granted he was "one of the leading advocates of the doctrine of original intent" (p. 159). Republican senator Robert Packwood of Oregon, a leading advocate of abortion on demand, is a "conservative" (p. 88). The weaknesses of this study are revealed in such assumptions: Packwood is conservative, Fortas distinguished, and Bork extreme.

On the one hand, readers are instructed that the judicial mainstream is represented by a philosophy Americans have rejected for a quarter of a century. On the other hand, the judicial philosophy now verging on dominance at the U.S. Supreme Court is extreme; and

those who have worked for its implementation in the judiciary are not worthy of mention. Massaro's study is recommended, but with the reservations noted.

Daily Oklahoman PATRICK B. MCGUIGAN

The Politics of Hispanic Education. By Kenneth J. Meier and Joseph Stewart, Jr. Albany: State University of New York Press, 1991. 275p. $19.95.

This is an important book offering a wealth of new information on Latino access to equal educational opportunities. Meier and Stewart analyze the quality of education for Latino children in 142 school districts with at least 5% Spanish-surname enrollment from 1971 to 1986. Most of the data for this study is taken from Mexican–American school districts, with some comparative data on Puerto Ricans in the Northeast and Cubans in Florida.

What emerges from *The Politics of Hispanic Education* is a pattern of systematic discrimination against Latino school children. While contemporary Latino students are no longer subject to the blatant abuses of racial segregation, they suffer a myriad of other malevolent practices. This process is what the authors call "second-generation" discrimination, policies that deny Latinos the educational opportunities available to Anglos. The authors argue that all that is negative about the American educational experience (tracking, expulsions, dropouts, corporal punishment, etc.) is associated with being Latino and that all the positive aspects (high school graduation, accelerated learning programs) is not.

One great strength of this work is that it will appeal to a wide range of readers. It addresses important issues in educational policy and fits in well with the literature on political mobilization, minority politics, and public policy. For example, the authors demonstrate the inherently political nature of the American educational process and how power relations between Anglos and Latinos determine the allocation of resources in a given school district. They document a clear association between Latino socioeconomic status, numbers, and system of local representation (ward vs. at-large) on the one hand and Latino representation on school boards, in administration, and on the school faculty on the other. These factors, in turn, affect Latino student's share of educational resources and their ultimate success in the school system.

Meier and Stewart also present two disturbing findings that will be of interest to policy analysts and political activists alike. First, bilingual education is not the panacea that some educational reformers thought it would be. Not only have bilingual education programs failed to boost Latino educational attainment, but under the control of biased school administrators, it has become yet another means of internally segregating Latino children. Moreover, the authors' data uncover a *negative* correlation between bilingual education programs and school success for Mexican–American and Puerto Rican students. These findings are all the more troubling because Latino public opinion fully supports the concept of bilingual education.

Second, Meier and Stewart find a positive relationship between the number of black students in a school district and Latino educational success. The authors hypothesize that as the black student body of a school district grows, they soon become the main focus of discriminatory policies. As Latino students cease to be the center of racial animosity, their educational attainment goes up. If this finding can be generalized to other situations, it implies an unfortunate predisposition for whites to make peace with Latinos in order to isolate blacks.

The book's major limitation is a data set that does not allow for an analysis of Latino educational success by generation. The literature on Latinos and mobility emphasizes that generation (e.g., first, second, third) is strongly and positively correlated with educational attainment, professional status, and income. Although Meier and Stewart document the disadvantages that all Latinos face in the school district, it is unclear how these variables impinge on the children of different-generation immigrants.

A second problem is that the book does not advance our theoretical understanding of racial politics and its underpinnings. The authors test a set of well-thought-out hypotheses with obvious theoretical implications, but do not create or build upon a dynamic model of racial politics. This is an unfortunate omission, given the authors' obvious command of the concepts and debates in the field. The first three chapters of *The Politics of Hispanic Education* consist of a detailed literature review of Latino educational politics. Finally, the authors' rich overview of the research on Latinos, politics, and education lies in stark contrast to their dry, methodical march through the data set. The ample use of jargon and the presentation of hundreds of statistical relations without a clear theoretical framework will make for slow reading for a non–quantitatively inclined audience.

Despite these problems, *The Politics of Hispanic Education* is essential reading for anyone interested in the politics of Latino education. Meier and Stewart ask the right questions and offer a well-grounded set of policy recommendations that might reverse the schools' failure to educate Latino children.

University of Wisconsin, Madison BENJAMIN MARQUEZ

The Political Failure of Employment Policy, 1945–1982. By Gary Mucciaroni. Pittsburgh: University of Pittsburgh Press, 1990. 317p. $34.95 cloth, $17.95 paper.

Employment policy, as Gary Mucciaroni observes in this provocative and important study, has been relegated to a marginal role in American economic policy. Formed largely as social policy with a welfare mission, it has featured modest goals for expanding employment, budgets too small to permit substantial accomplishment, and constituencies too narrow and weak to provide steady political support. It has been, in short, a political failure.

To document and explain this failure, Mucciaroni analyzes the development of employment policy in the post–World War II era—a period encompassing the Full Employment Act, the Manpower Development and Training Act, the Economic Opportunity Act, the Comprehensive Employment and Training Act (CETA), and the Job Training Partnership Act. On the basis of wide-ranging research, Mucciaroni discusses the historical and intellectual background of policy proposals, the debates and maneuvering in Congress, the implementation of programs, and their economic and political consequences. For comparative perspective, he offers a

parallel (though much briefer) treatment of employment policy in Sweden.

A central argument motivating the political analysis is that employment policy was not intrinsically a feckless undertaking. With appropriate design and the necessary political capacity, it could have been far more activist, ambitious, and closely integrated with economic policy. Such a policy could have used a variety of micro interventions (e.g., labor market monitoring, training and retraining, relocation assistance, employment incentives, and direct job creation) to compensate for business cycles, facilitate adjustment to structural change, and sustain high levels of employment. In contrast with seeking high employment entirely through fiscal or monetary policy, it would not have had massive effects on aggregate demand or inflation. Mucciaroni presents public opinion data to show that an activist employment policy would have had public support and cites evaluation research and experience in Europe to suggest that it could have been workable.

In the central chapters of the book, Mucciaroni develops an explanation of why employment policy has remained peripheral. As an organizing framework, he proposes that the constraints on employment policy can be understood in terms of three sets of factors: interests, institutions, and ideas. He proceeds to develop penetrating, carefully reasoned explanations under each heading. In the concluding chapter, he recapitulates the analysis and distinguishes the separate roles of each set of factors.

The chief difficulty in the configuration of interests has been the political strength of business and the weakness of the liberal–labor coalition. Business influence has prevented a strong national commitment to full employment and has channeled fiscal management toward the use of tax cuts, as opposed to spending increases, for economic stimulation—a constraint that has inhibited the development of active employment policies.

The major institutional barriers to employment policy have been the fragmentation of decision structures and the weakness of central authorities. Following statist theorists like Krasner and Skocpol, Mucciaroni argues that fragmentation has permitted business to block ambitious employment policies and encouraged a proliferation of small, narrowly focused programs that have been impossible to implement effectively. Conflict between the president and Congress delayed the adoption of CETA and led to administrative arrangements that were incompatible with Congress's objectives. The complexities of intergovernmental implementation have led to politically disastrous breakdowns of control.

As for ideas, Mucciaroni observes that employment policy has inevitably been shaped by the predispositions of policymakers and the values and beliefs of the American public. Policy has been constrained by the ambivalence of Americans toward the values of equality of opportunity and equality of result. In particular, the ideological climate has militated against public creation of jobs and proposals to guarantee full employment.

Qualifying his arguments about interests, institutions, and ideas, Mucciaroni points out that the constraints they establish are not immutable; for they can be modified by political learning. Employment policy has evolved because of new information and changes in the views of economists and other elites about what policies are feasible. Indeed, Mucciaroni develops the interesting notion that a group's influence depends on its capacity to contribute to that learning.

Some liberals will probably complain that Mucciaroni overstates the failure of employment policy. Many conservatives will undoubtedly argue that he oversells its potential benefits. But there should be little debate about the cogency of his political analysis. The book is one of the best studies we have of the long-term development of a set of policies and a profound commentary on the limitations of the American welfare state.

University of Illinois, Urbana–Champaign PAUL J. QUIRK

Money, Elections, and Democracy: Reforming Congressional Campaign Finance. Edited by Margaret Latus Nugent and John R. Johannes. Boulder: Westview, 1990. 294p. $31.95 paper.

Reforming our campaign finance system has proved to be an unusually difficult legislative task. There are sharp partisan differences based on competing policy objectives and divergent conceptions of what the real problems are. This collection of 15 essays attempts to clarify what is wrong with campaign finance law and to prescribe remedies for the ailments identified.

The volume includes articles by both scholars and practitioners, who analyze virtually all aspects of the congressional campaign finance system. The authors stick to the chore at hand, namely, to devise realistic reforms that warrant serious consideration by the Congress. Judged by this objective, *Money, Elections, and Democracy* is a clear success.

Although one wishes there had been more on the public and campaign finance, the two contributions that address this subject are first-rate. Ruth Jones's "Contributing as Participation" demonstrates that between 1980 and 1986 the percentage of Americans who donated to any candidate or campaign organization remained stable. There was, however, a significant drop in the proportion of donors contributing to political action committees, accompanied by a rise in the proportion of donors giving to political parties.

Frank Sorauf's "Public Opinion on Campaign Finance" is based on the author's close reading of all the polling data on public attitudes toward the campaign finance system. He concludes that "opinion on campaign finance is largely unorganized and unmobilized." For most Americans, campaign finance is an issue they find "difficult to define, difficult even to understand in all of its complexity" (p. 213). This goes a long way in explaining why, despite the hue-and-cry over campaign finance reform, Congress has not felt obligated to act.

The volume's primary shortcoming is more a reflection of the field than of the choices made by the editors. The common charge that interest groups wield undue influence because of their political action committees' donations is really given short shrift here. Only one of the political scientists in this work (Janet Grenzke) tackles this issue directly. Her article is an excellent review of the roll call analyses of political action committee influence on Congress. Many of these studies show no relationship between contributions and votes; others show a link between contributions and particular votes.

Although Grenzke herself suggests that the roll call studies fail to tell the whole story, the volume does not extend coverage of political action committee influence

beyond her overview. There is no analysis, for example, of the connection between such contributions and legislators who intervene in administrative policymaking. It is likewise ignored how political action committee may influence committee agendas. In short, political scientists have a limited answer on this question because they have generally restricted themselves to a methodological approach that can be used to analyze only part of the legislative process.

While this collection stumbles on political action committee influence, it hits its stride when it looks at the other problems involving political action committees. Topics that are addressed include elections and incumbency (L. Sandy Maisel), accountability of contributions (David Adamany), independent expenditures (Candice Nelson), and loopholes in the Federal Election Campaign Act (Anne Bedlington). In another essay, Clyde Wilcox documents the growing trend toward personal political action committees in the Congress. By 1988, 45 members of Congress had their own political action committees. Wilcox argues that these have become important tools in leadership races and contests for committee and subcommittee chairs.

There are many other fine contributions to this volume, and the range of issues covered makes this an especially valuable collection. The authors' carefully constructed reform proposals offer a comprehensive set of alternatives to current campaign finance practices. Their recommendations as to how to fix our ailing system merit our attention.

Tufts University JEFFREY M. BERRY

Balancing the Federal Budget: The Cure for U.S. Wealth Dissipation. By Ernest J. Oppenheimer. New York: Pen and Podium, 1990. 181p. $20.00.

Ernest J. Oppenheimer is convinced that deficit spending has inflicted enormous harm on the U.S. economy, and this unusual book is his explanation of why and how the federal government should balance its budget. In arguing for a return to fiscal orthodoxy, Oppenheimer presents some provocative points; but neither his approach nor his style is likely to appeal to a scholarly audience. There is little in his presentation that qualifies as serious economic or political analysis; and his descriptions of budget policy are, unfortunately, superficial.

Oppenheimer's central argument is that deficit spending leads to wealth dissipation. As chronic deficits multiply and public debt levels climb, wealth is lost to foreign interests in the form of trade deficits, asset acquisitions, and investment income flows. The domestic economy, in Oppenheimer's view, is further enfeebled by the negative impact of deficit policy on interest rates, inflation, and saving. A credible case can be—and often has been—made for these assertions; but Oppenheimer is not interested in presenting evidence and analysis, nor does he cite sources to buttress his position. Instead, he repeatedly states his undoubtedly sincere belief that deficits and debt lead to economic, if not moral, impoverishment.

To be fair, sincerity of belief has always played a major role in the debate over deficits, as James D. Savage recently demonstrated in his excellent *Balanced Budgets and American Politics* (1988). Since waiting for an economic catastrophe to occur in order to prove deficits are bad does not make sense for even the most circumspect scholar, one should probably grant Oppenheimer some leeway in arguing this point. When he moves to solutions for the deficit predicament, however, one expects a reasonable familiarity with the spending and revenue trends that have created deficit problems. Oppenheimer is not interested in analyzing these trends but, rather, in advocating defense cuts and a gasoline tax.

According to Oppenheimer, the defense buildup of the 1980s remains the single largest cause of budget deficits and, even more alarming, subsidizes the foreign nations we defend, while leading inexorably to economic ruin (pp. 47, 91–94). In support of his first point about defense and deficits, Oppenheimer explains that the Reagan administration tried to pay for its defense program with domestic policy cuts, which Congress fortunately blocked. In fact, total discretionary spending (defense and nondefense) declined from nearly 11% of gross national product to just over 9% during the 1980s. Moreover, discretionary spending has lagged well behind economic growth for several decades; and the relative decline is projected to continue for quite some time, making the impact on deficits problematical.

Oppenheimer not only ignores this decline but argues that all trust fund surpluses (especially social security) be eliminated from any budget-balancing formula. There is no mention of the fact that there are more than 150 trust funds, nor is there any recognition that transfers from general funds explain a large portion of the projected $200-billion trust fund surplus in the mid-1990s. Oppenheimer fails to discuss the conventional view that the on-budget deficit is not meaningful from the standpoint of economic policy. Finally, there appears to be no comprehension whatever of the herculean policy cutbacks necessary to balance the budget under Oppenheimer's requirement.

Nevertheless, Oppenheimer proposes that defense cuts contribute approximately $100 billion to his $300-plus-billion deficit reduction program, with an additional $50 billion to be gained by eliminating unspecified waste in spending programs. The tax program he advocates includes sin taxes, luxury taxes, and assorted soak-the-rich initiatives. Its centerpiece, however, is a gasoline tax, which is slated to contribute some $80 billion in additional revenues. None of these figures is keyed to baselines of any sort. Instead, each is an extrapolation of current dollar levels.

It might well be, of course, that defense cuts and tax increases are desirable. It could even be that the gasoline tax Oppenheimer has advocated for many years "would have such a profoundly positive effect on our nation that its value would at least equal, if not surpass, that of the computer" (p. 111). In any case, it is difficult to evaluate propositions and claims of this sort when there is no evidence, analysis, or sustained argument to back them up.

Oppenheimer concludes several of his chapters with imaginary dialogues with a Democrat and a Republican, respectively. Oppenheimer uses these conversations to rebut his versions of partisan objections to deficit reduction, but partisanship is not the primary obstacle he faces. Rather, Oppenheimer will find it difficult to convince even dispassionate readers that his views are to be taken seriously. Those who are interested in an in-

formed critical examination of U.S. budget policy must look elsewhere.

Southern Methodist University DENNIS S. IPPOLITO

The Humane Economy: Populism, Capitalism, and Democracy. By Norman Pollack. New Brunswick: Rutgers University Press, 1990. 215p. $40.00.

Who were the Populists? In Richard Hofstadter's pungent account, they were reactionary, intolerant, xenophobic, occasionally comical reformers searching for a lost agrarian age. Norman Pollack has long offered the opposite perspective that they were genuine grassroots democrats who offered America one last alternative to corporate domination. In *The Humane Economy* Pollack continues the argument.

Pollack explores Populist economics, then tries to look "through" the Populists for a glimpse at "the meaning of America" (p. ix). Between the lines, he defends the coherence and modernity of the reformers' vision.

In Pollack's view, the Populists were small rural capitalists. They celebrated property on the level of the individual home and community. (In his hands, these Midwestern yeomen look like direct descendants of the eighteenth-century civic republicans, though Pollack avoids making the connection.) The Populists spent their political passion attacking monopoly capital. They blamed the monopolists for, among other things, robbing the people of their livelihood, stealing their homes, forcing them into poverty, capturing their government, destroying their democracy and reducing the country to "a nation of hired men" (p. 16).

The Populist alternative would have grafted industry onto a democratic political base. They affirmed market competition; but they sought an independent state (independent of the monopolists) that would restrain capitalist activity, break up concentrations of power, and protect societal needs from economic activity. Wherever competition could not be restored, Populists would replace monopoly with state ownership. This, insists Pollack, is no machine-smashing outcry against modernity. On the contrary, Populists would embrace industrialization and labor-saving devices; but they would subordinate them to the general good, they would harness industrialism to create a "humane economy."

What happened? The usual answer is simple—rural reformers could not postpone modernity. Pollack scornfully attacks social scientists for accepting this deterministic view. Even words like *determinism* and *monopolism* have been replaced by the antiseptic *order* (or, he might have added, the relentlessly deterministic *rationalization*). History dismisses these losers and fears their commitment to democracy, says Pollack. As a result, we have forgotten that at the end of the nineteenth century, Americans were poised between corporate capitalism and the more humane Populist economy.

If so, the critical moment was a brief one. The Populists turned to politics (with the Omaha platform) in 1892; the movement collapsed four years later. Their swift demise gets to the heart of Pollack's contention. Once they entered the political fray, the Populists became steadily more radical till their reforms "threatened an irreversible conversion to radicalism" (p. 89). But these Midwestern democrats would not cross the line, would not allow their sectional and agrarian movement to become a national and class-based one. They would not violate the right of property. The limits of American populism lay, not in corporate opponents or irresistible economic forces but in the Lockean, liberal ideology that Pollack finds in the Populists' own heads.

Pollack's picture of the Populists is deft and nuanced. By insisting that we take their vision seriously by imagining what it would have looked like in practice, he creates a rich, compelling story. And a close look at the Populists does offer us glimpses of the genes that seem to get passed down the generations of American reform in the form of the search for an autonomous state free of special interests, a yearning for lost community (to be recaptured down in the grass roots), and (suicidally) a truncated political spectrum that limits the reforming imagination.

Still, political scientists are more likely to be tantalized than persuaded by the argument that the Populists had internalized an American political culture that checked their imagination and doomed their crusade. The trouble is that *The Humane Economy* does not explore competing explanations for the Populists' failure. It has little to say about the biases of the political economy or the reaction of the East Coast capitalists (or, for that matter, the Populist counterreaction). We get a deep, cultural, explanation before the more obvious alternatives are even considered. The book lacks the dialectic that Pollack tells us is missing from American history.

Finally, this may not be a book for readers meeting the Populists for the first time. The political history is sketched in a brief and perfunctory way. And the writing is tangled. But Pollack's passion for this movement gets him, and us, inside the Populist vision. The result is a fine, ultimately mournful account of one of the most important defeats in American political development.

Brown University JAMES A. MORONE

The Fourth Estate and the Constitution: Freedom of the Press in America. By Lucas A. Powe, Jr. Berkeley: University of California Press, 1991. 357p. $29.95.

This book, written by a law professor, is meant for other law professors. It contains little that is political and even less that is scientific. The author, without much explanation or justification, organizes his book around *New York Times v. Sullivan* (1964), the landmark decision that constitutionalized the law of libel. His rationale seems to be that "no other period manifested a tradition of protecting dissent better than this short, but remarkable, era" of the seven years from 1964 to 1971 (pp. 20–21). Fortunately, Powe does not limit his treatment to only these years; he surveys, instead, the whole of American history.

The problem, rather, is a degree of topical selectivity that leaves huge gaps in coverage. In addition to libel, he provides the reader with chapter-length treatments of prior restraint, access to sources and information, and media monopoly. These may well be, as Powe alleges, the most contentious contemporary legal press issues. Assuredly, they are not the only ones. Exacerbating matters is a focus restricted to the Constitution, ignoring statutory developments. The result is an intellectually unsatisfying treatment of press freedom. Dissatisfaction obtains, in part because coverage is unsystematic and in

part because the author sharply distinguishes freedom of the press from freedom of speech specifically and from freedom of communication generally.

For example, although the author notes in passing the importance of subsequent punishment (p. 171), clear-and-present-danger receives but two pages of discussion, with an additional two given to *Brandenburg v. Ohio* (1969), the authoritative statement of the clear-and-present-danger doctrine (pp. 71–72, 95–96). Although *Brandenburg* was decided during the author's key seven-year period, it concerned verbal communication, not the printed word.

Even the first third of the book, which provides the background of press freedom, is haphazard. Powe begins with a telling description of what may well be the most outrageous instance of suppression of political dissent in American history, the saga of a U.S. senator and Colorado publisher whose punishment for a wholly truthful statement was upheld by the U.S. Supreme Court in *Patterson v. Colorado* (1907) on the grounds that the First Amendment simply does not prohibit the subsequent punishment of communication. The remainder of the introductory material bounds across well-traveled terrain: Peter Zenger's case, the Revolutionary period, the Alien and Sedition Acts, and World War I.

In the final third of the book, Powe presents two "models" that "promise some descriptive and considerable prescriptive accuracy": the public's right to know and the "fourth estate," meaning press autonomy (p. 233). Needless to say, neither is a model in any political or scientific sense.

Powe's book is not totally bereft of a political orientation. His treatment of the politics of *Miami Herald v. Tornillo* (1979), in which the Court validated press autonomy, is exceptionally well done. Because Powe is a legal realist, the tendentiousness that characterizes so many doctrinal works is pleasantly absent. But true to the disjointed treatment he otherwise accords his subject, he does not allow his realism to override his partiality for the press. Thus, his flippant criticism of the secrecy of the Court's conference (p. 196) better serves an editorial page than a scholarly work.

Michigan State University HAROLD J. SPAETH

Critical Pluralism, Democratic Performance, and Community Power. By Paul Schumaker. Lawrence: University Press of Kansas, 1991. 258p. $29.95.

Critical Pluralism, Democratic Performance, and Community Power is an in-depth study of power, influence, and policy making in Lawrence, Kansas, and the degree to which the policymaking process of the community is consistent with the normative ideals of pluralist democracy. Specifically, these ideals, nicely elaborated by Schumaker, include responsible representation, complex equality, and principle–policy congruence.

At the outset, Schumaker provides the reader with an excessively brief theoretical overview of the elitist, pluralist, economic, and regime interpretations of community power, devoid of much specific reference to earlier inquiries. This material is followed by a socioeconomic profile of Lawrence and an overview of its council–manager (mislabeled *commission–manager*) form of government, with a five-member council elected at-large on a nonpartisan basis.

In conducting his investigation, Schumaker employed a well-conceived, complementary, two-fold methodology. First, to secure an understanding of the underlying political culture of Lawrence, citizen support for conservatism and liberalism, and nine pairs of competing political principles (e.g., neighborhood rights versus property rights), Schumaker resorted to public opinion data garnered from interviews with 239 active political participants and a sample of 1,017 citizens. Not surprisingly, the citizens of Lawrence are about equally divided in terms of conservatism and liberalism; and tenets associated with the council–manager plan (e.g., the availability of good public services and "businesslike," efficient, and economical government) enjoy widespread support in the community.

Second, Schumaker, utilizing the comparative issues approach, discerned the participants involved in the resolution of 29 policy issues that confronted Lawrence in the time frame 1977–87. These issues, each of which the author describes in a nicely crafted mini–case study, involved a wide array of matters pertaining to the democratic process, economic growth, social control, the provision of services, and tax distribution.

Schumaker's central finding is clear and to the point: "The ideals of representative democracy are reasonably well attained in Lawrence; the community has been able to resolve issues without the maladies of external, elite, bureaucratic, or minority domination" (p. 173)—to wit, not even a ghost of a power elite exists in Lawrence. Instead, a sort of sloppy political pluralism prevails in the community, where policy issues are often decided in a rough, consensual, pragmatic sort of way, rather than on the basis of political principles.

Although Schumaker makes an impressive case that pluralist democracy is alive and well in Lawrence, he readily admits that some segments of the community benefit more than others from the local political system: "Issues are usually resolved in ways that do favor relatively advantaged participants—especially the middle class, Country Clubbers, Seniors and Veterans, Hometowners, conservative Republicans, Market Providers, Managerialists, members of the Growth Machine" (p. 201).

Critical Pluralism, Democratic Performance, and Community Power is a solid study that makes a significant contribution to our understanding of community political power and democratic political theory. It should be read by all serious students of community power. However, given Schumaker's penchant for political theorizing (especially his defense of pluralism), his volume could have been strengthened by incorporating the findings of earlier relevant contributions. Concluding analytical commentary does not need to be limited to a discourse based on the empirical findings of a single community.

Virginia Commonwealth University NELSON WIKSTROM

The NSC Staff: Counseling the Council. By Christopher C. Shoemaker. Boulder: Westview, 1991. 152p. $29.95.

Christopher C. Shoemaker, a former military officer who served on the National Security Council (NSC) staff under both Carter and Reagan, has written a useful overview of the history and functions of the NSC staff. This book begins with a historical review of the growing

importance of the NSC throughout the cold war period. The author then turns to an examination of the various functions that the NSC has been asked to serve in recent administrations. He emphasizes, in particular, several relatively noncontroversial functions, such as administration of national security policy, interagency coordination, and advising the president on issues of foreign policy. He contrasts these with several more controversial functions, including crisis management, policy formulation, and (finally) policy advocacy.

Shoemaker argues convincingly that the strengths of the NSC derive from its standing bureaucratically independent of the preferences of any single agency and from its presidential perspective—the fact that it reflects in its composition and functioning the priorities and policy perspectives of the president, rather than the more parochial interests of any single bureaucratic agency. He then argues that this function reached its height during the Carter administration when Zbigniew Brzezinski was national security advisor, a role that largely deteriorated during the Reagan administration, when the NSC was downgraded and the advisor's position went through numerous hands.

Shoemaker suggests that this decline in central coordination of national security policy in the Reagan years was largely responsible for fragmented foreign policy decision making, of which the Iran–Contra fiasco was the most visible example. By contrast, he has a much more positive view of the functioning of national security policy in the last two years of the Carter administration; and he generally downplays the problems encountered in those years in the handling of the Iran hostage crisis, the SALT II treaty, and the Soviet invasion of Afghanistan. Indeed, most of the major foreign policy accomplishments of the Carter administration seemed to come in its early years before, as he puts it, "Brzezinski and the NSC Staff finally wrested control of the national security system" (p. 67). Thus, his prevailing view that the prominence of the NSC in the policymaking process seems to promote success of national security policy hardly seems to be born out by the actual experience of the Carter years.

The book concludes with an analysis of the changing national security environment (especially the shift from a military emphasis to a focus on issues such as economics, terrorism, the war on drugs, and ecological issues) and the implications of this changing environment for the structure of the NSC. He also notes the growing role of Congress and public opinion in the national security decision-making process. While these observations are generally appropriate, the suggestions offered are largely obvious and not particularly innovative. His major prescriptive recommendations include strengthening the role of the national security advisor, restoring the influence lost during the Reagan years, and adding staff positions to the NSC to deal with such issues as economics, low-intensity conflict, transnational issues (e.g., environment and population), mobilization and industry, and congressional relations. These sort of structural recommendations, however, fall far short of suggesting the kinds of conceptual changes that are required in dealing with national security in the post–cold war era, in which bipolarity has come to an end and American hegemony in the global system is also coming under increasing global challenge. These changes in how American policymakers conceive of national security are likely to be far more important than any simple bureaucratic reshuffling of the NSC staff in grappling with foreign policy making in the final years of the twentieth century.

In short, this book offers useful reading for all students of the U.S. national security decision-making process. To policymakers and public officials its offers some useful but not particularly innovative suggestions about how the NSC staff might be restructured. It acknowledges some of the important changes now underway in the international system, though largely failing to come to grips with the full range of rethinking that will be required to enable U.S. foreign and security policy to be more responsive to the rapidly changing demands of the new global environment.

Brown University P. TERRENCE HOPMANN

Congress Oversees the United States Intelligence Community, 1947–1989. By Frank J. Smist, Jr. Knoxville: University of Tennessee Press, 1990. 336p. $24.95 paper.

Frank J. Smist, Jr.'s interests in Congress and intelligence were stirred by his mentors James Lengle and Ray Cline. This his first book is a careful, scholarly history of congressional oversight of the intelligence community. Separate chapters are devoted to the Church committee, the Inouye, Bayh, and Goldwater Senate select committees, the Nedzi and Pike House committees, and the Boland committee in the House. A final chapter looks at the Senate Intelligence Committee under Republican senator David Durenberger of Minnesota; the House Intelligence Committee under Democratic representative Lee Hamilton of Indiana; the select committees on the Iran–Contra affair; and the Boren, Stokes, and Beilenson committees.

Smist's final chapter also contains his conclusions. Citing with approval the statement of a Senate Committee staffer that during the Iran–Contra affair, the Reagan administration had sought to escape the Constitution, Smist adds, "Such contempt for the American constitutional form of government was unprecedented."

"In the Iran–Contra Affair," Smist writes,

> President Reagan and his administration tried to do in secret what in a democracy they could not do in public. In showing contempt for the democratic process, President Reagan implemented policies that embarrassed the United States, damaged the national security, and undermined the American constitutional system. History must render a much harsher verdict on Mr. Reagan's job performance as president than the American people did in January, 1989.

In a summing up, Smist asserts that Congress must be "a full and equal partner of the president on intelligence." At the same time, he acknowledges that the executive branch unfairly refuses to trust the Congress to keep its secrets. Looking at the various proposals for improving Congress's carrying out of its intelligence oversight function, Smist comes down in favor of the present system of separate House and Senate committees. His other recommendations are unexceptionable: party leaders should appoint good people to the committees and find ways to reward them for a task that will not win them votes, the very best staff should be recruited, and the committees should demand a more meaningful role in decisions concerning covert action.

Only one of his recommendations should be challenged. Smist says that both the Senate and the House

should reduce the number of their committees to bring "more order and efficiency to both chambers." One is reminded of the lifelong and totally fruitless labors of George Galloway. Galloway was also a political scientist and the principal staff member behind the herculean reorganization of Congress following World War II. The purpose of that reorganization was precisely to reduce the number of committees. But the need of members for a political platform, for extra staff, and for the other perks of chairpersons was overwhelming; and the proliferation of subcommittees began.

This also illustrates a point that should be made about the whole book. What is almost never discussed is the *politics* behind the moves and countermoves of the various members, committee chairs, and other actors in the story of congressional oversight of intelligence. Except for a short and very disapproving section on how Democratic senator Patrick Leahy of Vermont used his membership on the committee to get a lot of favorable media coverage during a tough reelection campaign, politics is hardly mentioned.

Columbia University ROGER HILSMAN

Learning Together: The History of Coeducation in American Public Schools. By David Tyack and Elisabeth Hansot. New Haven: Yale University Press and Russell Sage Foundation, 1990. 369p. $28.50.

The policy and practice of coeducation in public schools is a central issue in the continuing American experiment in democracy. The authors explore this issue in historical perspective, examining the evolving institution of public schools over the two-hundred-year history of the nation. In the process, they raise basic questions regarding the role of the schools in preparing boys and girls for assuming the roles of responsible citizenship in a democratic society.

The organizing concept for the book is an innovative exploration of the treatment of gender as a design for institutional form and practice in public education. The authors carefully trace the decision processes that led to the education of girls in colonial America and the nascent republic and document the economic and social bases of decisions by local school boards resulting in the acceptance of coeducation as the dominant educational policy in community schools by the mid nineteenth century. Coeducation, in turn, prepared educated women who were effective teachers and accepted lower salaries than men. To economy-minded local school boards, this practical alternative served as the basis for the transition of public school teaching from a predominantly male, to a predominantly female, profession in the early twentieth century. To modern feminists, the practice appears as an early basis for differential treatment by gender in employment conditions. To educated women in the decades of the late nineteenth and early twentieth centuries, however, teaching offered a welcome and respected alternative to economic dependency. In a balanced and perceptive study, the authors trace the dynamics of the policy over two centuries, from its inception in colonial New England through its acceptance as the dominant educational practice and the backlash of conservative groups to the concerns of modern feminists today.

In method of analysis, the book combines historical inquiry with public policy concepts and sources. The authors use historical photographs in a novel way to document gender practices in public schools over time. Taken for other purposes, the photographs provide unobtrusive evidence of gender as an organizing concept in institutional practice. The photographs document the conscious evolution of coeducation in classroom activities, and, interestingly, reflect the self-selection of same-sex groupings by school children in unstructured play. The contrast indicates the importance of coeducation as an institutional form that differed from the dominant social attitudes toward gender in the early nineteenth century and the significant shift in those attitudes over time. The use of photographs was even more compelling as evidence of the interaction of gender with race in public school practice. Although treated as a minor theme in the book, the historical reinforcement, in public education practices, of a strong role for black women but undervaluation of black men identifies gender differences in economic opportunities and social acceptance for blacks in the evolution from slavery to civil rights as a source of contemporary difficulties for black males in a society that still struggles with discriminatory practices.

The basic findings of the study are not new; but especially valuable is the closely reasoned interpretation given them in understanding the importance of gender policy in public education as a fundamental socializing experience for a democratic society. The authors establish five basic points in their argument. First, women are considered coequal partners with men in the New World enterprise of building a democratic republic. This position, articulated by Benjamin Rush and other founders of the republic, represents a conscious policy decision that distinguishes the new nation from its Old World heritage. Second, men and women had different but complementary roles in the New World; and they joined together in the larger enterprise of achieving the ideals of freedom, dignity, moral integrity, and continuing growth in the unfinished task of building the republic. Third, economic advantages reinforced the remarkably rapid transition to the dominant practice of hiring women as public school teachers, which simultaneously created a legitimate and public role for women in their communities. Fourth, early notions of gender differences in scholastic abilities are disproven and documented solidly over time. Finally, the public schools are established firmly as institutions that provide a primary socializing experience for the continuing evolution of a democratic society. This role offers both the opportunity and the obligation to create learning environments in which gender differences are acknowledged—but not turned into the basis for differences in legal or economic treatment. In light of the evidence presented by the authors, the same argument applies to racial differences.

In its research, documentation, and interpretation of findings, the book exemplifies a balanced treatment of gender issues in the evolution of public education in the U.S. Tyack and Hansot have produced an informed, carefully documented, and closely reasoned interpretation of the public schools as a formative social organization that shaped values and practices in U.S. society toward gender and race, melding visionary goals, economic interests, and social expectations in the bold effort to create a democratic society. The authors offer fresh insight into the evolving issues of gender and race that are of fundamental importance in the development of a

democratic society and acknowledge the limitations in educational concept and practice that have subjected these issues to a full range of ideological distortions over the history of the republic. The book is remarkable for its balance, intelligence, wit, and reason in a probing inquiry into the history of coeducation as a continuing experiment in democratic practice.

University of Pittsburgh LOUISE K. COMFORT

The Rise of Candidate-centered Politics: Presidential Elections of the 1980s. By Martin P. Wattenberg. Cambridge: Harvard University Press, 1991. 186p. $24.95.

The American electoral system does not work the way it did within recent living memory. There are many who think that its performance, whether in democratic or governing terms, has significantly deteriorated. The changes have been well and truly rung in the professional literature on such themes as dealignment of the electorate, the emergence of candidate-dominated campaigns, the fracturing of electoral coalitions along office-specific lines, the emergence of divided government as a normal state of affairs for the first time in national history, and so on.

Thus, this little book by Martin P. Wattenberg may not appear at first glance to do more than restate emerging conventional wisdom along these lines of professional analysis. But *The Rise of Candidate-centered Politics* is not merely a new edition of the author's well-known *Decline of American Political Parties*. It attempts to construct a more general view of the current terrain and gives us some novel insights about it.

The overall story of dealignment is pretty extensively documented in this story. Some aspects of it are quite familiar, others not. Among the less obvious trends is a huge increase in the relative salience of economic factors in the open-ended candidate evaluations that voters make (p. 27). There was a clear jump-shift upward in the relative salience of economic factors between 1968 and 1972. Today, they enjoy a position of clear dominance over sociological and especially social–psychological factors in these evaluations. As the author points out, it makes sense for voters increasingly detached from traditional partisan identification and salience anchors to concentrate especially on the economic performance or promise of individual candidates.

It is also noteworthy that the popularity of presidential candidates in general has shown an aggregate tendency to decline over the past generation as candidate-centeredness in elections has emerged and become consolidated. Wattenberg gives us one more range of data that make it clear that in fact Ronald Reagan and his policies were anything but universally popular. Indeed, a review of both candidates in 1980 reveals that Carter actually came out ahead of Reagan, overall, along five dimensions of candidate popularity (p. 83). It was, in the end, economics that did Jimmy Carter in. For example, 1980 was the first presidential election year since 1932 in which there was negative growth in per capita disposable income.

By the same token, a prime key to Ronald Reagan's success in 1984 and George Bush's election in 1988 was a generally satisfactory macroeconomic performance—particularly notable in that it followed years of disorder and stress on both economic and other dimensions of our collective lives. As Wattenberg points out and many others have observed, voters in the aggregate are ends-oriented. They are largely indifferent to the means employed to reach a goal they desire (in this case, prosperity). We recall that the Democratic party suffered for decades from its association with the depression of 1893–97, just as the Republicans were to suffer, in their turn, for the "Hoover depression" that began in 1929. Now the wheel has turned again. A very clear implication of this study is that Republicans can continue to look forward to bright presidential election prospects until or unless major economic failure should once again coincide with their incumbency. In contrast with the past, of course, dealignment involves not Republican ascendancy across all of the policy institutions of government but, rather, divided government as a normal condition.

In one of the more interesting segments of this work, Wattenberg notes that there have been three major schools of voting analysis in modern times: the sociological, the social–psychological, and rational choice (associated with Berelson, the University of Michigan, and Downs, respectively). Analyzing the social sciences citation index over time, he notes that there has been a major change in citation dominance since the 1960s from the Michigan school to Downs, the latter enjoying impressive ascendancy by the 1980s (p. 19). While such evidence may be considered circumstantial at best in some quarters, the author's evaluation seems quite plausible. He argues that the transition away from party identification hegemony toward a candidate-centered electoral universe involves a more or less radical change in the field of observation. In this new empirical world, important professional problematics may be better or more credibly addressed in rational choice terms than through the social–psychological perspectives that once dominated voting analysis.

The Rise of Candidate-centered Politics impresses me as a credible and worthwhile study, but it is by no means beyond criticism. For one thing, I would have welcomed a somewhat broader and more extended discussion of macrosystemic consequences and implications as an appropriate capstone for the empirical presentations. For another, the author may be a bit too eager to argue that there has been no substantial change in the distribution of conservative, centrist, and liberal self-indentifications in the electorate over the past 20 years. This, of course, downplays the substantiality of Reagan's so-called mandate for policy revolution in the wake of the 1980 election. But even the data given in this work make it clear that the Right has gained quite substantially, while the center has shrunk and liberals hang on to their one-quarter share. Earlier NES data with a somewhat different measure strongly suggest that the 1960s distribution was a bell curve with the center dominant and the self-identified liberals somewhat outnumbering conservatives at each end. It would seem that the liberal erosion came first and was largely completed by 1972. Thereafter, it was the center that could not hold, so that by 1988 there is an unambiguous right-skewed curve replacing the old bell distribution. Party identification, a "lagging indicator," has been following suit since 1980, so that by 1991 we find a Republican plurality for the first time since modern surveys began asking identification questions in the 1930s. Despite the possible messiness of this indicator (For how many voters do such liberal–conservative categories really make sense?), there appears to be much more going on along such dimensions than the author says.

Finally, there is the question of placing this electoral and regime order in proper time boundaries. The rise of candidate-centered politics is not a phenomenon of the 1980s, although its consolidation certainly is. For all the vast importance of the Reaganite–Republican presidential transformation during the past decade, the system described in these pages came into being during and out of the vast political crisis of the late 1960s (which, interestingly, was not very visibly economics-driven). If one searches for major punctuational-change breakpoints in the author's long-term dealignment series, they can be found centering around 1966 for both split-ticket indicators and 1970 for the party identification and party attitude indicators. Other measures give similar breakpoint dates in system state. All of this constitutes evidence for a genuine critical realignment centering around 1968–72. Out of that crisis and realignment came the era of the "permanent campaign," the television-wired electorate, the dissolving of the class-centered glue of the Democratic presidential coalition, and candidate-centered politics—in short, the "sixth electoral era." Such scanning for periodization is not an exercise in quibbling but the first step toward placing the phenomena of the Reagan era in a more adequate historical (and thus analytical) context.

Nevertheless, much of the foregoing discussion implies a much longer work than the author chose to give us. Within its own terms and dimensions, *The Rise of Candidate-centered Politics* provides information and analysis that makes it a significant contribution to the professional literature on contemporary American electoral politics.

The University of Texas, Austin WALTER DEAN BURNHAM

Federal Preemption: The Silent Revolution. By Joseph F. Zimmerman. Ames: Iowa State University Press, 1991. 228p. $34.95.

The attention of political scientists to intergovernmental relations has risen and fallen with federal spending for grants. When outlays were high, the study of federalism was a booming business. When federal grant spending first stabilized and then dropped, many researchers lost interest. In this useful book, Joseph F. Zimmerman demonstrates that the flow of money has masked a far more important change in federal–state–local relations, with subtle shifts in power that have swamped programmatic effects in importance. A "silent revolution"—the rise of complex federal intrusions into what had previously been state and local domains—poses new problems of accountability and responsiveness.

At the center of this revolution is federal preemption, in which the federal government orders or induces the state and local governments to do what it wants done or in which the federal government itself moves onto turf previously dominated by subnational governments. Zimmerman identifies three kinds of preemptions: informal, total, and partial. *Informal* preemptions are the most widely recognized. The federal government awards grants conditional on prescribed state and local actions. *Total* preemptions supplant state and local with federal authority in a policy area. From the treatment of radioactive waste to the size and weight of trucks on interstate highways, federal rules have replaced state and local standards. Finally, *partial* preemptions permit "state regulation at the sufferance of Congress, which at any time may preempt totally the responsibility for a regulatory function" (p. 92). In the Water Quality Act of 1965, for example, Congress authorized the secretary of the interior (and later the administrator of the Environmental Protection Agency) to issue water quality standards that become effective within six months if states do not enact standards at least as tough.

Zimmerman's work is not the first on federal preemption. The U.S. Advisory Commission on Intergovernmental Relations, for example, published an important study called *Regulatory Federalism* (1984) that explored some of the basic issues. Zimmerman's book, however, is by far the most comprehensive on this critical question.

What most concerns Zimmerman is the erosion of the boundaries between the states and the federal government, an erosion promoted by Congress and abetted by the federal courts. At the core of Zimmerman's view of federalism is the concept of *imperium in imperio* (an empire within an empire), in which the states exercise certain powers relatively autonomously. With the rise of federal preemptions, especially since 1965, management has become more tangled, and accountability more elusive: "Federal preemption, total and partial, reduces citizen control of government as the decision-making forum is shifted from local governments and the states to Congress" (p. 13).

Accompanying Zimmerman's analysis is an impressive catalog of the forces underlying this trend. His chapter on informal preemptions, for example, contains thumbnail descriptions of 19 separate criticisms that have been raised against categorical grant-in-aid programs. Partial preemptions, he writes, have grown because of seven principal factors, including the failure of interstate cooperation and the failure of federal grants to provide enough inducements to shape state and local action. The formal preemptions that have arisen over the years number fourteen types. These catalogs and frameworks represent the book's strongest features. They are careful and exhaustive, laying out the groundwork and history of the issues.

At the same time, however, the book remains skeletal. Zimmerman suggests that only a dynamic model of federalism, based on mutual interdependence among the levels of the intergovernmental system, can account for the remarkable changes of the last generation; but he does not construct such a model. He conceives of the book as an exploratory study that he hopes will generate case studies to refine the broad outline he develops (pp. vii–viii). The reader might have wished Zimmerman's prodigious research and useful insights to lead past an initial exploration into both more case studies and a deeper examination of the issues. Furthermore, some important policy areas (especially the growing importance of Medicaid mandates) are not explored.

Nevertheless, long after general revenue sharing has become but a footnote in the federalism literature, mandates are likely to be an important issue. Zimmerman performs an invaluable service in drawing his readers' attention past the hot political battles surrounding federal grant spending to the quiet critical issues of federal mandates.

University of Wisconsin, Madison DONALD F. KETTL

COMPARATIVE POLITICS

Sadat and After: Struggles for Egypt's Political Soul. By Raymond William Baker. Cambridge: Harvard University Press, 1990. 365p. $35.00.

The Communist Movement in Egypt. By Tareq Y. Ismael and Rifa'at el-Sas'id. Syracuse: Syracuse University Press, 1990. 218p. $37.95.

Sadat and After attempts to answer the author's Egyptian students' question what kind of political community, "however fragmentary," they should try to fashion for themselves. Neither the official ideology nor development theory nor Marxist dependency theory were of assistance, because they reenforced these students' personal sense of powerlessness. According to the author, "political lives lived on the margins" are the resources they are utilizing to build a better tomorrow. The volume is about the margins of Egyptian politics, where alternative points of view offer competing views of Egypt's future. This pluralist alternative to the Egyptian state and to dependency theory (and, presumably, development theory) is said to be that of group theory accompanied by references to discourse and narrative self-descriptions and the potential power of all groups (p. 293). Instead, the reader is informed about such marginal groups and persons as the Arab Contractors (a construction firm) and Osman Ahmed Osman, who survived as an entrepreneur under Nasser, flourished in the economic liberalism of Sadat, and flourished (but less) under Mubarak. The Bar Association is described accurately as a redoubtable resistor of political authority under all regimes. Nasserism is presented as an enduring ideology, and Marxists are evaluated. The rugmaking and other artistic activities of the Wissa Wasef School are referred to as "creative empowerment," while the think-tanklike al-Ahram Center for Political and Strategic Studies is also described. Finally, the relative forthrightness of the important journal *Economic Ahram*, like the organization of the Muslim Brothers, is dealt with as a centrist politically important religious group.

The case studies are, in the final analysis, interesting and even empirically important. They provide glimpses of a more vital and diverse Egypt than do the stereotypical images of authoritarianism and the ritual of parliamentary democracy. However, although they are valuable vignettes, they are not empirically or theoretically very meaningful, in spite of the author's extensive efforts to make them so. (Two concluding chapters are devoted to this purpose.) First, not all the case studies are groups. For example, Osman Ahmed Osman is an individual entrepreneur, the Nasserists are a continuing important sentiment, and the Marxists represent an intellectual school of thought. Second, although the author speaks in his introduction of these groups in terms of their potential for power, in fact, among the eight, only the Bar Association and the Muslim Brothers have political importance, the others have either failed until now like the Marxists or are nonpolitical, like the Wassef School. Third, the author's determination to address groups on the margins of Egyptian politics and to theorize about them in such a restrictive way, keeps him from demonstrating theoretical and practical significance of his studies. This aside, however, the author has succeeded in informing us about the diversity of political voices in Egypt.

The Communist Movement in Egypt, 1920–1958 is essentially a chronological history of the myriad splinter organizations and personalities that figure in Egyptian communism. It is written by a well-known specialist on Middle East politics and a prominent Egyptian member of the party. Rifa'at el-Sas'id is presently the secretary general of the National Progressivist Union party, the major leftist opposition political party. The focus of the book is upon national organization, ideology, and personalities. It argues that whereas the first phase of the party's existence (1920–34) was one of external international initiatives, the second phase (1930–50) represented its Egyptianization. However, the authors fail to develop any interpretive themes as to the social background of its leaders or followers as the movement Egyptianized. Thus, the reader is presented with a succession of disjointed organizations that often appear to be parties of personalities, rather than institutionalized parties or parties with significant ideological differences. However, the movement was severely repressed both by Nasser and Sadat—in fact, el-Sas'id spent 14 years in prison.

The conclusion to the study is important because of its critical nature. It notes that communism was a Western doctrine adopted by Egyptians and that they have not been able to adapt it to the predominantly peasant-based economy. The author's comments that Marxism has become the intellectual conscience of the nation is also true. Some of Egypt's most respected intellectuals write regularly and critically about the problems of contemporary Egypt.

The book is useful as a chronicle of the movement but needs to be supplemented by Selma Botman's *Rise of Egyptian Communism* (1988) and Joel Beinin and Zachary Lockman's *Workers on the Nile* (1987). It contains a sizable appendix with translations of documents related to the party from 1915 to 1983.

University of Maryland, Baltimore County LOUIS J. CANTORI

The Politics of Social Solidarity: Class Bases of the European Welfare State, 1875–1975. By Peter Baldwin. New York: Cambridge University Press, 1990. 353p. $44.50.

The Welfare State in Capitalist Society: Policies of Retrenchment and Maintenance in Europe, North America, and Australia. By Ramesh Mishra. Buffalo: University of Toronto Press, 1990. 152p. $60.00 cloth, $19.95 paper.

Mishra and Baldwin offer contrasting thematic approaches to the study and analysis of the welfare state. Mishra's approach is to consider the degree of fragility of the welfare state under neoconservative attack. He observes that the neoconservative political movements in the United Kingdom and the United States led not to the

dismantling of the welfare state but to the development of more class-stratified societies, in which the solidarity commitments of the welfare state to the poor and the needy were gradually eroded. On the other hand, he notes that commitments to maintaining the welfare state have generally been sustained in Sweden and Austria in spite of curtailment in the expansion of existing social programs. He discusses both the Canadian Progressive Conservative party's brief flirtation with, and retreat from, neoconservatism and the Australian Labour party's failure to move toward universalistic income maintenance programs as reflecting the unique political patterns of those two countries. Baldwin presents a historical critique of the theme of industrial worker "social solidarity" as a key explanatory variable in the development of European social welfare policies. He documents a variety of specific group interests, other than the interests of industrial labor, which have advanced social benefit programs and contributed to the development of the welfare state in diverse national settings.

While Mishra observes that neoconservatism, with its postulates of privatization and the maximization of free market activity, has not dismantled the welfare state and has been only partially successful in achieving its goals, what he does not fully explore is the fact that neoconservative successes in the United States have resulted in the erosion of the well-being of many of the poor. Nevertheless, the welfare state in the 1980s in Britain and the United States, as Mishra notes, "has not been dismantled by neo-conservatives. Much of the building stands intact though cracks are beginning to show, upkeep and maintenance have been minimal and the furnishings are beginning to look shabby" (p. xi). Here, he is on the mark as against analysts, Peter Ruggles and Michael O'Higgens, who minimized the ability of the New Right to advance its agendas in the United Kingdom and the United States (in *Stagnation and Renewal in Social Policy*, ed. Rein, Esping-Anderson, and Rainwater, 1987). The failure to develop a full employment policy in the United Kingdom plus the budgetary squeezing of the National Health Service have contributed to the development of a new, increasingly class-stratified society in the United Kingdom. Similar trends—albeit in a different context—can be observed in the United States.

Mishra notes that these social policies are tied to taxation policies that seek to minimize the impact of taxation on the wealthy and shift the impact of taxation to the middle and lower classes. What Mishra fails to notice is the success of New Right policies in the United States in paralyzing the development of needed social services in a period of rapid social change. While never fully examining the issue, Mishra does note in passing that, indeed, the Thatcher and Reagan policies created "a whole host of changes . . . significan[t] from the standpoint of social inequality and the social well-being of individuals" (p. 43).

Mishra also discusses balancing the goal of maintaining the social welfare state in Austria and Sweden with the goal of being economically productive and competitive internationally. For instance, he discusses the role of the tripartite Swedish Labour Market Board (government representatives and representatives of employers' and workers' organizations) in seeking to maintain full employment, universal social services, and basic minimum standards. Its emphasis on training, retraining, and relocation programs for workers has managed to deal with such dysfunctional tendencies as overmanning, labor rigidity, and low productivity.

Mishra observes that constraints exist on the neoconservative agenda of dismantling the welfare state, as well as on social democratic goals of advancing the welfare state. However, he noted that the neoconservatives in the United States and the United Kingdom have been successful in redistributing power and privilege upward, while social democracy (particularly in Sweden) has been able to defend social program commitments and full employment.

Peter Baldwin's book is an excellent comparative historical account that sustains a thesis that class interests in the development of social insurance programs are broader than the nature of working-class interests. Within this perspective, he undertakes to debunk the theory that labor solidarity is necessarily the key element to solidaristic social insurance policies. Among his examples are the role of the agricultural bourgeoisie in support of late nineteenth-century Swedish old-age social pensions and workmen's compensation insurance. Similarly, Denmark's noncontributory, tax-financed pensions appealed to Denmark's rural workforce of cottagers and smallholders.

In the post–World War II period, solidaristic trends to create more tax-financed social insurance–connected pension, survivorship, and health insurance funds resulted from a number of class interests. This included the importance of Liberal support (with a constituency of prosperous farmers) for universal pensions in 1953 in Denmark and of universal earnings-related benefits by the Danish Conservative party in 1964. Also in France, the groups that had too narrow an actuarial basis for social pensions and health insurance influenced the creation of insurance funds (regimes) organized by region, rather than by occupation, that included artisans and commercial and industrial firms. Similarly, in 1957, the Christian Democratic Union of the Federal Republic of Germany pushed the passage of a law that invited the self-employed (including lawyers) to join the general old-age pension fund retroactively and make retroactive payments. On the other hand, wageearner's representatives opposed flat-rate, universalist publicly financed social insurance measures, due to concerns of self-interest, not of solidarity.

Baldwin's key point is that narrow class interests, which would include working-class interests, often shape the context of social insurance legislation. Such interests are shaped by "sunken costs," that is, by previous legislation. Interest group pressure in such areas may vary dependently in relation to various economic and historical contexts. Claims for redistributive justice may be important, but the classes at the bottom vary from workers in Great Britain and Germany in the late nineteenth century to peasants, smallholders, and agricultural laborers in Sweden, Norway, and Denmark in the same period. Insightfully, the author observes that interests for and against solidaristic social policy reform are played out in terms of a triangular relationship between social insurance policy, society, and class. For example, the desire of the West German and French self-employed to eliminate the burden of being a narrow actuarial risk group and enter the wage-workers fund in the 1960s was initiated by both economic decline and the

presence of socially separated insurance funds developed in the post–World War II period that had brought together for risk sharing all in the same occupational category. These events led those in "shallow risk pools" to support overall solidaristic policies—often over the opposition of the wageearners group.

Because these books are so different methodologically and in terms of their content, it is difficult to compare them. Nevertheless, both Mishra and Baldwin provide useful contributions to current public policy debates. Mishra's book is a significant antidote to public policy theorists who can see no impact on welfare state policies by neoconservative regimes in the United States and the United Kingdom. He also provides a lucid discussion of the political and economic circumstances that have led to a halt in the expansion of social welfare programs in Sweden and Austria, as well as a worthwhile discussion of the social policy quandaries of the Progressive Conservative party in Canada and the Labour party in Australia. Baldwin's book serves as a corrective to theorists who have viewed the evolution of European social welfare states too exclusively as driven by the policies of labor solidarity reflected in social democratic or labor movements.

University of Maryland HOWARD A. PALLEY

Identity, Competition, and Electoral Availability: The Stabilisation of European Electorates, 1885–1985. By Stefano Bartolini and Peter Mair. New York: Cambridge University Press, 1990. 363p. $59.50.

This volume is an exceptional achievement. Bartolini and Mair offer a theory of electoral stability in European democracies that covers most European democracies between 1885 and 1985. The volume's powerful argument, encyclopedic data base, conceptual richness, and methodological sophistication place it at the top of the reading list for those who would study European electorates.

The research flows from the scholarly agenda established by Stein Rokkan. Following Rokkan's advice, Bartolini and Mair "pin down numbers on the European variations" (p. 5), presenting numerous tables and graphs, as well as a detailed appendix filled with data on electoral volatility in Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Netherlands, Norway, Sweden, Switzerland, and the United Kingdom. By examining aggregate data, they are able to cast a wide net, encompassing elections in countries for which there exist no reliable survey data and that occurred in decades before the advent of polling. The analysis divides a century of democratic competition into four phases: 1885–1917, 1918–44, 1945–65, and 1966–85. At its heart lies a set of related concepts and hypotheses: Electoral volatility (separated into bloc and within-bloc volatility) and cleavage (votes for a party or parties defined as representing a particular class) detail the dependent variable, electoral stability. A general framework offers change in electoral institutions, change in participation, party system format, spatial distance, cultural segmentation, and organizational density as explanatory factors; and a multivariate analysis demonstrates the importance of socioorganizational bonds (organizational density and cultural segmentation) and institutional incentives (institutional change and party system format) as persistent determinants of variation in the level of electoral stability. Variables associated with policy distance influence the dependent variable only in cases where socioorganizational bonds are weak. Bartolini and Mair maintain that European electorates are stable, that Lipset and Rokkan's freezing hypothesis stands, that social class has not declined as a primary basis of political identity, that the postwar era emerges as an extraordinarily stable period, and that recent changes do not mark realignments in electoral politics. The authors mount a spirited defense of Lipset and Rokkan and offer a path-breaking analysis of electoral stability in their own right.

Any judgment about the merits of the argument must address the authors' decisions about their concepts, measures, and exclusive use of aggregate data. Bartolini and Mair (1) treat aggregate levels of electoral volatility as if they were the same as the rates of individual vote switching and (2) test the freezing hypothesis by examining change across blocs of parties, rather than single parties. They provide intriguing, if not quite compelling, reasons for these two decisions. The first rests on finding both a relatively high correlation between vote switching as reported in surveys and as uncovered through the analysis of aggregate results and a striking argument detailing the relatively high probability that the correlation is not due to chance. At the same time, however, their argument ignores the tendency for all forms of data to report artificially high levels of vote stability. The second decision is justified by their reading of the relevant passage in Lipset and Rokkan's text; but their interpretation is not the only plausible understanding. The problem is that Lipset and Rokkan's claim is an empirical generalization, not a hypothesis embedded in a theory with deductively related hypotheses that could be tested. Similarly, the analysis of cleavage examines volatility by aggregating parties into blocs said to represent classes and offers a precise measure of organizational density (tapping how far persons who share a class category interact with each bloc) but then analyzes cultural cleavages with no measures of organizational density. Nowhere does the difference between their results and those obtained from surveys appear more clearly than in the analysis of the effects of social class on vote choice. Bartolini and Mair see strong and persistent effects, maintaining that class remains a powerful source of political identity. This stands in direct opposition to most political scientists, who see declining and weak, if any, relationship between class and vote. By staking out strong positions on these critical matters, Bartolini and Mair have helped to set the agenda for the next wave of research on European electorates.

Bartolini and Mair offer a model of how to do theoretically informed political analysis that remains close to particular events in particular countries. They address central theoretical and substantive questions, present powerful hypotheses, carefully construct variables and measures, and apply appropriate tests to their arguments. As a result, they teach us much about the electoral bases of European democracies.

Brown University ALAN S. ZUCKERMAN

Chinese Marxism in the Post-Mao Era. By Bill Brugger and David Kelly. Stanford: Stanford University Press, 1990. 223p. $32.50.

Philosophy and Politics in China: The Controversy over Dialectical Materialism in the 1930s. By Werner Meissner. Stanford: Stanford University Press, 1990. 230p. $37.50.

China and the Crisis of Marxism–Leninism. By Franz Michael, Carl Linden, Jan Prybyla, and Jurgen Domes. Boulder: Westview, 1990. 213p. $39.50 cloth, $18.95 paper.

These three books are all concerned with the nature and evolution of Marxist–Leninist ideology in its Chinese context, although their areas of focus and methodological approaches differ considerably. There is an underlying pessimism about the doctrine's future in all three books that perchance befits the times; but there is considerable variation in estimates of the intrinsic value of its contributions.

Werner Meissner, whose study focuses on the period of origin of a distinctively "Chinese" concept of dialectical materialism, is perhaps most witheringly pejorative in his assessment of its ideational content. The author's interest in the topic began with a reading of Mao Zedong, and he decided to trace the ideas set forth in "On Practice" and "On Contradiction" to their philosophical background in the intellectual controversies that raged over the "new philosophy" in Shanghai leftist circles in the 1930s. In his analysis of the works of Ai Suqi, Ye Qing, Chen Boda and others, he found them to be "strangely flat and lifeless" and "lack[ing] a genuine philosophical content, compared for example with the original works of Hegel and Marx": "Indeed, they are absolutely empty" (pp. v, 4, 5). He proceeds to dismiss their philosophical import, viewing them as "a self-contained system of analogous thought," a set of "symbols" or "totems" whereby the strategic and political conflicts being conducted inside party councils were expressed publicly, but in esoteric form. This is not immediately apparent upon reading one or two articles; only after reading the entire polemic are the various philosophical schools and their inner-party factional correlates discernible. Once this handful has been identified, everything falls into place, since the myriad lesser figures are found to repeat the themes and phrases of the more seminal thinkers with ritualistic precision. Indeed, Mao himself is found (in part 4) to have engaged in wholesale "plagiarism" (of Ai Suqi) in his work on dialectical materialism.

Meissner's approach is derivative of the "sociology of knowledge." Indeed, he acknowledges his debt to Mannheim. What is distinctive, however, is the scholarly thoroughness and rigor with which he applies it. Thus, the relationships between philosophy (i.e., Marxism) and science, perceptual and theoretical knowledge (concerned with differences and similarities, respectively), analysis and synthesis, induction and deduction, formal and dialectical logic, and dualism and eclecticism are all found to be an elaborate shorthand for debating the proper relationship between parties and armies in the second united front. After Xian—when the compromise was, in fact, negotiated—there are telltale shifts in philosophical positions. The author demonstrates the relationship between philosophical and political argumentation with considerable subtlety but finds it possible to conclude with a glossary that equates the two quite unambiguously.

China and the Crisis of Marxism–Leninism was first conceived by its four coauthors in the fall of 1986, when they presciently foresaw the dire turn of events that would culminate in suppression of China's democracy movement at Tiananmen in June 1989. Their book is essentially a collection of essays, focused for the most part on the post-Mao reform era but ranging quite freely over the entire forty-odd years of communist hegemony in China. Only Jurgen Domes focuses squarely on the ideological dimension, thereby providing a point of comparison with the other two volumes. Methodologically, his approach is not dissimilar to Meissner's; but he is less concerned with ideological controversy and its correspondence to inner-party factional and policy disputes than with the use of an authoritative general line to delineate the road to socialism that everyone must follow. During the pragmatic reform era, he finds the party doctrine to have become "somewhat blurred," with "only little development in the field of ideology since the summer of 1981" (p. 183). By, in effect, arresting further ideological development with his Procrustean four cardinal principles in 1979, Deng unwittingly precipitated a "crisis of ideology," manifested on the one hand by mass indifference to endlessly repeated official doctrines and on the other by spontaneously generated (and officially unwelcome) attempts to introduce ideological innovations by heterodox elements.

These spontaneous ideological contributions are of greatest interest to Brugger and Kelly, whose work constitutes the most sustained analysis yet of doctrinal developments in the post-Mao era. Their topical concerns thus coincide most closely with those of Meissner, focusing on the ideological controversy that flourishes before an authoritative line has been laid down. Yet methodologically, they are light–years apart; for while Meissner derisively dismisses the philosophical pretentions of his authors, Brugger and Kelly are interested in little else. "This book . . . is not a work of political science that seeks to trace networks of influence and power," the authors state: "It is about political, social, economic, and philosophical ideas" (p. 14). While the authors have a political subtext in that (like Domes) they hold Deng responsible for pretermission of the reform of Marxist dogma, this is the most purely "academic" analysis of Chinese Marxism that has yet appeared. The authors adumbrate Chinese discussions of the Hegelian notion of totality, functionalism and systems theory, structuralism à la Althusser, Jin Guantao's notions of the dialectics of nature, the objectivity of economic laws (such as the law of value), pragmatism (a favorite of Deng's), the implications of alienation, and Marxist humanism. These sometimes esoteric debates are related not to inner-party struggle but to parallel developments in Eastern European or Western Marxism (e.g., Lukàcs, the Frankfurt school, Popper). Perhaps this methodological shift reflects reality. If Meissner's Marxists were intellectual soldiers in a revolutionary war, contemporary Chinese theorists may wish to detach themselves from the political melee. In discussing Chinese Marxism's academic implications and growing integration with intellectual developments abroad, Brugger and Kelly have pointed to an important trend in its evolution.

From the perspective of political science, what seems to be needed is some sort of synthesis of these three

approaches. A sociology of knowledge is still required, for the simple reason that much as some academic theorists might wish to detach themselves from politics, Marxist theory is still highly politicized—as is revealed not only in the continuing tendency of politicians to intervene in academic or literary debates but in repeated efforts by intellectuals to influence political tides. Yet to demonstrate the political utility of these debates surely does not necessarily imply a denial of their intellectual connotations. Lenin's contributions can also be shown to have had political utility, as, indeed, can many of Marx's. This is to be expected in any philosophical tradition built upon the quest not merely to rethink, but to change, the world. Thus, an academic analysis à la Brugger and Kelly is quite appropriate, though it should be leavened by somewhat more attention both to the political upshot (as in earlier writings by both authors) and to the relationship between ideological superstructure and political authority, which assumes pride of place in the approach of Domes and others.

University of California, Berkeley LOWELL DITTMER

East Asian Dynamism: Growth Order, and Security in the Pacific Region. By Steve Chan. Boulder: Westview, 1990. 134p. $35.00 cloth, $10.95 paper.

The Politics of Development: Perspectives on Twentieth-Century Asia. By Robert A. Scalapino. Cambridge: Harvard University Press, 1989. 137p. $19.95.

Scalapino's book stems from his lectures at Harvard in 1988, in honor of Edwin O. Reischauer. No doubt Mr. Scalapino was under no injunction other than to teach according to his best lights; and the result is graceful writing, informed and deliberate analysis, and plenty of food for thought. His book illustrates the bargain we all make as teachers. With the freedom to express ourselves as we see fit, we are obliged to explain what we know clearly, without artifice, and with respect for our audience.

Chan's book does not meet that test. It is often extremely patronizing; it overuses italics and boldface to highlight "special" words and concepts and the allegedly newfound dramatic conflicts faced by nations, which form the theme of the series; and it strains pretentiously to spell out the book's "architecture" and its "thematic elements."

Both books are concerned with roughly the same goal, namely, to describe, in a relatively brief and presumably an introductory way, the main patterns of East Asian development since World War II; and they often discuss the same issues. For both, the central question is what factors have led some East Asian and Pacific nations to achieve great and positive changes in their peoples' well-being while others have stagnated or even slipped back.

Chan, however, fights the evidence. While very familiar with the classical economic issues involved, he does not want to say that an economy based on individual incentives does the most for its people. In Asia (as elsewhere) the argument about the best path to development has revolved strongly around the conflict over export promotion versus import substitution policies. Scalapino knows where he stands, and tells us plainly. Import substitution has not worked and has led to other policies and structures (e.g., strong monetary controls) that have set nations like Argentina and Brazil behind where they were years ago.

On these issues, Chan equivocates, writing that "categorical assertions regarding the merits of either laissez-faire or command economies are not likely to be very informative" (p. 47). He also tells us (without any qualification) that "one of the hallmarks of the capitalist East Asian economies has been the political subjugation of their industrial workers" (p. 53). It is true enough for some, but what about Japan, or even Singapore? It is also hard to swallow his remark that the weakness of organized labor in East Asia has led its workers to fare in unemployment benefits, maternity leaves, minimum wage laws, occupational safety standards, and old-age pension funds much worse than their North American and even their Latin American counterparts (p. 53). It may be true of Costa Rica, but what about Mexico, Brazil, or Argentina?

His prejudices are evident also in connection with the much-maligned multinationals: "Fortunately for the capitalist East Asian [newly industrialized countries], the [multinational corporations] did not view them as attractive investment sites in the 1950s" (p. 55). In that period, most multinational corporations were active in the extractive industries; and neither South Korea, Taiwan, Hong Kong or Singapore are known for their mineral deposits. But why say "fortunately" unless to suggest that as a general matter, the involvement of multinational corporations is always negative for a nation's economy? But then, why does every developing nation today want to attract more, not less, multinational corporation investment?

At too many points, moreover, Chan is simply wrong on his facts. For example, he writes of the "inability of U.S. manufacturers to penetrate the East Asian markets" and asserts that "Japanese products sell much better than U.S. products in Taiwan, South Korea, and Southeast Asia" (pp. 52–53). Neither is true, as the quickest glance at actual trade figures over the past generation will demonstrate. American manufactured goods have held a rock-steady market share in some of these markets and have risen in others.

Scalapino writes, "I reject the view that imperialism was the controlling . . . factor in retarding China's development until 1937 . . . . The basic problems China faced . . . lay within China [and were] not governed by external factors" (p. 7). His message throughout is that internal factors and sound political leadership have been the crucial determinants for national improvement. He also continues his long effort to demonstrate the failures of Leninism and will infuriate many by saying that in Asia, two groups are potentially the "ultimate enemies" of democracy: the students and the media (p. 100).

But Scalapino shows us whence he arrives at these views. Untroubled by the trendiness that characterizes Chan, his is ultimately the far more satisfying effort. It is a sound introduction that will encourage beginners to explore further a mature synthesis with plenty of interpretation for the specialist to argue about. Chan, on the other hand, will only infuriate the specialist with his tendentiousness and misstatements, and his scholasticism will be off-putting to the novice.

University of New Hampshire BERNARD K. GORDON

The Political Economy of China's Special Economic Zones. By George T. Crane. Armonk: M. E. Sharpe, Inc., 1990. 204p. $42.50.

China's Foreign Trade Reforms: The Impact on Growth and Stability. By John C. Hsu. New York: Cambridge University Press, 1989. 221p. $49.50.

China's opening to the outside world is the most discussed aspect of China's economic reforms. China has become a major trading nation, receiving more direct foreign investment in the 1980s than any other Third World state. In contrast to the former socialist states of Europe, China's foreign trade and its economy have continuously expanded since the reforms began. In short, China has been integrated into the international economy, and is an increasingly important factor in world trade and capital flows.

These two books present different facets of China's involvement with the international economy in the 1980s. John Hsu's book is an economic examination of China's trade performance, particularly in the 1979 to 1985 period. He is particularly concerned with the issues of trade's contribution to economic growth and the effect of trade on inflationary pressures in China. In contrast with Hsu's macroeconomic and econometric analysis, George Crane offers an in-depth case study of the varied experiences of China's special economic zones, covering the period from 1979 to 1987. (Crane has updated his analysis in several recent publications for those who wish more recent details.)

Political scientists studying China will probably find the first half of Hsu's book more useful and accessible than the second part. However, it is in the second part where Hsu probably makes the greater contribution to scholarship. The first half of the book is a largely descriptive analysis of the role of foreign trade in China's economic development, both before and after the onset of the reforms, and a discussion of the changing commodity composition of China's trade both in general and in relation to its major trading partners, Hong Kong, Japan, and the United States. Hsu argues that there is a strong correlation between expanded foreign trade and periods of economic expansion in China. From this, he implies that trade causes domestic economic expansion in China. But because he does not break trade figures down by type of imports, his correlation and his inference may be overly broad. For example, grain imports will have little to do with economic expansion in China. Moreover, given that trade prior to 1978 was roughly about 5 percent of GNP at best, it would seem that Hsu has the tail wagging the dog with his argument that trade drives expansion.

Unfortunately, Hsu seems to have taken 1985 as his cut-off date out of convenience rather than as a date marking a fundamental turning or end point. At some point we all have to come to a point of closure, but by stopping in 1985, for a book published in late 1989, Hsu risks seeing his work outdated in short order.

The second half of Hsu's work is more interesting though much more difficult for political scientists studying China to follow. Here Hsu, drawing on the work of Richard Portes and others, tries to measure the effects of trade on consumer shortage in China. The work is heavily econometric, though the basic argument can be gleaned without too much trouble. Hsu concludes that trade since 1979 has had a less inflationary impact than trade prior to that date. The arguments about trade and consumer shortage are interesting, perhaps because political scientists studying China might not ordinarily think about such questions. Whether economists will be persuaded by Hsu's analysis of this issue is beyond my ability to assess.

Hsu's book is useful and there is much that political scientists can learn from it. It is already and increasingly dated however, and people familiar with Chinese trade will know much of the story already. Not surprisingly, it lacks almost any form of political analysis.

In contrast, George Crane's book is in many respects the inverse of Hsu's. Crane's work is an intensive case study of Chinese special economic zone (SEZ) policy based on political analysis, largely arguing that the dynamics of SEZ are explainable by internal dynamics of the Chinese state. Crane argues that "[p]olitical and policy instability are consequences of bureaucratic underdevelopment and ingrained personalism" (p. 163). By bureaucratic underdevelopment, Crane means that the bureaucracy lacks coherence, or that there is overlapping and confusing jurisdiction among the units of the Chinese bureaucracy. At the same time, he argues that the Chinese state is also overdeveloped to the extent of its extensive involvement in the Chinese case. Crane also makes the interesting observation that through the expansion of and publicity about SEZs, the conditions of their potential disappearance are being laid (p. 145). Other cities and regions all desire their own access to direct foreign investment and are lobbying the central government for their own special trading powers.

Crane's work is an insightful and useful study of the origins and developments of the SEZs that will be a constant reference source for people working on them. I think Crane's internal focus may be somewhat overexaggerated; after all, if foreign investors do not invest, or their investments fluctuate, it is not surprising that SEZs fail to live up to the (exaggerated) expectations placed upon them. Indeed, another way to read the story of the SEZs is that in bowing to investor concerns, the SEZ environment is increasingly attentive to foreign concerns. There may have been internal contention over the liberalization of SEZ terms, but the driving force remained the international environment.

Both Hsu and Crane would have us believe that the contradictions inherent in the Chinese reform process mean that reform will stall or fail. But we are now 5-7 years after the periods covered by these books. The Chinese have not made the kinds of reforms that both authors call for to maximize the benefits from China's opening to the outside world. Yet Chinese trade continues to grow and direct foreign investment continues to increase (despite the Tiananmen Massacre of 1989). All of this suggests that the dictates of economic theory or administrative rationality have little to tell us about the nature of the Chinese reform. Chinese leaders had to reform China given the structure of the system they created or inherited. They lacked the power and the inclination to undertake "shock treatment" to transform China's economy from socialism to capitalism. Given the comparative performance of China and the former socialist states of Europe, China's muddling through process of economic reform, with all its attendant contradictions, may have been the best model for other socialist or former socialist economies.

University of Washington, Seattle DAVID BACHMAN

Interest Groups and Political Change in Israel. By Marcia Drezon-Tepler. Albany: State University of New York Press, 1990. 308p. $49.50 cloth, $16.95 paper.

Stiff-necked People, Bottle-necked System: The Evolution and Roots of Israeli Public Protest, 1949–1986. By Sam N. Lehman-Wilzig. Bloomington: Indiana University Press, 1990. 213p. $25.00.

Struggles over the nature and purpose of Jewish political sovereignty dominated the early years of Zionist history. The founding fathers and mothers of the Zionist movement, who were Israel's political pioneers, engaged in extended debates over the extent to which a Jewish state ought to be "normal" and resemble other nation–states, as if by speech alone they could mold the characteristics of the Jewish state. Ironically, the debate itself seems to have had little pertinence for Israel's evolving society and polity. Implicit in Lehman-Wilzig's *Stiff-necked People, Bottle-necked System* and Drezon-Tepler's *Interest Groups and Political Change in Israel* is the notion that Israel has (1) become more normal as a democracy and (2) produced a more intensely Jewish political culture. In one way or another, Israeli political behavior provides the empirical, if disputed, terrain for observations on these two centripetal developments and specifically, why they have endured. Methodologically, the studies are quite different. Lehman-Wilzig's is a sophisticated statistical analysis. Drezon-Tepler's gathers data from interviews and the records of three important organizations with influence on policymaking: the Manufacturers' Association, the Ihud Kibbutz Federation, and Gush Emunim (the last committed to building Jewish settlements on the West Bank). Both studies are insightful and thoughtful, although the first may present a more useful angle of vision from which to evaluate Israel's polity.

In the literature on Israeli politics, scholars tend to focus on political parties and to discount the political influence of other organizations. With or without hegemony, contending parties have occupied ministries in coalition and often pursued highly divergent policies. Notwithstanding a high voter turnout, elections have not been decisive mechanisms for resolving policy conflicts or for determining priorities.

Aware that elections and other formal institutional mechanisms will not address issues of fundamental importance to citizens, a growing number have taken to demonstrations and protests. Public protests have come to play a significant part in communicating political demands to the established government. Electoral stalemate and an increasingly complex society have prevented Israel's elected rulers, whatever their political affiliation, from addressing many of the country's endemic problems. Without the high visibility of street protests and demonstrations, more and more Israelis of all classes and from all sectors—rich and poor, educated and uneducated, rural and urban—have come to believe that their needs, however legitimate, will be ignored. As political parties become more distant from their members, they no longer either provide accessible channels for communication or serve as agencies for resolving disputes. Protests, in part, redress the problems induced by proliferating bureaucracy.

Lehman-Wilzig explores the incidence of street demonstrations and extraparliamentary activities and generates a profile of the participants and their major demands. Attending also to their tactics, particularly the use of force and violence, he argues that public protests in Israel erupt because of a Jewish cultural disposition to be critical and a Jewish penchant for reform and because Israeli political institutions operate so unevenly.

Does the picture of Israeli political life so ably drawn hold any surprises? Only for those who assume that Israel's political development has been entirely exceptional. As Israel's economy has become increasingly industrialized and its population more diversified, traditional political institutions have been strained. In Israel's first two decades, a dominant political party achieved sufficient votes to broker a governing coalition and direct the most powerful ministries, including, among others, the office of prime minister, the ministries of foreign affairs and defense, and the treasury. Israeli political parties still provide an impressive array of services, answering many of the basic needs of Israeli citizens. Nevertheless, access to power and to the distribution of resources has always been limited.

Reflecting the willing recourse of Jews to criticism and reform, extraparliamentary associations and movements have continually cropped up to demand new and better solutions to old problems. In their marches and confrontations with authorities, however, protesters have mapped a political order considerably tangential to the one rooted in long-standing Jewish traditions or imagined by Zionist pioneers. While this political development was authentically Jewish, it, too, mimicked trends in other societies.

That interest groups operate in Israel is an indisputable assertion. Whether or not interest groups have decisive influence over policy is arguable. Subsuming Gush Emunim under the interest group category may strengthen Drezon-Tepler's claim that interest groups shape public policy; but the classification itself might invite considerable controversy. The claim that interest groups, rather than wars or territorial and demographic shifts, have transformed Israel's polity infuses Drezon-Tepler's analysis; but it, too, may be disputable. Many political developments in Israel, framed in the wake of international trends, seem way beyond the control of Israeli politicians. The Jewish state has become more entangled than ever in Jewish history in international politics. Israel's viability depends heavily on the balance of international forces.

The two books present contrasting comments on both the distribution of power in Israe and the process of political change. Drezon-Tepler posits that ongoing, multilayered contacts exist between government policymakers and the leadership of grassroots organizations, with these interactions shaping the contours of public policy. Lehman-Wilzig adopts a skeptical view of the pluralist model adopted by Drezon-Tepler and builds his argument around the notion that public protests signify the breakdown of communication and normal political procedures. Israelis take to the streets in order to be heard by the powerful.

Zionist history has imposed a weighty burden on Israelis and on Israeli society. For decades, aspects of this Zionist legacy have been subjected to criticism in the best works of Hebrew literature. Juxtaposing these two books suggests that political studies are beginning to join the humanities in probing the conventional wisdom from new perspectives. In the process, our knowledge

and understanding of Israel's polity and society will be immeasurably enriched.

Smith College DONNA ROBINSON DIVINE

Democracy and Markets: The Politics of Mixed Economies. By John R. Freeman. Ithaca: Cornell University Press, 1989. 311p. $39.95 cloth, $12.95 paper.

Conservative Capitalism in Britain and the United States. By Kenneth Hoover and Raymond Plant. New York: Routledge, 1989. 341p. $45.00 cloth, $16.95 paper.

With their election victories in 1979 and 1980, Thatcher and Reagan initiated campaigns (which were to last for the better part of a decade) against big government—the alleged source of the several economic ills that afflicted both nations in the 1970s. Both leaders won reelection by substantial margins. And for a time, at least, in the mid-1980s, as growth rates soared and inflation rates plummeted, conservative capitalism appeared to represent the necessary antidote to the stagflation that had plagued both countries in the 1970s.

Reagan and Thatcher are no longer in office; and as the United States and Britain have experienced once again in recent years low growth, accelerating inflation, rising unemployment, recession, and looming budget and trade deficits, the putative achievements of both administrations seem more ephemeral. That being the case, it is appropriate for scholars to compare and assess the enduring impact of the Thatcher and Reagan governments. One of the most detailed and explicitly comparative assessments to date is that provided by Kenneth Hoover and Raymond Plant in *Conservative Capitalism in Britain and the United States*.

Hoover and Plant argue that in their antipathy to big government, endorsement of markets, and efforts to shift resources from the public sector to the private sector, Thatcher and Reagan represented a more aggressively conservative form of capitalism than had existed in either government in the previous half-century. Arguing, correctly, that Thatcher and Reagan demonstrate the continuing importance of ideology in politics, Hoover and Plant begin their analysis by examining the core values and beliefs of conservative capitalism. The conservative critique enshrined monetarism, which implied reductions in spending and in budget deficits and increases in real interest rates; supply-side economics, which implied tax cuts to assist investment; deregulation; and privatization.

Why did Reagan and Thatcher become the symbols of the conservative critique of big government? Hoover and Plant suggest that while neither politician invented the critique (that was done, in bits and pieces, by academic economists, journalists, and other politicians well before either took over as party leader), both found the new conservatism not only ideologically congruent with what they already believed but very useful, as well, in their battles for the party leadership against more moderate and incumbent conservative leaders (e.g., Richard Nixon, Gerald Ford, and Edward Heath). Equally interesting, one surmises from Hoover and Plant that the new leaders' subsequent electoral successes in 1979 and 1980 may have had less to do with the specific tenets of this new conservative critique than with the persona of the leaders and the electorates' considerable distaste for the Democratic and Labour party incumbents and their policies.

Be that as it may, the two leaders governed for much of the next decade; and Hoover and Plant present an excellent and useful summary of the economic and social policies enacted by both administrations. If the discussion and appraisal of the implementation of those policies in Part 2 seems more detailed and complete for the British case, that may reflect the greater scope and implementation of the Thatcher program. Whether that was in fact the case (and if so, why) remains, unfortunately, something of a mystery, since the authors eschew a systematic comparison of the extent of programmatic and policy change in both governments, preferring simply to describe separately the policy of each. One wonders, for example, why Thatcher's government was considerably more successful than Reagan's in reducing the size of the budget deficit and (after the recession of 1980–81) the aggregate size of public spending relative to gross domestic product. Did this reflect Thatcher's greater commitment to the conservative critique? Did it reflect her greater capacity to impose her will upon her cabinet and government? Did it reflect the greater institutional coherence (some would say subservience) afforded by a three-term majority in a parliamentary system (in contrast to the House Democratic majority Reagan faced)? Or did it simply reflect the fiscal bonus generated by Thatcher's privatization campaign, a source of revenue not available to the Reagan administration?

The most provocative—and perhaps problematic—discussion in the Hoover and Plant volume appears in Part 3, where the authors attempt to provide the outlines of a response from the Left to conservative capitalism. The authors suggest that the Left (whose definition, particularly in the United States, remains imprecise) must accept the market, the need for economic incentives, private ownership, a certain "tolerable" degree of inequality in incomes, the centrality of production, and the constraints imposed on government by the economy. Then the Left can then develop a theory of needs, of which some (perhaps pertaining to health, education, welfare, and employment) could be provided by a "minimal" welfare state without adversely affecting the performance of the economy.

However well-meaning—and perhaps even laudable—these authors' suggestions may be, the discussion is, unfortunately, quite imprecise and general. We are told, for example, in regard to the United States, that voters would be offered "real choices," "realistic policies," and "credible programs." What those choices, policies, and programs would be is unclear, beyond the claim that they would aim to raise the incomes of the poor and provide jobs for all. How they would be funded (By cutting other programs? By raising taxes?) and how those objectives would be achieved (especially in the devastated inner core of urban America) remain unanswered questions. Likewise, we are not told how the Left would attempt to improve the efficiency, productivity, and rate of growth in the economy and, in particular, whether it would pursue distinctive policies to achieve those goals or simply mimic the conservative parties. Finally, we are not told in this otherwise excellent study whether the necessary reconstruction of the programs of the left parties is politically feasible or likely. Nor are we told why we should expect the voters of the two countries to be enamored enough of left

parties with these new policies to provide the support necessary to recapture and hold national executive office.

As in Hoover and Plant's discussion of privatization in Britain, the role of public ownership figures prominently in John Freeman's *Democracy and Markets*. Indeed, *Democracy and Markets* is, above all, about the welfare implications of a mixed economy (i.e., one in which a significant portion of investment, production, and employment occurs in publicly owned enterprises) in an advanced capitalist democracy. In particular, Freeman seeks to elaborate how the presence or absence of significant public ownership (as well as of a corporatist mode of interest intermediation) affects both the performance of nations in regard to the level and rate of societal consumption and wealth of future generations ("*inter*generational equity") and the distribution of wealth between asset holders and wageearners ("*intra*generational equity"). How nations perform in these distinct aspects of collective welfare (more simply, using indicators from chap. 6, economic growth and distributional equality) depends critically, Freeman argues, upon the distinctive blends of a mixed, or private-enterprise, economy and a corporatist, or pluralist, mode of interest representation.

Despite the common concern with public ownership and private enterprise, the two books differ markedly in intellectual style. Whereas the Hoover and Plant volume is largely a description of the antecedents, driving ideologies, and practices of the Thatcher and Reagan governments, *Democracy and Markets* is driven by a more abstract and theoretical ambition to understand how and why capitalist democracies vary. Thus, while Freeman derives a typology of system types that yields a set of predictions about the welfare consequences of each type that can be compared with the actual outcomes of specific countries, he is not much interested in political parties, ideologies, or the policies of particular governments. Nor is he concerned with developing a full explanation of why the capitalist democracies vary in both the rate of economic growth over the long term and the degree of distributional equality. Instead, he is concerned with using the experiences of particular countries (Britain, Austria, Sweden, and Italy) to validate—and, if necessary, amend—his logico-deductive analysis of the characteristic blends of welfare outcomes produced in different types of political-economic system. And whereas Hoover and Plant conclude their study by framing the outlines of a programmatic response by the Left to conservative capitalism in Britain and the United States, Freeman concludes (also on a normative note, but more abstractly) by identifying what he believes to be the most desirable form of political-economic system in terms of achieving both intergenerational and intragenerational equity.

Freeman begins his analysis by identifying two dimensions along which the capitalist democracies can be arrayed. One involves the extent of public ownership of enterprises. The other involves the extent to which a corporatist mode of interest representation exists. Dividing each dichotomously thus yields a fourfold typology of political-economic system: corporatist with a mixed economy, corporatist with a predominantly private enterprise economy, pluralist and mixed, and pluralist with private enterprise.

Freeman then argues that each of these four system types produces a distinctive blend of welfare outcomes. The argument proceeds from two claims about corporatist and mixed systems: (1) corporatist systems achieve, all else being equal, higher rates of intergenerational equity (growth) and greater intragenerational equity (equality) than pluralist systems and (2) mixed systems achieve lower rates of intergenerational equity but higher levels of intragenerational equity than private enterprise systems. That being the case, corporatist–private enterprise systems should achieve higher rates of growth and greater distributional equality than pluralist–private enterprise systems. Corporatist–mixed systems should achieve lower rates of growth but higher levels of distributional equality than corporatist–private systems (and hence, in comparison to pluralist–private systems, roughly comparable levels of growth but considerably greater levels of equality). And pluralist–mixed systems should achieve lower rates of growth than either the pluralist–private enterprise system or the two types of corporatist system. But they should achieve somewhat higher levels of distributional equality than pluralist–private systems.

Having developed this complex set or arguments, Freeman moves to assess their validity. In this exercise, the author relies on four nations to represent the system types: Britain (pluralist–private enterprise), Sweden (corporatist–private enterprise), Austria (corporatist–mixed), and Italy (pluralist–mixed). After a brief cross-national analysis, Freeman discusses the performance of each of the four nations in detail and assesses the extent to which each nation's performance corresponds to the predictions. In the course of this discussion, he attempts to account for various anomalies that appear in the cross-national analysis or in the consideration of the four cases (e.g., the relatively high level of intergenerational equity in Italy but low levels in Sweden and Britain and the apparently low degree of distributional equity in Austria but higher-than-expected levels in Sweden and Britain). After a provocative discussion of the impact of openness on the welfare outcomes of the various system types, Freeman concludes the book with the analytic argument essentially intact. And shifting to a normative mode, he closes by arguing that based on the analytics, the corporatist–mixed type is the "most desirable form of democratic capitalism" because of its "superior welfare outcomes" (pp. 284, 286).

For all its analytic elegance, *Democracy and Markets* is not without shortcomings. Indeed, both those interested in the substantive issues of growth, stagnation, and distributional equity and those interested in, and informed about, European politics and economics will find this book frustrating and, in some respects, disappointing. For one thing, Freeman is engaged in what might be called marginal or partial theorizing. His ultimate ambition is not to explain why nations vary in terms of growth and equality but to ascertain whether different system types display different blends of those welfare outcomes. While the former task requires a full elaboration of all the factors that might account for variations in those outcomes and an assessment of their relative importance, the latter confines itself to only a few (in this case two) hypothesized sources of variation and requires only that discernible differences be observed among the system types. However elegant the supporting argument, such an approach is ultimately limited in scope and in its theoretical claims. Because of the inherent limits of this form of theorizing, readers who search *Democracy and Markets* for general explanations of why

nations vary in rates of growth and distributional equity will find only a scattered assortment of ad hoc, country-specific factors brought forward to explain why apparent deviations from the theory-generated predictions do not weaken the theory. Thus, in regard to economic growth, various factors are enumerated to account for Britain's unusually low rate, including lack of private investment, the poor "entrepreneurial spirit," the overvalued currency, the exit option of capital, etc. Italy's unusually high rate of growth is attributed to the vitality of private enterprise. Sweden's low growth is explained by the deleterious effects of parliamentary and electoral politics, the concentration of Swedish capital, the decline of corporatism, and so on. A similar concoction of ad hoc, country-specific factors is mentioned to account for apparent deviations in the degree of distributional equity (although Freeman is less successful in accounting for these, perhaps because he largely ignores the distributional impact of taxation and expenditure policy). In other words, he specifies and evaluates neither the factors that systematically cause the rates of growth to vary across nations over the long term nor those which account for variations in degree of distributional inequality.

If the limited scope of the theorizing in *Democracy and Markets* will frustrate those concerned with substantive issues of growth and equality, the confrontation between theory and evidence will frustrate those who are knowledgeable about Europe. For example, the theory asserts that corporatist–private systems (Sweden) will have the highest rates of intergenerational equity and pluralist–mixed systems (Italy) will have the lowest rates. In fact, however, as Freeman's all-too-brief cross-national analysis suggests, the actual performances of the two were reversed. Indeed, over the period 1976–89, Sweden had an average growth rate of 1.8%, one of the *lowest* in the OECD and well below those of Britain and Austria (both 2.5%). And Italy had the *highest* rate of economic growth among Freeman's four cases, 3.2%! Similarly, while the theory claims that corporatist–mixed systems (Austria) will have the highest level of intragenerational equity and pluralist–private systems (Britain) the lowest, in fact (as Freeman acknowledges on pp. 173, 182), Austria's degree of distributional equity, in terms of income, is roughly comparable to that of Italy (one of the most inegalitarian systems in Europe) and considerably lower than that of Britain (or Sweden).

Such anomalies can, of course, be rationalized and explained in an ad hoc and unsystematic manner. But their existence may also be indicative of a fundamental mistake in theory and analysis. In particular, these several anomalies—involving, as they do, outcomes that are precisely the reverse of those predicted by the theory—suggest that Freeman may have misconceived the impact of corporatism, public ownership, or both on intergenerational and/or intragenerational equity. The magnitude of those anomalies suggests that the theory is in need of repair. Perhaps (some scholars might argue) the existence of a mixed economy affects neither the rate of growth nor degree of equality over the long term. Or perhaps it enhances (rather than diminishes) the former and diminishes (rather than enhances) the latter. And perhaps, (as the large literature on corporatism, only partially sampled by Freeman, might suggest) the impact of that form of interest representation on growth and equality is more complicated and varied than his theory implies. Perhaps corporatism enhances distributional equity only under certain conditions. And perhaps it enhances growth under certain conditions but diminishes it under others. Unfortunately, rather than considering these possibilities and the alterations in theory they imply, Freeman chooses instead, in the second half of the book, to preserve his theory despite the fact that it significantly mispredicts the performance of each of his cases on one or both aspects of collective welfare.

Whatever the real effects of a mixed economy and corporatism on economic growth and distributional equity, the incongruities between theory and reality seem so pronounced that Freeman must have either misspecified some or all of those effects or failed to identify other attributes essential to an explanation of why nations vary in those welfare outcomes. Thus, for all the analytic power deployed in *Democracy and Markets*, it seems to me that much remains unknown about why nations vary in level and degree of economic growth and distributional equality and how and why corporatism and public ownership affect those outcomes. And as a result, it seems at best premature—and in all likelihood simply wrong—to conclude, as Freeman does, that a corporatist–mixed system is the most desirable form of democratic capitalism.

Yale University DAVID R. CAMERON

Hope for South Africa? By L. H. Gann and Peter Duignan. Stanford: Hoover Institution Press, 1991. 225p. $9.95.

A Democratic South Africa? Constitutional Engineering in a Divided Society. By Donald L. Horowitz. Berkeley: University of California Press, 1991. 293p. $24.95.

These two books, published in early 1991, are timely analyses of South Africa's democratic prospects—a long-debated question that has assumed special significance since President F. W. de Klerk's dramatic policy shifts announced in February 1990. The authors are guardedly optimistic, although they also agree that South Africa is a deeply divided—and deeply troubled—multiethnic country. They strongly disagree with Heribert Adam and Kogila A. Moodley's assessment in *South Africa without Apartheid* (1986) that South Africa is more a "common" than a "plural" society. In his first two superb chapters Horowitz presents abundant evidence documenting the depth of both interracial and interethnic conflict, exacerbated by what he calls the "metaconflict"—strong dissensus on what the conflict is about, especially the wide gap between the common society interpretation and Horowitz's own much more persuasive view that once white domination is eliminated, "intra-African differences will be particularly important" (p. 85). Horowitz also correctly points out that Adam and Moodley's benign common society diagnosis is contradicted by the "strong [consociational] medicine they prescribe" (p. 138).

A major difference between the two volumes is that Gann and Duignan devote a great deal of attention (almost half of their book) to an excellent systematic analysis of South Africa's economic and strategic importance in the international—regional and global—context (chaps. 6–7 and parts of chaps. 2–4). In chapters 8–9 they turn to political and constitutional options. They recommend a consociational, or power-sharing, system, based

on proportional representation and coalition government, as the most promising method that can create a peaceful South Africa "albeit with great difficulty" (p. 167). In particular, they emphasize the problem of reconciling the insistence on unqualified majority rule and nationalization by the African National Congress (ANC) with the National party's attachment to minority group rights and private ownership. Their exaggeration of the South African Communist party's influence on the ANC adds to these worries.

Horowitz explicitly rejects power sharing, as well as other less frequently proposed solutions, like partition and the Zimbabwe model of white overrepresentation (chap. 4). He supports federalism, which can be a useful component of power-sharing systems; but he distrusts coalition building in proportional representation systems. Proportional representation does have the useful tendency to create a multiparty system in which no party has a majority of the parliamentary seats and in which multiparty coalitions have to be formed: "[But] the mere need to form a coalition will not produce compromise. The incentive to compromise, and not merely the incentive to coalesce, is the key to accommodation" (p. 171). Coalescence and compromise are, indeed, analytically distinct; and Horowitz cites plenty of examples of coalitions that have been unable to compromise and that, as a result, have fallen apart. But of course, there are also many contrary examples; and logically, the desire to coalesce implies a need to compromise. If parties are interested in gaining power (a basic assumption in political science), they will, in multiparty situations, want to enter and remain in coalition cabinets (a basic assumption of coalition theory); and then their only choice will be to reach compromises with their coalition partners.

While Horowitz underestimates the moderating effect of coalition building per se, he is right that additional incentives to compromise would be helpful. In addition to "seat pooling" (forming government coalitions), he therefore recommends "vote pooling"—electoral systems (especially the alternative vote) that encourage parties to appeal across ethnic boundaries. The alternative vote asks the voter to rank-order the candidates; if no candidate receives a majority of first preferences, second and subsequent preferences are redistributed until a majority emerges. For instance, if three candidates—A, B, and C—are supported by 45%, 40%, and 15% of the voters respectively, C is eliminated; and the ballots with C as first preference will be reallocated according to second preferences, making either A or B the winner. The great advantage, Horowitz argues, is that A and B have to bid for the second preferences of C's supporters in order to win, which will reward moderation. The problem is that precisely the same argument can be used—and is frequently used—in favor of plurality elections. In the same hypothetical example, A and B have to appeal to the voters who prefer C but do not want to waste their votes on C's hopeless candidacy or cannot vote for C because C wisely decides not to pursue a hopeless candidacy. Yet Horowitz rejects plurality, approvingly quoting Sir Arthur Lewis's well-known conclusion that "the surest way to kill the idea of democracy in a plural society is to adopt the Anglo–American electoral system of first-past-the-post" (p. 164).

The alternative vote resembles the majority runoff method even more closely, except that C is now eliminated in the first round, and A and B have to compete for the votes of C's supporters in the runoff. But it was exactly the majority runoff that was replaced with proportional representation in several ethnically heterogeneous European countries around the turn of the century because it raised ethnic tensions. Unfortunately, there is only one clear case of the use of the alternative vote in a plural society (Sri Lanka's presidential elections), where, Horowitz admits, it has not worked well (pp. 191–93). The dubious value of the alternative vote also weakens Horowitz's recommendation of presidentialism (largely based on the alternative vote's supposed moderating effect), which has to be weighed against the disadvantage of concentrating executive power in one person instead of sharing it among a broad coalition of leaders.

Since the completion of the two book manuscripts in the second half of 1990, the main South African parties have moved closer to a constitutional settlement and shown greater flexibility than most observers had expected. Horowitz worries about the ANC's "hostility to changes from first-past-the-post" (p. 268)—a concern shared by Gann and Duignan. But in the meantime, the main parties, including the ANC, have come out in favor of proportional representation (as well as parliamentary, instead of presidential, government). Such progress shows that there are grounds for even more optimism than these already reasonably sanguine books convey.

University of California, San Diego AREND LIJPHART

Isolated States: A Comparative Analysis. By Deon Geldenhuys. New York: Cambridge University Press, 1990, 764p. $79.50.

It is a pleasure to report that Deon Geldenhuys has produced a truly impressive work on "isolated," or "pariah," states." It is both an exhaustive research effort and an important contribution to international relations theory and comparative foreign policy, combining historical analysis with an impressive array of statistics and rummaging into every conceivable corner of the subject.

The book's title is itself interesting. The author appears to be eschewing the catchy but value-laden and pejorative term *pariah states* in favor of the seemingly more neutral or objective *isolated states*. This also provides him a perhaps more favorable vehicle for extending his analysis back in time to search for historical precedents to the recent situations of Israel, South Africa, Taiwan, and Chile. But it is obvious that these four states are his central concern; and he is not inhibited about labeling them pariahs. In actuality then, pariah states becomes a contemporary subset of the broader and longer-term phenomenon of isolated states. The latter term is used to put the former term in historical perspective.

The book's strength is made apparent in two introductory conceptual chapters preceding the case studies: one laying out the framework of the problem with definitions, typologies, and categorizations, the other devoted to earlier (and some almost current) historical examples of isolated or quasi-isolated states.

Among the foci of the conceptual framework are isolation as voluntary or imposed by others and (tapping a vast theoretical literature) isolation as the counterpole of integration. Otherwise, the author lays out a panoply

of variables used to measure isolation under the headings of political, economic, military, and sociocultural factors. In the political category are discussed the pariah image, international recognition, diplomatic relations (number of missions), membership in intergovernmental organizations and participation in conferences, involvement in international treaties, official visits to and from foreign countries, and levels of censure and support in intergovernmental organizations. Trade, investment, and aid statistics are comprised in the economic category; the military sphere encompasses military agreements, arms purchases, and military aid and cooperation either received or dispersed. The sociocultural factors include cultural agreements, travel, tourism, air and sea links, sport, art, entertainment, and academic and other exchanges. All of these variables are systematically examined and used to compare the cases of Israel, South Africa, Chile, and Taiwan.

The author's framework also entails an examination of the basic structural and functional components found in the process of isolation, suggesting how the empirical data might be fruitfully integrated. That involves, among other things, the target of isolation as directed primarily at the government or at interest groups or at the public at large (the nation itself); the means or instruments used (sanctions, boycotts, etc.); the origins of isolation and its rate of change (creeping, galloping, or sudden); the extent to which the very existence of the state (and not merely a regime) is at issue; the identification of the primary inspirers of isolation, whether a sole state (e.g., China) or a grouping (the Arab states, black Africa); the duration of isolation and the process of its exacerbation or mitigation; and the tendency for isolated states to constitute an identifiable political type, namely, what others might define as pro-Western, right-wing authoritarianism.

Otherwise, the author devotes some attention to secondary case studies as precedents and comparisons to his focus group, or "gang of four." There is extensive coverage of Bolshevik Russia, Weimar Germany, fascist Italy, Francoist Spain, Portugal before the 1974 revolution, the People's Republic of China, the former German Democratic Republic, Castro's Cuba, Rhodesia, South Korea, and Stroessner's Paraguay. On the basis of these cases, Geldenhuys says that the phenomenon of enforced isolation is by no means confined to the small group of pro-Western states popularly depicted as pariahs or outcasts. Indeed, communist, as well as right-wing, regimes have been represented. He stresses that the cases reveal degrees of isolation. And indeed, some of these states, such as the People's Republic of China or the Soviet Union, were restored to international "respectability" on their own terms—presumably not so much on putatively moral grounds as because they are not weak and vulnerable, like a Rhodesia. Still, I am unconvinced that the modern pariah state phenomenon is something other than historically unique.

That uniqueness, indeed, seems strongly underscored in the author's exhaustive comparative analysis of Israel, South Africa, Taiwan, and Chile. In his summary, the author carefully compares the four cases, showing that they each involve distinct profiles of isolation. Taiwan is the most isolated on the diplomatic dimension. It and Israel have the most-questioned national legitimacy. South Africa and Israel rank first and second in economic isolation; South Africa is the most isolated in the military and sociocultural spheres, with Israel close behind socioculturally. But each has a specific combined profile. Taiwan, for instance, is extremely isolated in the diplomatic sphere, much less so in the military area, slightly in the sociocultural realm, and hardly at all in economic terms. Chile, meanwhile, is shown to have been the least isolated. Indeed, it is now following South Korea out of "pariahtude," having become more integrated with the surrounding world even at the end of the Pinochet era.

The author ends with a good summary of the need for further research, particularly as regards the process of the "rehabilitation" of isolated states. That need has become more timely since Taiwan's security problems have mellowed, apartheid seems on the road to being dismantled, Pinochet is gone, and even the prospects for an Arab–Israeli settlement may have improved a bit.

The book's overall excellence allows for only a few quibbles. I have some questions about the extent to which the author seems to accept too easily the (cynical) grounds for ostracism of the isolated states by the so-called international community. The author, a Boer South African, seems, in short, to bend too far backward in identifying with his own nation's detractors—at times to the point of self-abasement apparently intended to play to an external audience. There is some confusing of moral issues with the realities of power, leverage, and ideological conflict. For instance, he says that "for many years, Taiwan has displayed the typical pariah feature of authoritarian government which has prompted adverse reaction before" (p. 93), without pointing out that perhaps as many as a hundred of the nations represented in the United Nation's international community have worse human rights records than Taiwan—many far worse.

For the graduate student or young scholar, Geldenhuys' work on isolated states can serve as an excellent positive example of how to do serious, empirical, comparative international relations research and still make it eminently readable.

Pennsylvania State University ROBERT E. HARKAVY

The State and Policy Outcomes in Latin America. By Lawrence S. Graham. New York: Praeger, 1990. 206p. $39.95.

Ascent to Bankruptcy: Financing Social Security in Latin America. By Carmelo Mesa-Lago. Pittsburgh: University of Pittsburgh Press, 1989. 290p. $49.95.

The State, Corporatist Politics, and Educational Policy Making in Mexico. By Daniel A. Morales-Gomez and Carlos Alberto Torres. New York: Praeger, 1990. 197p. $39.95.

Development policy can be conceptualized as a set of choices policymakers make in attempting to achieve economic growth, distributive justice, bureaucratic efficiency, and political participation. Each of these components is necessary for sustained development, and each affects the other three. For example, policies designed to promote economic growth affect the distribution of income, different parts of the bureaucracy, and which groups are permitted (and not permitted) to mobilize.

Lawrence S. Graham combines an overview of public bureaucracy in Latin America with a series of country (Brazil, Mexico, and Nicaragua) and regional profiles. Graham is particularly concerned with identifying strat-

egies that can improve the effectiveness of development policies as they are formulated and implemented by national, regional, and local structures.

Carmelo Mesa-Lago's *Ascent to Bankruptcy* consists of the best available set of tables comparing social security statistics in all 20 Latin American countries and six case studies focusing on the evolution of social security policy in Costa Rica, Cuba, Chile, Mexico, Peru, and Uruguay. The title is based on the author's conclusion that the history of social security in Latin America is like a staircase leading to the abyss of financial collapse; that is, as the coverage of risks and population expands, so does the probability that the system will not be able to cover its costs.

Daniel A. Morales-Gomez and Carlos Alberto Torres analyze the conflicts and contradictions of educational policy planning in Mexico since the early 1970s. They examine the relationship among three elements: the politics of a corporatist state grafted onto the remains of a revolutionary political system, a revised model of associated-dependent development, and the process of policy formation in formal and nonformal education. The authors conclude, "The crisis faced by the Mexican State today is not primarily an economic crisis but a crisis of hegemony. It shows the failure of the dominant class to use the political, moral, and intellectual leadership that emerged with the Revolution to develop a new type of society" (p. 4).

Each of these books deals with the problem of bureaucracy in Latin America. By emphasizing the bureaucratic state-centered approach to development, most Latin American political systems have become dependent upon the rationality of their administrators in making public investment decisions, managing public enterprises, and providing a complex variety of social services. However, as Graham points out, the very success of many Latin American governments in having already created central bureaucracies now restricts further development: "Because their civil services have fulfilled essentially the roles of employer, guarantor of the status quo (insofar as the government in power is concerned), and defender of national autonomy, they have become an obstacle to further change over recent decades, especially when the imperatives of decentralization and autonomy—be they political or economic—are articulated" (p. xii).

The crucial point made by Graham is that despite all their attempts at political, economic, and administrative reforms over the past few decades, most Latin American governments have failed to enhance the quality of life significantly for large numbers of their citizens. In response to these failures, Graham claims that the most promising strategy is to explore the possibility of regional development as an effective compromise between centralized top–down and bottom–up (community-centered) reform efforts. He believes that mediating structures at the regional level (e.g., the state of Minas Gerais in Brazil) show the most promise for promoting democracy and mobilizing the necessary resources for local developmental needs.

Mesa-Lago is correctly concerned about administrative costs in implementing social security policy in Latin America. Whereas administrative expenditures in North America and Western Europe vary between 2% and 3% of total social security costs, in Latin America they are frequently above 10%. The author blames these high costs on "the persistence of a plethora of administrative agencies," contributing to inefficiency, inhibiting efforts at control and inspection, and facilitating evasion. He complains that because of political and/or union pressures, the social security agencies have often been used as employment agencies. Mesa-Lago also points out that in most of the Latin American countries, the rate of return on social security funds is only a fraction of the bank interest rates and that in many cases the real rate of return is negative. I wish the author had analyzed the role of corruption in the high administrative costs of social security.

The Morales-Gomez and Alberto Torres book deals with the political economy of education in Mexico. Education is often the most important and expensive distributive public policy, but it can also be viewed as an investment in human resources required for future economic growth. The 1917 Mexican Constitution committed the nation to the creation of a free, secular, and compulsory system of education, with little or no involvement from the private sector. By the early 1980s, the Mexican educational system had expanded to 24 million students and eight hundred thousand teachers. Yet despite this expansion, the Mexican educational system has been plagued by serious problems. In the early 1970s, for example, 30% of Mexican students were failing the first grade. By the mid-1980s, after great reform efforts, only about 50% of Mexican children were completing elementary school. The drop-out rates in rural areas continue to be high.

The authors argue that the success of the model of dependent economic development from the 1940s through the 1960s provided the Mexican political elite with the financial resources to support an expanding educational system. The political purpose of this policy was to legitimize a corporatist system that was retreating, in substance but not in rhetoric, from its revolutionary tradition. The authors claim that the fiscal and foreign debt crises of the 1970s and 1980s are forcing the political elite into new styles of technocratic thinking and experimentation in order to attain more legitimation from educational policy. In brief, they argue that the elite has tried to use educational reform as a substitute for more substantive distributive achievements. They predict that such efforts will no longer be successful.

These three books demonstrate the variety of developmental experiences in Latin America and suggest that no ironclad historical, cultural, or ideological determinism is at work; Alternatives are available. The dilemma is that so few developmental strategies can be created that are both politically feasible and economically effective.

University of Houston JOHN W. SLOAN

Authoritarian Power and State Formation in Ba'thist Syria: Army, Party, and Peasant. By Raymond A. Hinnebusch. Boulder: Westview, 1990. 350p. $54.50.

This book is about how the Syrian political system works. It is also an important effort to understand the political dynamics of a complex social order; and for this reason, the analysis has wide application to other authoritarian regimes. The author writes that the purpose of the book is "to examine the rise of the Ba'th Party, . . . to interpret the meaning of the authoritarian–populist state, and . . . to study the centrality of the linkages

between elite and society." The book covers these topics successfully. In addition, it raises important questions about the tension between traditionalism and modernity and between political participation and political legitimacy. As such, this book is an important addition to the comparative politics literature.

Whereas this book faithfully attempts to explain the relationship between the Syrian state and society in depth, it falls short of discussing the informal processes that embody much of what makes the stuff of Syrian politics so interesting, namely, the role of social networks and the nature of antiregime political socialization. The formation of a counterelite in the form of political opposition such as the Muslim Brethren has deeper sociopolitical underpinnings than the author has portrayed. More probing in this area is necessary if we are fully to understand the role of "muted" political opposition to the Asad regime. After all, this type of political behavior deprives the regime of its most prized goal, political continuity—especially after Asad. Finally, the book attempts to discuss the political economy of Syria but falls short in delving into economic issues and problems that are responsible for Syria's continuing economic ills—ills that contribute to political alienation and disaffection. Aside from these observations, the book is well researched and thought-provoking. It is a useful tool to the serious student of Syrian and Middle East politics.

The book is divided into 10 chapters, the first and last of which are attempts to wed theory and history which I found to be thought-provoking and rich in explanatory and predictive capacity. Parts of the book cover the historical evolution of the Ba'th party, while parts focus on Asad's leadership style, the bureaucracy, and the military. The section on the military is sparse and, in some ways, weak, even though military institutions are the centerpiece of authoritarian regimes in general and Syria is no exception. While outlining the role of the Syrian military institutions in a historical context, the author fails to explain the nature of the relationship between the civilian population as a whole and the military. This conceptual flaw warrants further criticism.

Whereas the Ba'th party under Asad has been somewhat successful in increasing political integration of the periphery (outlying rural provinces and provincial towns) with the center (Sunni urban areas), this effort has not gone unnoticed by the Sunni citydwellers (as Hinnebusch correctly states). The efficacy of this integration remains limited, however, both because the traditional urban population is vehemently opposed to it and, more importantly, because of the persistent patrimonial nature of the Asad regime, which opposes multiple centers of power.

The Ba'th party has also failed fully to integrate the existing social forces (ethnic, tribal, and religious) into the armed forces. Hence, the armed forces, who are the true guardians of the "authoritarian" regime in Syria, are not only outside civilian control but also isolated (from within) and vulnerable to political temptations or corruption. Although the party has attempted to widen political participation, most players are exiled from key decisions or from Asad's inner circle of confidants, particularly in the foreign policy realm. Only through a process of cooptation and coercion (not voluntary obedience or a contractual relationship of political obligation and consent) has Asad been able to build a precarious power structure that pits the military against the civilian population, the military against itself, and the civilians against each other. Such a system is politically fragile and likely to collapse during a crisis—such as the sudden death of Asad.

Hence, Asad continues to play a political role with boundaries and responsibilities defined by neither civilian nor military popular support. The result, of course, has been a persistent crisis of legitimacy, one that continuously puts the Asad regime on the defensive.

U.S. State Department KAMAL BEYOGHLOW

State Failure: The Impotence of Politics in Industrial Society. By Martin Janicke. University Park: Pennsylvania State University Press, 1990. 171p. $29.95.

After the Rights Revolution: Reconceiving the Regulatory State. By Cass R. Sunstein. Cambridge: Harvard University Press, 1990. 284p. $25.00.

The politics of industrial countries in the 1980s retreated from relying on the government to regulate or intervene in the economy. Many industrial countries privatized the production of public goods and services, deregulated price controls and entry restrictions, and initiated a reversal of social regulation in health, safety, and the environment. The political impetus for these policies derived in part from a slowdown in economic productivity combined with growing inflation and a continual increase in the size of public debt and annual deficits.

Both of these books take a strong stand against this political trend toward economic laissez-faire and present arguments in favor of regulation and government intervention to help prevent and ameliorate the problems of modern industrial society. Advanced industrial society, in the view of these authors, has a list of problems, including an unequal distribution of wealth and income, discrimination, unemployment, inflation, damage to the environment, ill health and death from toxic chemicals, poor quality products, and industrial accidents. Neither of these authors, however, advocates big government, expensive subsidies, or the kind of regulation that causes inefficiency in markets and exceedingly high costs for minimal gains. They depict the difficulties and irrationalities of past policies and regulatory practices and offer new approaches to regulation and government spending and taxing that could become the future intellectual grist for the ideological mills of liberal and social democratic parties.

While both authors overlap in their concerns for reshaping the role of government in bringing about a more humane industrial society, the coverage, methodology, and approach of the two books differ substantially. Sunstein's book deals exclusively with the historical development, legal theory, and implementation of social, health, safety, and environmental regulation in the United States. He presents a cogent set of arguments for why regulation is needed and why it has failed. In the second part of the book he develops a set of interpretative principles for the courts in the regulatory state and shows how the use of these principles would improve regulatory decision making in a number of different applications.

His recurrent theme is that the common law principles derived from the constitutional period are woefully

inadequate for the modern industrial state. For regulation to succeed, the courts must follow the reforms of the New Deal that introduced into the legal system "a sympathetic understanding of the functions of social and economic regulation" (p. 6). Under these reforms, citizens not only have political rights but also economic rights to health, safety, a job, and education. Sunstein seeks to reconcile and accommodate this twentieth century understanding of the state and public policy with the traditional legal concepts of constitutional government and private law. Sunstein's principles, in particular, are designed in priority order to 1) enhance political accountability and institutional deliberation, 2) protect constitutional rights for disadvantaged groups and protect against procedural unfairness and narrow interest group deals, and 3) to counteract statutory failure due to irrationality, unanticipated systemic effects, collective action problems, or other causes.

Janicke takes a much more sweeping and comparative approach to the question of regulatory and government failure. His thesis is a familiar one: successful and large producer groups, such as the construction, arms, finance, banking, and transportation industries, over time build more effective lobby relationships with the government administration than the average citizen is capable of building. Consequently, government panders to the needs of these groups through subsidies, tax abatements, weak regulation, and legal protections. This alliance between big business and big government creates a hierarchical and centralized power structure that stifles innovation, treats symptoms not causes, leads to economic stagnation and indecision, and drives out the courage to tackle the social, health and environmental needs of the society. He makes a useful comparative public policy contribution by applying these ideas in the areas of public health, the environment, transport, energy, and economic policies, and public budgeting and tax policies.

Since the basic inability of the society to pursue a humane industrial development is not a technical problem but a problem of power, according to Janicke, his solution proposes to break up the monopoly power of the industrial producers through strengthening countervailing powers of other groups and institutions in society. These actions to strengthen countervailing powers include reinvigorating cartel and antitrust policies, improving the legal position of citizens affected by industrial mishaps and costs, expanding political accountability to institutions in government that better represent more generalized interests not just producers, and decentralization to local units of government within a more effective central or federal coordination. Janicke proposes that citizens exploit economic and environmental crises as opportunities to build these countervailing powers in society.

Neither book's description and analysis of the causes and consequences of regulatory and state failure offer anything new. Sunstein's sections on the need for regulation and on regulatory failure, while covering no new ground, do serve as excellent and valuable reviews of this literature for courses in regulation, public policy, and public law. Sunstein also provides a highly valuable contribution to a multidisciplinary understanding of regulation by integrating his legal doctrine and philosophy into economic, rational choice, and political science literature on regulation and regulatory failure.

Janicke's explication of regulatory capture and collusive power between industry and the state neither offers a new explanation nor serves as a thorough and carefully presented review of the literature. Janicke leaves the reader confused on certain key points. While he maintains that producer groups dominate policy making, his own figures show that pensions and health constitute two of the greatest increases in state expenditures over the past decade. These areas serve a broad constituency of the elderly and the middle class not just, or even primarily, producer firms. Janicke also rails against the excessive military expenditures of the western industrialized countries, but in none of his figures are these expenditures shown to be very high relative to total public spending nor have they grown faster than other domestic spending. In addition, the book fails to explain how to exploit crises politically to overcome centralized power. This kind of inconsistency relates to a more basic problem: the book needs more carefully defined concepts and systematic empirical data on the areas under investigation.

The particular value of both of these books is not in their analysis of the problems but in their proposed solutions. Both books recommend ways for limiting the political strength of industry in regulation and government policy making. Sunstein's solution weaves an intricate carpet of ideas on legal interpretation that seeks to reconcile the constitution, private law, and the new social regulation. His ideas deserve attention from public law scholars, regulatory policy experts, practitioners, and political scientists. Janicke's section on solutions is both understated and underdeveloped. It is the place where he could make the greatest potential contribution to the literature but fails to do so beyond outlining what might be done. Yet his central idea that the solution to economic stagnation lies not in a technical approach but in new power relationships is critically important and deserves further elaboration and study by scholars interested in comparative public policy and political economy.

Michigan State University JACK H. KNOTT

Revolution: A Sociological Interpretation. By Michael S. Kimmel. Philadelphia: Temple University Press, 1990. 252p. $44.95 cloth, $15.95 paper.

Books that review theories of revolution, civil violence, and social movements continue to proliferate. Kimmel reviews over 30 theories in some detail and makes brief reference to at least a dozen more. James B. Rule's *Theories of Civil Violence* (1988) evaluated fewer theories but referred to a great many others for supporting evidence of one kind or another. Despite the different subject matter, about two-thirds of the theories in Kimmel's book are also covered, to one extent or another, in Rule's.

The sheer number of theories suggests that we know very little about revolution, rebellion, protest, social movements, civil or political violence, or other phenomena that are sometimes grouped under the same rubric and other times are not. If there are (as is probable) over 50 theories on the subject or subjects, there seems to be no way of knowing which, if any, is correct—or even has potential. Thus, books that evaluate what has been written about such phenomena are still useful. They help, if they are well done, in three ways: they provide

students with overviews of a field that would be impossible to cover in a course of normal length; they group theories into categories and thereby provide ideas about common assumptions underlying each group; and they evaluate theories and may suggest useful ways to proceed in subsequent research.

The first type of help is provided by both the Rule and the Kimmel books. While some may quibble about interpretations of a theorist or which theorists are included or excluded, I think it is fair to say that both books provide enough background on prior theoretical work to give students a basic overview of the field. The second point is more difficult to evaluate. Rule uses a fairly standard categorization of the literature, with some interesting exceptions (such as tracing the intellectual roots of rational choice theory to Hobbes). Kimmel's categorization is more controversial. He divides theories of revolution into structural and nonstructural ones and argues for the merits of structural theories. By *structural theories* he means those that locate the revolution in an international context and analyze the effect of class conflict (defined in more or less Marxist terms) and the state on the potential for revolution. His justification for this view, however, is largely an argument by assertion that does not satisfy his own criteria for the evaluation of revolutions (pp. 153–56, cf. 3–12). In particular, he offers no empirical or theoretical support for his views.

Kimmel's criteria for evaluation of theories are much broader than Rule's. Rule largely assesses the merits of a theory by its falsifiability—a somewhat simplified criterion, given the multiple steps involved in deducing hypotheses, measuring key variables, evaluating and analyzing data, and so on (p. 13). Kimmel maintains that theories have to be internally consistent, explain various aspects of a revolution, and make sense of empirical cases. The use of theoretical criteria for evaluation is helpful. The addition of stronger criteria for empirical validation would add to the merits of the book. As it stands, a reader may disagree with Rule's application of the falsifiability criterion, but at least it is clear how it is applied. In Kimmel's case, the criteria for evaluation are often loosely applied and the reader is sometimes (although by no means most of the time) left with the feeling that the evaluation is done by assertion.

Given differences of subject matter and criteria of evaluation, it is surprising and somewhat reassuring to learn that Kimmel and Rule come to the same conclusion, namely, that Tilly's work forms the best basis for theoretical development. Kimmel uses Tilly as a synthesis of previous theories of revolution (pp. 206–216). His theory is "an admirable achievement" (p. 216). Rule views Tilly as having made "enormous contributions" to an understanding of civil violence (p. 199). Other theories do not fare as well, although both Rule and Kimmel appreciate the contributions of a wide variety of theorists. Nevertheless, both authors suggest that Tilly's work is probably the most useful starting point for further investigations in the broadly defined field of either revolutions or civil violence. Recent work in the field of social movements has emphasized the importance of looking at the structure of political opportunities, a perspective that also derives from Tilly's work. Thus, it appears that for the present at least, there is more consensus than might be imagined, given the multiplicity of theories. Various types of protests, including revolutions, are to be understood as fights among groups (usually including the state), who mobilize resources and gain allies to secure things that will benefit them. We are left with an assumption of "collective rationality" that may or may not prove useful in gaining more understanding of what has been an intractable problem.

Washington University BARBARA SALERT

Social Change in Modern France: Toward a Cultural Anthropology of the Fifth Republic. By Henri Mendras and Alistair Cole. Cambridge: Cambridge University Press, 1991. 254p. $44.50 cloth, $14.95 paper.

Democratizing France: The Political and Administrative History of Decentralization. By Vivien A. Schmidt. Cambridge: Cambridge University Press, 1991. 406p. $54.50.

Given the titles, these two books, *Democratizing France* and *Social Change in Modern France*, would seem to lend themselves to a single, integrated review. However, that is no easy task. Schmidt's title is an all-too-typical exaggeration. Her book is not really about democratization but decentralization, which, while an important topic, leaves us far short of democratization writ large. Mendras and Cole, on the other hand, understate their topic with their title, since their book is an attempt to cast as broad a net as possible over France since 1945. Both books have a lot to offer to anyone interested in France.

Schmidt includes everything a reader would ever want to know about decentralization, much of which will be new even to those of us who have done work on what the Mitterrand government called *le grande affaire du septennat*. Hers is an encyclopedic approach not only to the reforms of the 1980s but of similar attempts over the last two centuries. Schmidt does an especially good job with the legal and administrative aspects of decentralization.

Mendras and Cole cast a wider net and have written a very different book, the gist of which is better conveyed by the title of the French edition, *La seconde révolution française*. While one may question whether there really has been a second revolution, the authors do a first-rate job of charting the sweeping social and economic changes of the last half-century as well as their equally sweeping political impact, including the decline of ideological divisions, political stability, and a deeply rooted consensus in support of the Fifth Republic. While there may be little that a French specialist has not seen before, the book is a treasure trove of insights into the way France has changed over the last half-century, ranging from statistical analyses to the comparison between nineteenth-century bourgeois dinners and contemporary barbecues to illustrate just how much things have changed.

The difference between the two books can best be seen in the way the authors deal with the one subject they treat in common, local politics. Schmidt treats it far more fully but does so without giving much of any consideration to the other aspects in French politics that help shape it or raising any of the broader issues that would be of interest to comparativists for whom decentralization in France is not a top priority. Those connections leap from almost every page of the less detailed but more provocative volume by Mendras and Cole.

Both books also have their problems. Schmidt really provides more than anyone who is not personally con-

ducting research on decentralization in France needs or wants to know. Moreover, her failure to address any of the larger issues will limit the impact of her brilliantly researched book, becuase of failing to address comparative scholars in general.

On the other hand, Mendras and Cole probably cast their net too widely. In part because Mendras, the primary author, is a sociologist, much of what they cover will not prove to be all that interesting to many political scientists. Moreover, many of their most sweeping generalizations could use a lot more empirical support, while the statistical material they do present tends to be drawn from sources that are too disparate to convince a skeptical reader, especially in their analysis of the declining importance of class.

Both books are first-rate examples of what modern scholars can do, albeit examples drawn from very different scholarly traditions. *Democratizing France* will be an invaluable resource to the handful of us who care about decentralization policy in France since 1789. *Social Change in Modern France* is nowhere near as comprehensive or encyclopedic. But in Mendras and Cole's broad treatment and in their tables (as well as their metaphors) there is some information. More importantly, there are intellectual challenges to anyone with even a passing interest in either France or sociopolitical change in advanced industrial societies. Both succeed in meeting the goals the authors set for themselves. Students of French and European politics should give each serious attention.

Colby College CHARLES HAUSS

China Misperceived: American Illusions and Chinese Reality. By Steven W. Mosher. New York: Basic Books, 1990. 260p. $19.95.

Mosher examines American perceptions and misperceptions of contemporary China. While following in the tradition of Harold Isaacs's classic *Scratches on Our Minds* (1958), he attempts to go further and "distinguish between truth and fiction." Unlike Isaacs, he eschews the visual media, focusing, instead, on the writings of scholars, journalists, and tourists and offering a partial and caustic view of the "China watcher" community, mainly since 1949. At the same time, Mosher recapitulates the national and international context that conditioned the familiar swings in American "love–hate" attitudes toward China.

Mosher briefly recapitulates the traditional conflicting images of China as laid out by Isaacs and the predominantly admiring views of American correspondents there prior to, and during, World War II. He usefully summarizes the fascinating retrospective conference of many of these journalists published in MacKinnon and Friesen's edited volume, *China Reporting* (1987). An excellent chapter on the "age of hostility" links the McCarthy hysteria over "who lost China" with Beijing's alliance with Moscow and entrance into the Korean War to explain the next two decades of largely negative writing. The sudden swing back to the admiring themes of World War II after the dramatic Kissinger–Nixon visits to Beijing in 1971–72 muffled the mayhem of Cultural Revolution violence, thanks to "an informal but effective coalition of admiring China-watchers and conservative realpolitikers" (p. 33). Then, post-Mao revelations with Deng Xiaoping's ascendancy in 1978 started a reverse mood of disillusionment, followed by a decade of "benevolence" in the belief that capitalism was replacing communism. The Beijing massacre of 4 June 1989 abruptly closed this period, introducing what Mosher sees as the end to American illusions with the blinding truth of Chinese reality.

Mosher offers a searing indictment of American journalists and scholars for their favorable but distorted depiction of Chinese reality; but he also has little praise and virtually no specific citations for those who had it right, unless they are from the Right or have subsequently acknowledged their earlier error. The late John K. Fairbank becomes a prototype of the academic who tailors analysis of China to fit a pattern suitable for influencing a particular audience in either country, depending on the writer's motive at the time. The case against Fairbank as a dispassionate historian and observer is closely argued, including a frank acknowledgment of partiality by the acknowledged dean of American sinology. To his credit, Mosher avoids invidious assessment of intent except when explicitly admitted (as by the wartime correspondents post hoc and Fairbank himself).

However, Mosher's selection of writings implicitly portrays the bulk of American sinologists as fools or knaves. He makes a good case with the quotes he offers but is not comprehensive either in his examination of a single author or in his coverage of the field. The absence of a bibliography underscores this selectivity. Casting a wider net would have captured an impressive number of rigorously objective and accurately analytical American scholars whose carefully balanced work has stood the test of time, despite the obvious obstacles for empirical research.

University of Arizona ALLEN S. WHITING

Education Reform in Japan: A Case of Immobilist Politics. By Leonard Schoppa. New York: Routledge, 1991. 319p. $59.95.

The central question of this book is, Why has the Japanese government failed in its efforts to reform education? The question is answered theoretically, by first establishing a modified Pempel–Campbell model of conflict resolution pattern in Japanese policymaking. Within this framework, the author hypothesizes that

> (1) low-conflict disputes—some combination of high divisibility, wide scope and low affect—tend to result in incremental change or . . . "contrived consensus" type decision-making; (2) high-conflict disputes involving *outside forces* result in camp conflict and either (a) forced resolution by the government or (b) opposition success in convincing some conservatives to back down, [resulting in inaction]; (3) high-conflict disputes involving primarily *conservative camp actors* result in conflict avoidance and little change." (p. 20)

The essence of the policymaking process in Japan is to get "consensus *within* the conservative camp while not requiring the approval of non-conservative forces" (ibid.).

The book's organization is tied closely to the Pempel–Campbell model, for which the author provides the theoretical background in his opening chapter. He goes on to identify the major conflicts in the current battle over education reform and then treats divisions within the conservative camp in the ruling Liberal Democratic party (LDP), the bureaucracy, and special interest

groups. After discussing the role of the progressive opposition, particularly the Japan Teachers Union, he tests his model by examining the education reform movement in the 1970s and in the Nakasone era.

The author concludes that the education issue has been characterized by several patterns of conflict and that the resultant policy decisions could be predicted by the modified model. For example, the educational expansion program of the 1970s represents Pempel–Campbell type 1, the Shoninsha Kenshu program under Nakasone is typical of type 2a, and the proposal for unifying nursery school and kindergartens is type 3. The author argues that the long-standing dominance of the LDP has led to an internal diversity that has obstructed political change. The paralysis in the policymaking process usually occurs when rifts develop between bureaucrats, party leaders, and politicians specializing in education; but it afflicts other areas of Japanese life beside education, as well.

The author's conceptual framework is original and is presented in a logical, straightforward, and remarkably lucid manner. The data with which he supports his views in each chapter are informative, well balanced, and recent; and his sources of information are useful and usually reliable. Figures and tables, a glossary of Japanese terms, and references are not only accurate but conveniently placed.

The author's model of conflict resolution patterns may be challenged by the *household* (*ie* or *uchi*) model advocated by many Japanese scholars, including Chie Nakane, Seisaburo Sato, and Eiichi Kato. It is impossible fully to explain the household concept here, but clearly it has a vastly different conceptual foundation. To the Japanese, *ie* is a corporate residential group formed on the basis of an established frame of reference—a managerial group, whether of a family, a business corporation, or a community. According to Nakane, the household concept, which puts great emphasis on relationships, trust, personality, and group unity, actually restrains the strong-willed individual. The leader, who usually exerts his power indirectly through his immediate subordinates, must have their consent to adopt a new policy or reform an old one. The process of consensus building (*nemawashi*), in Kato's words, is perhaps more important than formal legislative debate. Advocates of the household model may feel that Schoppa has put too much emphasis on issues and conflict, and too little on personalities and consensus building.

American scholars, meanwhile, may object to the lack of clarity of such concepts as *high-conflict dispute* and *conservative camp*. Those familiar with Herbert Simon's criticism of Luther Gulick's four principles of administrative organization (purpose, process, clientele, and location) on the ground of imprecise definitions of terminology may find that the same criticism applies here.

Nevertheless, the merits far outweigh the faults, and students of Japanese affairs are fortunate to have it. One of its strongest points is that the author has discovered an Achilles' heel in Japanese politics—the Japanese cannot make national policy decisions when there are strong division between bureaucrats, LDP leaders, and legislative specialists. This provocative finding will challenge the belief—increasingly widely held in the West, as Japan's economic power has grown—that the Japanese invariably make the right decision at the right time. Whether his other findings will be borne out must still be seen.

Gannon University PAUL S. KIM

Liberty and Community: Canadian Federalism and the Failure of the Constitution. By Robert C. Vipond. Albany: State University of New York Press, 1991. 349p. $14.95 paper.

There has been a spate of books on Canadian federalism in recent years, as there has been on American federalism. Both systems have felt a variety of cross currents as the role and the power of the central government shift and the subnational governments seek appropriate responses. Most of the recent books on Canadian federalism have focused on the impact of the Constitution Act of 1982 and the reforms it introduced into intergovernmental relations in Canada. None of the recent analyses have found a solution to the long-maintained interlevel struggle. Few of them are based on historical examinations of Canadian political thinking.

What Vipond argues in this carefully reasoned book is that the "constitutional debate has . . . center[ed] on two 'competing conceptions' . . . of what the country is or ought to be; conceptions which [have] helped to polarize political debate"—namely, the *liberty* and *community* of the book's title, meaning the liberty of individuals versus the several communities' interest in "collective choice" (p. 2). In other words, it pits the central government as defender of individual rights against the provinces as guardians and proponents of regional community differences and desires. But the evolution of Canadian federalism, Vipond argues, need not be based on that dichotomy; and therein lies the original thesis of his work, which is basically a historical review of the early federal debate in Canada.

Although Vipond recognizes that the tension between liberty and community in Quebec has received the most attention by scholars and practitioners alike, he chooses to study in depth the Provincial Rights movement in Ontario from confederation in 1867 until roughly 1990. He does so because in that movement he finds that the proponents of provincial rights rejected the incompatibility of liberty and community and argued, instead, that the provinces—the separate communities of Canada—were also capable of protecting individual liberties. His arguments are convincing.

His approach is clearly laid before the reader in his introductory chapter. As he proceeds, he seeks to lay the basis for a shift in the future thinking about Canadian federalism from the either/or stalemate of the last quarter-century, which has caused the Canadian polity to lurch "from one constitutional crisis to another" (p. 1), to a recognition that both concepts may exist together within the broad historic contours of the Canadian view of liberalism. He does not dot every *i* and cross every *t* in the debate over provincial autonomy in Ontario during the chosen time period but concentrates on what he considers to be "pivotal episodes" in that long debate (p. 13). In successive chapters, he looks at the concept of sovereignty in Canadian thought before 1867; the British North America Act's provision for a centrally appointed lieutenant governor of each province and consequent

impact (or lack thereof) on provincial autonomy and self-government; the lessons taught to Canadians by the broader concept of imperial home rule as clues to the federal relationship in Canada; and the deep faith of debaters on both sides in the rule of law as the fundamental basis of both liberty and community.

In his well-argued conclusion, Vipond demonstrates that in the Ontario debates proponents of provincial rights (of the provinces as independent political communities) based their defense "on a deep commitment to liberal individualism" and an equally deep commitment to the view that the people were "the source of, and the ultimate check on, governmental authority," wherever it might be exercised! "The ideology of provincial autonomy, in short, was both considerably more ambiguous and considerably more subtle" than it came to be seen in later years (pp. 194–95). Vipond begs his readers "to see beyond, or see through, the 'competing visions' that animate, but also divide most constitutional discussions" in Canada today and to understand that underlying both concepts is the "common commitment to liberalism" of all Canadians (p. 196). In his view, the principles of liberalism are sufficient today (as they have always been) to allow a resolution of the Canadian constitutional crisis. That is what he sees as the challenge to Canadian federalism and perhaps its ultimate solution.

Some 45 pages of chapter notes give evidence of Vipond's careful and thorough research and understanding of the complexities of his theme; the index is unusually well constructed. It is a most thought-provoking and constructive book.

Duke University RICHARD H. LEACH

The Child and the State in India: Child Labor and Education Policy in Comparative Perspective. By Myron Weiner. Princeton: Princeton University Press, 1991. 213p. $45.00 cloth. $14.95 paper.

The core proposition of Myron Weiner's study of child labor is that India's low per capita income and economic situation is less relevant as an explanation for the high numbers of children in the labor force than the belief systems of the state bureaucracy (p. 5). "It is the absence of strong support for governmental intervention within the state apparatus itself and the absence of a political coalition outside the state apparatus pressing for governmental intervention that explains Indian policy (p. 15)." Weiner's confirmation of the proposition leads him to the "pessimistic conclusion that barring a conceptual change in the thinking of those who make and implement policy and a new direction in policy by the Indian government, the number of children in the labor force will not significantly decline . . . (p. 207)."

The discovery that the bureaucrats show little interest tells us not that the situation is hopeless, but rather where campaigns for change need to be undertaken. The other countries Weiner studied also went through phases in which their bureaucrats were as disinterested. Many countries allocate insufficient funds to education, because they are poor or they hold education in low priority or both. Many countries spend a good deal on education but favor the middle and upper classes. Weiner provides a useful history of attempts to implement compulsory education in India and, for comparison, in advanced industrial countries. He might have also discussed the complementary idea of a right to education, under which governments could be called to account if adequate educational services were not provided. As a result of a decade of negotiations, a new Convention on the Rights of the Child came into force in September of 1990. The convention devotes several articles to labor and education. Remarkably Weiner gives no attention to the idea of children's rights. He discusses the rights of parents and the rights and obligations of the state but does not explore the idea of children's rights. India has not signed the convention, but the developing consciousness of childrens rights worldwide will nevertheless have an impact on India.

Weiner repeatedly recognizes that Indian schools are not good at building human capital and thus are not attractive to the poor: ". . . the Indian educational system is defective because it does not prepare village children and the urban poor for living and working in their own environment (p. 187)." Education generally is not compulsory and most schools are not good enough to attract voluntary attendance. The solution does not lie in simply passing more laws limiting child labor or requiring school attendance, both of which reduce the families options, but rather in improving the quality of the schooling opportunities available to children. In the 1930s Gandhi, in his journal Harijan, argued that:

> As a nation we are so backward in education that we cannot hope to fulfill our obligations in this respect in a given time if this programme is to depend on money. I have, therefore, made bold, even at the risk of losing reputation for constructive policy, to suggest that education should be self-supporting . . . I would, therefore, begin the child's education by teaching it a useful handicraft, enabling it to produce from the moment it begins its training. Thus, every school can be made self-supporting, condition being that the State takes over the manufacture of these schools (p 105)."

Gandhi's scheme for paying for education by marketing items produced in the schools was not implemented, possibly because of resistance from private manufacturers of similar items.

The fundamental dilemma of child labor is that parents feel they cannot forgo the current earnings of the children in order to invest in building their "human capital" and thus enhance family income at a later time. A well designed loan is precisely the instrument needed to transform the anticipated future earnings into present benefits, sufficient to pay tuition and to compensate for forgone short-term income. Perhaps private vocational schools could be created, with curricula designed to build the skills that would enhance long-term earning capacity in the local setting.

Through the Grameen bank experiment in Bangladesh, micro-enterprise loans through the United States Agency for International Development, the Accion International organization, and many other programs, it is now being recognized that novel loan arrangements have remarkable potential as instruments of development. India, already familiar with the institution of bonded labor, should have no difficulty with the concept of long-term loans designed to enhance the individual's earning capacity.

India's massive child labor problem cannot be addressed simply by passing new laws. There is a serious need for strategic planning and for creative institutional design. Weiner's study advances our understanding of

the problem, but there is much more work to be done to move toward meaningful remedies.

University of Hawaii GEORGE KENT

Transforming Settler States: Communal Conflict and Internal Security in Northern Ireland and Zimbabwe. By Ronald Weitzer. Berkeley: University of California Press, 1990. 292p. $29.95.

How can ethnically divided societies become genuine democracies after decades of conflict? Weitzer marries a Weberian conception of the state to the new institutional literature and then compares the evolution of the security services in Rhodesia/Zimbabwe and Northern Ireland in search of an answer to this question. Weitzer argues that a necessary-but-not-sufficient condition for a successful transition is that their internal security bureaucracies can be professionalized and rendered "ethnically" neutral to a significant degree. Weitzer sees the security apparatus as the core of the settler state, which by its very nature rests on physical coercion of an indigenous population. Only a security apparatus subordinated both to law and to the civilian parts of the state can generate enough legitimacy among the population (particularly formerly oppressed ethnic groups) to allow consent to prevail over coercion as the major determinant of people's behavior. To the extent that the new security services do not approach this Weberian ideal type of a neutral, rule-governed bureaucracy, they either can be turned against some new population or will not be able to generate legitimacy and consent from the population. Weitzer's argument thus addresses issues familiar to those studying democratization in Latin America and Eastern Europe, as well as more traditional students of state building in decolonizing areas.

In both of Weitzer's cases, genuine democracy remains a distant goal. In Zimbabwe, the Zimbabwe African National Union (ZANU) under Robert Mugabe simply appropriated the old security apparatus wholesale, replaced whites with Shona tribesmen loyal to ZANU, and then used the security system to destroy the rival Zimbabwe African People's Union (ZAPU), which was associated with the minority Ndebele tribe. Mugabe's party merely changed the ethnic composition of an ethnically dominated state. In Northern Ireland, the collapse of the (Protestant) settler state's control over Protestant militants and hegemony over the minority Catholic community led the British state to usurp the Protestant state's security functions. In this situation, both communities ended up distrusting the security services. This made any transition to a locally controlled state security service impossible, even though the security apparatus was partially liberalized and bureaucratized.

The strength of this book lies in its discussion of the legal and institutional trajectories of the security apparatus in these two countries. Weitzer nicely situates this discussion in the origins of the collapse of the two settler states, showing how the security services won an increasing but counterproductive autonomy from civilian control. But this discussion reveals the implausibility of successful and peaceful liberalization of the security services.

First, Weitzer argues that the security systems' personnel themselves increase ethnic discord by eliminating legitimate channels for dissent and militarizing the state's responses to extralegal ones. He argues that a dialectic of repression and resistance soon transforms the relationship between the security services and the state. As resistance increases, the civilian (but settler-dominated) parts of the state grant extraconstitutional powers to the security services and waive any efforts at oversight. Precisely this phenomenon suggests the impossibility of reforming the security apparatus. As these services generate newer and harsher responses to indigenous resistance groups, they become increasingly *debureaucratized*. Standard operating procedure gives way to a desperate search for spectacular one-shot "solutions" engineered by highly trained and essentially lawless commando forces like the Selous Scouts and the Special Air Service. Indeed, as the Birmingham Six trial showed, this lawlessness can metastasize back into the metropolitan state itself.

Second, in settler states with a low ratio of settlers to indigenes, the settler population in its entirety often *is* the state and (particularly through obligations to serve in the reserves) the military. This was certainly true in Rhodesia (5% European), and almost true in South Africa, where Afrikaners are primarily employees of the state and state-owned corporations. "Decommunalizing" the security services here implies a wholesale attack on one community's employment, which that community can be expected to resist. Weitzer's discussion of the transition from Rhodesia to Zimbabwe shows just how unusual and *accidental* the preservation of the white-dominated security services during the transition was. Because all the participants anticipated a victory in the 1980 election, they all tried to preserve the security services they assumed they would inherit. A more cautious approach to the negotiations would have dictated a preemptive "defanging" of those services, particularly by the minority whites and ZAPU.

Despite these flaws, Weitzer helps focus attention on a crucial internal determinant for successful liberalization and democratization. His analysis is particularly useful to students of South Africa, where the security services' role in fomenting tribal conflict and its connections to different political parties make clear the costs of having an uncontrolled state-within-a-state. It will also be useful to students of the (former ?) Soviet Union, where Russian "settlers" are a potent political and demographic force in many of the new republics.

University of Virginia, Charlottesville HERMAN M. SCHWARTZ

INTERNATIONAL RELATIONS

ASEAN and the Diplomacy of Accommodation. By Michael Antolik. Armonk, NY: Sharpe, 1990. 208p. $39.95.

The Association of Southeast Asian Nations (ASEAN) is one of the least known regional organizations and is subject to considerable misinterpretation as well as general neglect. This book by Michael Antolik is both a history of its origins and an explanation of the members' perspectives on how the ASEAN serves each of them. As such, it is a welcome addition to the literature of both Southeast Asia and international organization. It is particularly useful in filling the "non-Vietnam" gap that has existed for Southeast Asia since the early 1960s. Since that time, attention has been so focused elsewhere that Malaysia is sometimes thought to be the entire archipelago and ASEAN is often thought to be a misspelling of *Asian*.

In addition to the important task of bringing the details of the ASEAN and its members' roles to our attention, Antolik's book makes a contribution by examining the purposes that the ASEAN serves, rather than its structures. He notes that these purposes have evolved. Early on it was to serve as "a face-saving and noncontentious instrument of war-termination diplomacy," with immediate successful application in the Confrontation period. The ASEAN has gone on to be a forum, in which its six members (Malaysia, Thailand, Indonesia, Brunei, the Philippines, and Singapore) have ready access to each other for discussions of bilateral problems. It has facilitated common stands on important regional and global issues so that the six can synergistically generate more power and speak with a single voice. The regular contact among the members has brought about mutual understanding and the discovery of many commonalities.

Antolik correctly points out that there has been little of the economic integration that was so highly touted at the origins of this and many other regional organizations. He suggests, however, that political cooperation has far exceeded what was expected in 1967, and he indirectly documents the bilateral military cooperation that might well not have occurred without the auspices of the ASEAN environment.

His argument that "progress toward cultural cooperation has been more dramatic" than that toward economic integration needs to be subjected to further research, however. The exciting technological progress of ASEAN members has kept them in contact with the rest of the world; and the same influences that are reflected in Yugoslavia's divisions, the Soviet Union's continuing disintegration, and the disarray in the Middle East are present in Southeast Asia. Cultural cooperation between Malay Muslims and the region's Chinese has not improved in any documentable way. Also, internal cultural divergences have reverberations in the bilateral relations of the member countries that do not suggest increased cultural cooperation. In fact, the situation looks much like it has looked for many centuries. What the ASEAN has done is provide a forum for alleviating the potential political and military problems that arise out of continued cultural cleavages. The cleavages, however, are still there and may even be growing.

Antolik's treatment of the colonial period's contribution to the ASEAN's origins and mission reflects the ambivalence that the member nations themselves often display. He seems to want to distinguish between domestic and regional circumstances in advancing the positions that colonialism is the source of regional disruption and that significant divisions existed prior to colonial times, while not fully recognizing that *domestic* and *regional* are not dichotomous circumstances in this region. Singapore was once domestic Malaysia, for example, and Indonesia was a region with many domestic parts. If the ASEAN had focused on resolving problems that are presumably a function of the colonial period, it would have had little of the success that the author rightly points out it has.

The strength of Antolik's book is that it draws attention to the communication and other functional successes of ASEAN, pointedly downplaying the integrationist and economics criteria that often characterize such works. His descriptions of both domestic and bilateral contributions of the ASEAN forum are particularly noteworthy and important. His histories of the circumstances under which each ASEAN member found solace or utility in the community are also interesting portrayals of the linkage between nation-building processes and the nature of the international environment. The reader will be well educated about the development of both the ASEAN and its component states. The book's only real weakness is in the author's good-willed intent to paint a positive picture of the future of economic and cultural cooperation in the region (p. 156). Fortunately, he has gone into great detail to explain how ASEAN serves its members well, specifically in controlling economic competition and deterring cultural disintegration. If we compare the ASEAN with other similar organizations, these low-profile but critical successes already demonstrate the worthiness of the association. We need not place on the ASEAN the burden of altering resource distributions and centuries-old patterns of social and political identity.

American Graduate School of International Management LLEWELLYN D. HOWELL

Investment Behavior of Multinational Corporations in Developing Areas: Comparing the Development Assistance Committee, Japanese, and American Corporations. By Bret L. Billet. New Brunswick: Transaction, 1991. 161p. $32.95 cloth, $24.95 paper.

Multinational corporations have been the topic of numerous books and articles over the last several decades. Bret Billet takes an innovative approach by addressing the question "How do multinational corporations react to the environment that they find in less developed countries?" (p 15). In answering this question, he argues that (1) various economic and political factors help determine whether foreign companies invest in poor countries; (2) foreign investment behavior

often is different for Development Assistance Committee (DAC), Japanese, and American multinational corporations; and (3) foreign investment behavior frequently varies by geographic region of the world, that is, different corporations have a greater propensity to invest in some areas than in others.

Although this book contains many good ideas, it relies heavily on a flawed quantitative analysis that diminishes its overall contribution. Because Billet stresses his empirical analysis, I will discuss four serious problems with it. First, contrary to his claim (p. 21), the statistical analysis is *not* really based on a lagged dependent variable model. This type of model should include a lagged dependent variable as an *independent* variable, thereby controlling for the prior effect of the endogenous variable. Although Billet measures his dependent variables at different time points, he does not include a lagged dependent variable in the right-hand side of the equation. This reduces the causal explanatory power of his model, a point which has been stressed in previous development research.

Second, Billet supposedly uses dummy variables to analyze regional differences in foreign investment behavior. He claims to examine subsamples of Latin America, Asia, and Africa by including dummy variables for region (p. 59, n. 53). Throughout the book, however, no equation *reports* a dummy variable coefficient for *region of the world*, confusing readers and forcing them to wonder what is happening. There are three possibilities. Billet may have restricted his sample to specific regions when conducting the regional analyses. This is not possible, however, because the sample size is still close to 90 for *each* regional analysis! Or Billet may have included an intercept dummy variable in the analysis but failed to report it, which would be incorrect methodologically. Even if reported, an intercept dummy variable does not measure his primary concern, namely, the potentially different effects of *each* independent variable in Latin America, Asia, and Africa. This can be tested by calculating a series of *slope* dummy variables—dummy intercept variable for each region times each independent variable of interest. These interaction terms *must* be reported in the relevant equations. Finally, Billet may have attempted some other procedure that is not clear from his discussion and tables.

Third, case selection is problematic throughout the analysis and warrants discussion by the author. Specifically, the multiple regression equations examining the determinants of DAC and American corporate investment generally are based on 80–90 cases (countries). Importantly, however, the regression equations assessing the determinants of Japanese investment are based on 38 or fewer cases because "the Ministry of Finance reported foreign direct investment figures for a limited number of less-developed countries" (p. 58, n. 49). This is problematic, because the book is based on a comparison of DAC, American, and Japanese investment, thereby requiring a common sample in order to draw reliable statistical comparisons. Results produced by a small subsample of countries that have Japanese investment data should not be generalized to the larger sample, which is used to assess DAC and American investment. At the very least, the analysis should contain some *additional* equations that utilize the subsample to explore determinants of DAC and American investment.

Finally, the indicators tested in each chapter explain very little variation in the dependent variables. By my count, this book contains 176 multiple regression equations, 119 of which have an adjusted R-squared value of .10 or less (often much less). Although the R-squared value is overemphasized by many social scientists, it still tells us something about the explanatory power of an equation. When the value is consistently near zero, it should compel the author to examine additional variables or explain the situation. For example, although Billet posits that political repression, human rights, and other factors lead to foreign investment, it is possible that the reverse ordering is true. Previous research has demonstrated that foreign investment may well contribute to a climate of political repression, suggesting that political repression should be the *dependent* variable in this instance.

In conclusion, this book contains many good ideas. It asks important questions and frames them in an interesting analytical and theoretical framework. Unfortunately, however, the book also stresses the statistical testing of its arguments, and these results are not convincing because of various methodological shortcomings. This is a serious problem because we cannot trust the findings "uncovered" in the data analysis, an analysis that would carry great promise if properly executed.

Indiana University, Bloomington YORK W. BRADSHAW

American Hegemony and World Oil: The Industry, the State System, and the World Economy. By Simon Bromley. University Park: Pennsylvania State University Press, 1991. 316p. $39.50.

The age of cheap, clean, abundant energy supplies is over. Domestic oil production has been on a decline since 1970. The nuclear power industry is nearly at a standstill. New technologies in coal and other fossil fuels promise higher prices, delayed deliveries, and a set of politically sensitive questions about externality costs and environmental risks. Yet the political recognition of these trends lags significantly behind.

Simon Bromley's new book explores this dissonance inadvertently and at times brilliantly. His main claim is that American control of oil still exists. Yet his own meticulous reconstruction of history shows that in 1950 the United States produced 40% of world output and 27.3% of world exports, in 1980 only 25% and 17% (p. 72). Now if *hegemony* means controlling 40% or more of world economic activity, S. P. Huntington and others have noted that American hegemony disappeared long ago. Once we acknowledge this, Bromley's book might be better titled *The American Insistence for World Oil: 1900–1990*.

In fact, it is when Bromley discusses oil as a strategic commodity, rather than as a physical supply, that his book proves most useful. The book documents the shift of leadership during the world wars in the Middle East from British to American; explores America's strategic involvements with OPEC in the 1970s; and defines our allies and rivals in Europe, Japan, and the Soviet Union along a petrochemical yardstick. Bromley reveals that what really matters is "the complex conjunction of trends by which U.S. control over world oil became a key resource in the overall management of its global leadership" (p. 86). His book, particularly the middle chapters, examines these trends with insight and a wealth of details.

Missing from this book is a sense of consequence—an

awareness of what this strategic insistence on oil costs. Instead, Bromley states with a strange abandon, "Within the general global energy balance, oil remains the regulator of the world energy markets due to its versatility, convenience, and even cost" (p. 216). In his sixth and last chapter, he discusses America's "reconsolidation" of world oil in the 1980s as if the Exxon Valdez spill, the international meetings on global warming, and the long foreground of stresses that led to our most recent war with Iraq were mere sideshows, curious blips in a prerecorded message meant to repeat itself without question or interruption until worn smooth by reuse.

Other titles go much further in describing the costs of this strategic insistence. Three of the best are John Blair's *Control of Oil* (1976), Daniel Yergin's *Energy Futures* (1980), and Martin Melosi's broad energy history of the United States, *Coping with Abundance* (1985). According to present mineral reserve lifetime figures of the U.S. government, if current rates of consumption and reserves continue, the United States will run out of oil in 9.9 years, while the world supply will be depleted in 45.3 years. These are the indicators that guide the above titles, not simply the continued dominance of a worldview and its politics.

Nonetheless, the story Bromley reconstructs is an important one, full of astonishing facts and political anecdotes. It should be read by all Americans who already know that 80% of the oil wells drilled in the world have been drilled in the United States and those intrigued by the political consequences of having these three million domestic wells slowly run dry.

Rensselaer Polytechnic Institute BRUCE PIASECKI

China's Changing Relations with the Middle East. By John Calabrese. New York: Pinter, 1991. 183p. $49.00.

In several respects, John Calabrese has written an ambitious and useful book. China's attitudes and policy toward the Middle East remain one of the least explored subject areas in the literature of Chinese foreign relations. This book and Yitzhak Shichor's *Middle East in China's Foreign Policy* (1979) fill this gap in our understanding of China's changing relations with the Middle East.

The main objective of the book is "to map the changes and continuities in China's relations with the Middle East: to periodise the forty-year history of Sino–Middle Eastern relations" (p. 2). This exploration takes a chronological narrative format, describing China's policy toward the Middle East as comprising seven distinct developmental phases represented by seven chapters: "Leaning to One Side," "The Cost of Militancy," "Social Fortress," "Prelude to Pragmatism," "Thwarting Superpower Hegemony," "Leaning to the Other Side," and "Developing Influence and Influencing Development" (phases beginning in 1950, 1958, 1966, 1969, 1972, 1978, and 1982, respectively).

The author sees China's Middle East policy as reflecting the changes and shifts in Chinese foreign policy as a whole. Calabrese is at his best when providing an account of China's changing perceptions of the Middle East, from the theater of Sino–Soviet collaboration in the 1950s and the arena of Sino–Soviet rivalry in the 1960s, to the vortex of superpower contention and collusion in much of the 1970s, and an integral and increasingly important part of the globalization of the open door policy in the post-Mao era. He is less illuminating in depicting the continuities of Chinese foreign policy. Throughout the book, he repeatedly recalls the realist premise—that China has consistently interpreted Middle East developments in terms of the structure of global politics dominated by the superpowers—as accounting for the continuity of Chinese foreign policy. But this realist premise is not applied rigorously or systematically enough to add anything significant to the narrative.

Essentially, the book thus *assumes* the realist perspective. Beijing's Middle East policy over the years is said to have consistently followed a "strategic logic" defined in global, rather than regional, terms. Hence, Calabrese tacitly embraces the conventional wisdom in the study of Chinese foreign policy that China is a regional power without a regional policy. Much of the argument seems to echo Schichor's central thesis that China's Middle East policy can be best understood as a reaction to global events dominated by the superpowers, rather than to indigenous conflicts in the region. He uncritically accepts another realist premise—the state-centric rational actor model—as he asserts that Chinese foreign policy in general and Middle East policy in particular have enjoyed an elite consensus within the Chinese leadership.

The book provides a balanced and thoughtful examination of China's changing perceptions of, and relations with, Egypt, Iran, Iraq, Syria, Saudi Arabia, Kuwait, Israel, and the PLO. It is particularly impressive in providing fascinating background briefing on China's relations with Israel and the PLO.

Yet *China's Changing Relations with the Middle East* strikes me as disappointing on several counts. First, there is the problem of timing and coverage. Since the book is designed to "update Shichor's analysis" and "reconsider the question of China's role in the Middle East in the light of the decade that has elapsed since its publication" (p. 4), there is no compelling logic or justification in rehashing the entire forty-year history of Sino–Middle East relations, devoting only the last two chapters of a slim volume to the missing decade. It seems inexcusable that the author could not wait to take sufficient account of the post-Tiananmen developments, including China's involvement in the 1990–91 Gulf crisis, the most serious litmus test of Chinese policy toward this trouble spot. As a result, a book published in May 1991 was already made-to-order for historical anachronism, being replete with references to the "Gulf War" (meaning the 1980–88 Iran–Iraq War). The author himself (presumably unwittingly) committed this book to swim or sink based on its coverage of China's Middle East policy during the post-Mao era. The establishment of full diplomatic relations between China and Saudi Arabia in 1990 is another post-Tiananmen event that has not only rendered much of the analysis of China's relations with Saudi Arabia stale but also invalidated what little prognostication the book embodied. Second, the book is marred by an excessively sympathetic "Nixon-in-China" strategic realism. There is little probing of the theoretical and normative implications of the erratic twists and turns in China's Third World policy. Relying mostly on secondary sources and official pronouncements, Calabrese too readily accepts the rhetoric of independence at its face value, papering over the words/deeds—and even words/words—disjuncture between political independence and economic interdependence in the 1980s. Calabrese too readily accepts China's

rhetorical identification with the Third World, failing to see and analyze the steady decay, since 1978, of China's moral regime and credibility as an antihegemonic champion of the Third World. As if to bridge the widening gap between policy pronouncements and policy performance, Beijing began to rationalize its decline as a Third World leader as proof that it has kept its pledge never to act like a hegemon.

Finally, the realist perspective seems to have led Calabrese to commit the fallacy of overstating external systemic factors and understating domestic factors in the making and execution of Chinese foreign policy. The notion that China's Middle East policy (especially arms sale policy) commands a consensus among the Chinese leadership is wide of the mark. The chronic gap between promise and performance on a range of global issues, most notably on arms sales and transfers of sensitive nuclear and missile technology to the Middle East—largely left undetected and unexplored in the book—is not entirely due to mendacity (although this, too, is a problem). Because of the rise of multiple power centers within the Chinese government in the 1980s, the Ministry of Foreign Affairs finds itself making international pledges and commitments that it has no power or authority to implement.

Princeton University SAMUEL S. KIM

The Dynamics of Domestic Politics and Arms Control: The SALT II Treaty Ratification Debate. By Dan Caldwell. Columbia: University of South Carolina Press, 1991. 234p. $32.95.

The weight of domestic politics on American foreign policy comes under detailed scrutiny in this case study of the nonratification (after seven years of negotiation) of the U.S.–Soviet Strategic Arms Agreement (SALT II) in the waning months of the Carter administration. Caldwell examines Carter administration approaches to foreign policy and arms control; its SALT II ratification strategy; the roles of major domestic actors (public opinion, interest groups, and the Senate); and the impact of external events (the Soviet-brigade-in-Cuba contretemps, the hostages in Iran, and the Soviet invasion of Afghanistan).

He deals rather thoroughly with two major explanations of nonratification (Carter administration shortcomings and Soviet bad behavior), less thoroughly with the substantive security merits of the treaty. Proposals for fundamental reform of American institutions by the losing treaty advocates are noted: a parliamentary, rather than presidential, system; a six-year, one-term presidency; and ratification by a majority of both houses of the Congress, rather than two-thirds of the Senate. In sum, both domestic politics and external events matter for arms control agreement ratification. Presidents need to pay much greater and much earlier attention to ratification strategies.

According to Caldwell, six lessons are derived from his case study. First, presidential failure to damp—indeed, almost to directly guarantee—lack of foreign policy consensus among key officials has adverse consequences. Major policy steps have strong opponents at all times in the administration itself. They build blocking coalitions with the Hill and the media. Potentially decisive institutions (in this case, the CIA or Department of Defense) cannot be used decisively to break deadlocks between the national security advisor and the secretary of state. More generally, mixed signals result that involve concessions to committed opponents of particular initiatives and slights to undecided and middle-of-the-road actors.

Second, on controversial matters, it pays to borrow legitimacy from previous presidents, especially ones of the opposition party. For instance, the early decision not simply to proceed along Vladivostok lines was a policy error. Beware of raising the threshold of objectives lest more modest steps become suspect. For example, the Jackson-backed comprehensive proposals to Moscow early in the Carter administration quite possibly led Carter into a trap set by opponents of détente.

Third, no one familiar with Washington during the Carter years could have been unaware of the systematic "Jackson mafia" campaign against both détente and anyone with possible influence who did not oppose it. Dealing effectively with such opponents involves changing the "face of the issue" or mobilizing proponents of equal zealotry and clout. The Carter administration made little attempt to do either.

Fourth, personal resentments and career motives will dominate all but the most dramatically meritorious policies. SALT II was not one of those. Nor is there any reason to believe that the institutional reform suggestions will in general offset more parochial considerations, especially shortly before elections. When policies involve personal costs, ask for the ones that matter most first because you will not get support for very many.

Fifth, Carter's tactics almost worked in spite of everything. SALT II was headed for ratification until foreign events knocked it off track. A faster timetable or a different process might have worked. Why did Carter not use available precedent to go for a majority approval by both houses of Congress? With continuing foreign cooperation, ratification might have succeeded. Where was the strategy to secure continuing Soviet and other foreign cooperation or to hedge against its not being forthcoming? Domestic strategy will not suffice.

Sixth, arms control, unlike arms reduction, focuses on national advantage and enhanced military power. It is not compatible with signs of U.S. decline. Ratification was lost when foreign events seemed to signal decline and an administration ambivalent about national power and national principles and fumbling. Simple principles and confidence increase presidential influence. The opposite traits undercut it.

The work is, in general, balanced and judicious. Yet several matters merit more attention than they receive. The reader looks in vain for explicit connections to an American policy world far different from that centered on U.S.–Soviet rivalry and mutual threats of extinction.

University of Pittsburgh DAVIS B. BOBROW

Core/Periphery Relations in Precapitalist Worlds. By Christopher Chase-Dunn and Thomas D. Hall. Boulder: Westview, 1991. 300p. $27.50 paper.

Sociology of the Global System. By Leslie Sklair. Baltimore: Johns Hopkins University Press, 1991. 869p. $42.00 cloth, $13.95 paper.

From perspectives that view the world as primarily a capitalist system but in a spirit that is increasingly

ecumenical, two new books out of sociology raise issues of broad implications for the social sciences.

Leslie Sklair deplores the fact that "with few exceptions . . . social scientists have not yet come to regard the whole world as a legitimate object of knowledge." The difficulties attending such an undertaking are obvious, and attempts to derive universal propositions from comparative sociology have been unfruitful. To remedy this state of affairs, the author proposes a sociology of the global system that goes beyond the "state-centric" approaches of many students of international relations, a conception based on "transnational practices."

For Sklair, "sociological totality" is constituted by transnational practices that are analytically distinguished on three levels: economic, political, and cultural-ideological. In the world of today, "largely structured by global capitalism," these three practices are typically represented by three major institutions, namely, the transnational corporation, the transnational capitalist class, and the culture–ideology of consumerism, respectively: "Effective [transnational corporation] control of global capital and resources is almost complete" (p.).

This is a bold and simple idea, buttressed by fieldwork on export-processing zones (e.g., in China and Mexico); a wide reading of recent literature on multinational corporations, especially in the Third World; and much attention to the Second World. But it offers no more than a "thin description" of the "whole world," whose mysteries we are trying to unravel. Transnational corporations are indeed responsible for the bulk of international trade, but they employ less than 1% of the world's labor force and add no more than about 10% to the world product. To claim for them "effective control" of world capital and resources, as well as of politics and culture, seems to exaggerate unduly and to leave room for little else. In any event, time may be ripe for a new synthesis. The bulk of transnational corporation activities is now in the developed world. The Second World is in the process of dissolution, and the Third World is, consequently, also transforming. The source of difficulty seems to lie in Sklair's equating the global system, in whose economic elements the transnational corporation is indeed quite prominent, with the world system and the world economy, in which it is much less so.

Christopher Chase-Dunn and Thomas Hall's *Core/Periphery Relations in Precapitalist Worlds* is an effort to reinvigorate the world system theory by extending its reach in time. The editors of this collection of 10 pieces (including substantial contributions by two political scientists, David Wilkinson and Barry Gills, coauthor with André Gunder Frank) set out to place world system analysis on a new track, by discarding what have been some of the basic tenets: the singularity of the modern capitalist world economy, the axiom that the capitalist core exploits the periphery, and the strategic role of the trade in bulk commodities to that process. While "in no way endorsing a unilinear model of evolution," they propose a threefold typology of world systems (kin-based, tribute-based, and capitalist) and include a paper by Stephen Sanderson, who denies any "inherent antagonism between an evolutionary and a world-system perspective."

Such ecumenism is welcome. It might be signaling the waning of the "age of paradigms" and the return of the search for truth. It also reflects the growing conviction that the social sciences, in their quest for a full grasp of the human experience, cannot be confined to the last five hundred years and need to extend their sights to maybe at least five thousand years of world system history. But the book itself does not point to a clear road ahead. Chase-Dunn and Hall themselves advocate a "variation-maximizing," comparative research program on intersocietal inequalities among different sorts of world systems, including stateless societies.Their contributors Gills and Frank argue for a continuity-emphasising study of the "one, cumulative" world system leading up to a "comprehensive world system history" that is "humanocentric." I opt for the latter.

University of Washington GEORGE MODELSKI

Irredentism and International Politics. Edited by Naomi Chazan. Boulder: Lynne Rienner, 1991. 161p. $26.50.

With the breakup of the Soviet Union and the Cold War officially declared at an end comes a well-timed book on the subject of irredentism and international politics. Irredentism—a state's attempt to redeem foreign-controlled territory containing ethnically similar peoples—is a concept that has not received a great deal of examination or discussion within the field since the period of decolonialization began, following World War II. Irredentism is a latent phenomenon that normally only manifests itself during periods of massive transformation in the international system—as with the establishment of the new European states following World War I, the process of decolonialization following World War II, or the end of the rigid bipolarity of the cold war era. Contemporary irredentist issues are evidenced by Poland's cautious acquiescence in the reunification of the German state, Iraq's invasion of Kuwait, and the newly installed Khadafi-aided regime in Chad. All point to the resurgence of irredentist claims and the role of irredentism in international politics.

One of the major reasons for undertaking a reexamination of irredentism is that because of its intermittent manifestations and lack of attention from the field of political science, it lacks conceptual clarity. The term *irredentism* has been mistakenly used in the past to describe any form of territorial dispute. On the other hand, it has also suffered from overly restricted usage. Crawford Young states in his *Politics of Cultural Pluralism* that "despite the historical artificiality of most state units in the third world, Somalia is the base for virtually the only culturally based irredentist movement" (1976, 95). Most scholars within the field of ethnicity and nationalism would take issue with this statement. Benyamin Neuberger devotes his entire essay in Chazan's volume on the intricacies of real and potential irredentist claims on the African continent. Young's statement does illustrate, however, the lack of a precise understanding of what is meant by *irredentism*.

Milton J. Esman defines *irrendentas* as "territorially-based minorities contiguous to a state controlled by their co-ethnics" ("Ethnic Pluralism and International Relations," *Canadian Review of Studies in Nationalism* 17 [1990]: 83). The concept of irredentism, he argues, is closely related to the doctrine of self-determination of peoples; but (as his definition suggests) it is the state and its elites that seek to reclaim irredentas.

Although Chazan and the other authors in this collection seek to establish a concrete definition of irredentism and to clarify some of its conceptual ambiguity, their discussion leads only to a further muddling of the

concept. This is particularly the case with the essay by Donald L. Horowitz, who seemingly blurs the distinction between the right to national self-determination and irredentist claims. Horowitz maintains that beside the statist element recognized by Esman, irredentism also consists of "the attempt to detach land and people divided among more than one state in order to incorporate them in a single new state," citing the formation of a Kurdistan as an example (p. 10). The Kurdish call for the creation of their own independent state is an example of a nonstate nation's attempt to exercise its right to self-determination. It is not, however, an example of irredentism.

In her introductory chapter, Chazan states that this examination of irredentism was undertaken to "shed light on the key facets of irredentism in comparative and historical perspective" (pp. 1–2). She argues that irredentism is conceptually distinct from separatism and that irredentist claims are more than mere territorial disputes. The Argentine claim upon the Malvinas/Falklands could not be considered irrendentist because "the islands do not contain an Argentinian population" (p. 3). Unfortunately, Chazan, like Horowitz, seeks to attach a "second variant" to the concept of irredentism, in which "components of multiethnic states have irredentist claims aimed at creating entirely new nation–states" (p. 2). Again, Chazan seems to be addressing here the right to the self-determination of peoples or perhaps even separatism, concepts that are related to, but distinct from, irredentism.

Not all of the authors of this collection fall into the same trap as Horowitz and Chazan in seeking to meld two distinct concepts. Neuberger, in a highly competent and instructive discussion of the dynamics of African irredentism, recognizes the role of the state in launching irredentist claims. He states that "we must make clear that irredentism means an attempt made by an existing state to 'redeem' territories and peoples it considers its own" (p. 97).

In her concluding chapter, in light of the discussion within the eight essays that make up this text, Chazan offers a synthesized redefinition of *irredentism* that (finally) concedes the role of the state: "Irrendentism . . . refers to attempts by existing states to annex adjacent lands and the people who inhabit them in the name of historical, cultural, religious, linguistic or geographic affinity" (p. 139). In light of this definition, however, I am confident that I could still introduce Chazan to an Argentinean friend or two who would like to discuss with her the merits of Argentina's irredentist claims upon the Malvinas.

Taken as a whole, this is an extremely well-timed book in light of issues in contemporary international politics; and it seeks to address issues that lie either at the heart or in the rhetoric of interstate conflict—issues that have received scant attention from the field as a whole, with the exception of those who focus upon ethnic group relations and nationalism.

University of Arizona THOMAS A. HOPKINS

Debt and Crisis in Latin America: The Supply Side of the Story. By Robert Devlin. Princeton: Princeton University Press, 1989. 320p. $35.00.

Given the vast literature spawned by the international debt crisis of the 1980s there is now an added burden of originality applicable to any new monograph on the topic. Robert Devlin discharges that burden impressively in the present volume. While most analysts have focused on the role of both misguided debtor policies and 'northern' macroeconomic conditions in the creation of the crisis, few have subjected the creditor banks and the conditions of loan supply—what Devlin calls 'the supply side'—to sustained examination. Central to this lacuna are certain myths which have obscured the role of the banks in the genesis and management of the crisis. Absorbed by the possibilities of internationally unregulated private credit in the 1970s, an influential section of northern opinion came to view the banks as virtual 'freedom fighters' restoring classic laws of capital allocation on a global scale and enforcing financial discipline on a largely profligate South. Subsequently, when key debtors fell into defacto default the same opinion-makers largely absolved the banks of responsibility and endorsed the forced adjustment of debtor states by the compression of internal demand.

Devlin's central concerns are to critically evaluate this worldview, and to highlight the coresponsibility of the banks in the genesis of the debt crisis, their success in pushing the bulk of its costs onto the debtor states, and the gravity of the resulting situation in Latin America. He elaborates these concerns in five ways. First, he portrays the growing incentives for the U.S. money-center banks to go abroad from the 1950s. Second, he constructs an eclectic theory of loan supply to Latin America. From modern portfolio theory he draws insights as to how profit differentials influenced the drift of banking activity from domestic to international markets, and using industrial organization theory he highlights the oligopolistic character of international banking especially the leadership role of the key U.S. banks. This institutional context, Devlin contends, led the industry leaders to focus excessively on non-profit objectives and created a 'bandwagon' attitude to lending among the smaller banks. It also encouraged a high degree of permissiveness in loan approval and refinancing. Devlin illustrates his theory by constructing detailed and persuasive interpretations of the Bolivian and Peruvian cases. He concludes by advancing a relatively novel view of the crisis: namely, that the banks displayed a strong tendency to overlend in the upswing of the credit cycle and to overcontract on the downside. In this his work bears a close affinity with that of Hyman Minsky and other theorists of financial crisis.

An analysis of the banks' bargaining stategies during the repeated rescheduling negotiations of the 1980s forms the third element of the study. Devlin argues that the banks, buttressed by both the key OECD states and widely shared precepts of financial orthodoxy, effectively formed a creditor's cartel through which they shifted most of the costs of the crisis to the debtors. While this argument is not new, Devlin's achievement is to give it a highly persuasive analytical and empirical cast. The fourth element of the inquiry is a critique of the forced adjustment imposed on the debtors. For Devlin, the resultant outward transfer of resources from Latin America, nearly one-fifth of the region's domestic savings, has been a major disaster. The excessively short-term focus of adjustment programs, the serious erosion of the debtor's productive base they induced, and the inadequacy of the compensatory finance flowing to Latin America have all created a vicious cycle of economic and political instability. Arguing from a Keyne-

sian perspective, Devlin contends that any socially efficient adjustment should target the debtor's capacity to pay, and, in turn, transfer resources abroad only out of trade surpluses which reflect a rise in savings and tradeable goods. He is scathing of the I.M.F.'s failure to balance the provision of lender-of-last-resort facilities for the banks with socially efficient adjustment for the debtors. The final part of the study considers options for change. Anticipating the most recent policy debates, Devlin argues for debt forgiveness, the importance of multilateral debt initiatives, and adequate public finance for growth.

Foremost among the strengths of this volume is the theory of loan supply which insightfully synthesizes approaches across perspectives. This is complemented by rich empirical material and a subtle, nascently post-empiricist methodology. It is, however, open to at least one major criticism. While Devlin marshals a vast amount of material on the role of the banks, state elites and ideas in the episodes he considers, he fails, even briefly, to theorize the wider politico-economic context within which the banks acted and bargained. Why, for instance, have the banks enjoyed so much power with governments in matters of international banking over the last two decades, and what does this tell us about developments in both the direct and indirect power of business especially transnational finance over this period?

This criticism aside though, Devlin's study is a subtle and judicious one which has made an important contribution to both an under-researched perspective on international debt issues and a growing literature on the politics of creditor coalitions in the international political economy.

University of Sydney IAN BELL

George Ball, Vietnam, and the Rethinking of Containment. By David L. Di Leo. Chapel Hill: University of North Carolina Press, 1991. 220p. $37.50 cloth. $12.95 paper.

David L. Di Leo has produced a well-written and thoroughly researched volume on one of the more influential American leaders of the cold war years, George Ball—a man who has been called a "curmudgeon" and would most likely agree with the description. Di Leo even titles one of his chapters describing Ball, "A Wholly Partisan Critic."

Di Leo traces the wide sweep of Ball's career through government work, dabbling in politics and the business and legal professions. Ball began his varied career in the 1930s, just as storm clouds were looming over Europe and formally ended it in the 1980s, just before the formalities of the forty-year-old cold war began slowly coming to an end. Ball began as something of a political outsider at the beginning of the Kennedy administration; but through the help of his friend and mentor Adlai Stevenson, he was appointed to the number 2 position in the State Department as undersecretary for economic affairs.

Ball is perhaps best known for his involvement in the debates on American policy towards the Vietnam War. These debates began as early as 1961, even though (as Di Leo notes) other areas of the world were actually of greater interest to Ball than Southeast Asia. By 1961 Ball had already emerged as an opponent of any further American action in Vietnam based both on his broader interests (in his judgment, other areas of the world were much more important to the United States at the time) but also because of his reading of the defeat of the French in 1954. Even at that early stage, it was clear that Ball was in the minority.

These debates continued and became the fulcrum point for the Johnson administration in 1964 and 1965. The familiar arguments were put forth by many: the so-called commitment by the United States to South Vietnam under Eisenhower, the "need" and the "vitalness" of South Vietnam for United States national security interests, and the analogy with Korea (i.e., aggression must be stopped). Di Leo presents a very detailed account of these debates, picturing Ball as an extremely articulate player although very much in the minority—primarily because key players Johnson, Rusk, and MacNamara very much favored the early-1965 bombings and the further prosecution of the war itself. Of note about these debates, however, is the way at least some of the players could be simultaneously "antagonists and allies"—disagreeing on Vietnam but not necessarily allowing that to spill over into other areas. Needless to say, such a pattern did not necessarily apply to all the players.

Fortunately, Di Leo goes beyond the rather strict confines of a historical narrative that most of the book is devoted to. He also places Ball—especially in terms of the arguments over the Vietnam involvement but also in the larger argument about containment as a policy—into the broader context of the theoretical debates of the cold war years. While "realism" was the linchpin of many of the justifications for the containment, he notes that the "radical revisionist" school opposed many of the tenets of realism and that policy prescriptions derived from such a school therefore differed radically from many of the policies derived from containmnt. Here, "the rethinking of containment" comes into the picture, because these debates were the first time that any of the tenets of traditional "containment" policy as manifested in the Vietnam decisions were brought into question.

Di Leo's book is certainly detailed in terms of historical argument and presentation. In fact, therein lies one of the minor criticisms, namely, that it is often difficult to separate the wheat from the chaff. What is important, and what is not? Which meeting is important, and which is not? Second, while subtly critical of Ball at various points, Di Leo's admiration clearly shines through, and one wonders how much positive bias this might have rendered. Third, it is not clear exactly how Di Leo's book fits into the broader literature on the Vietnam conflict for there is little systematic citation of it. Fourth, the findings and evaluations presented throughout the book are sometimes confusing because so extensive. It is as if the reader is continually treading water, not knowing whether the end of this paragraph or the next or the end of the chapter will bring some relief, conclusion, evaluation.

Going beyond these somewhat narrow comments, however, two other observations can be made. First, Vietnam was not of major concern to Ball. Di Leo makes it quite clear that other areas of the world (especially Europe, with a focus on France and England) were of much more interest to him. As Di Leo notes:

> An estimate derived from his appointment calender during his six-year tenure as under secretary of State shows that he spent perhaps as little as 5 to 10 percent of his time on Vietnam. Save for intermittment periods when he was thoroughly consumed by the subject, the Europe-focused undersecretary mostly busied himself with the world far to the north and west of Indochina. (p. 153)

This leads to the second observation: Why was this the case? If 90%–95% of Ball's attention was devoted to non-Vietnam issues, why is he remembered mostly for his dissenting views on Vietnam? Is it because he was one of the few dissenters on the subject? Or is it because, in fact, at root, he was—or played—the curmudgeon? But curmudgeons are often known to alienate those they are "curmudgeoning" against. One is then led to ask the broader question whether, if Ball had eschewed this role during the Vietnam debates (especially since his true interests were apparently elsewhere), he might have had a much more direct and positive impact on the debates? It is of note that his role in history will most likely hinge around the Vietnam debates, while his true interests were apparently elsewhere.

Despite these clearly speculative observations, Di Leo's book is extensive and well researched. Anyone interested in the internal Vietnam debates of the early 1960s, in the give-and-take that goes on in many internal debates, and (obviously) in George Ball, this is must reading.

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Conflict and Intervention in Africa: Nigeria, Angola, Zaire. By Herbert Ekwe-Ekwe. New York: St. Martin's, 1990. 195p. $59.95.

The Superpowers and Africa: The Constraints of a Rivalry, 1960–1990. By Zaki Laidi. Chicago: Chicago University Press, 1990. 232p. $45.00 cloth, $14.95 paper.

The Nonsuperpowers and South Africa: Implications for U.S. Policy. By Richard J. Payne. Bloomington: Indiana University Press, 1990. 314p. $35.00.

Middle Power Internationalism: The North–South Dimension. Edited by Cranford Pratt. Montreal: McGill–Queen's University Press, 1990. 167p. $32.95.

Given the tremendous changes occurring in the structure of international relations, particularly the collapse of the Soviet empire, the need to focus greater attention on the foreign policies of countries other than the two superpowers is slowly being recognized. Three of the four books reviewed here are of particular interest for their contribution to the less well developed literature on the interests, policies, and actions of nonsuperpowers.

Cranford Pratt's book focuses on the failure of the South to achieve any significant reforms of the international economic system. In the introductory chapter, Pratt reviews the debate of the 1960s and 1970s over the creation of a new international economic order based on "humane internationalism." He argues that there is a "carefully reasoned theoretical and ethical foundation for humane internationalism" and presents three possible methods for its implementation: radical internationalism, reform internationalism, and liberal internationalism (p. 8). However, despite some support for a more equitable international order, most leading policymakers in the developed world have accepted too readily the principles and assumptions of the school of realism. According to Pratt, realism pushes states "to enhance their power and their influence in order better to advance their interests," which eventually "becomes a self-fulfilling prophecy" (pp. 11, 13). While deploring the policies and shortsightedness of the North, the book focuses on the possible role that middle powers (five in particular) might play in persuading the countries of the North to accept humane internationalism as a priority.

In the three chapters that follow, Asbjorn Lovbraek, Bernard Wood, and Raphael Kaplinsky examine the possible contribution of middle powers to alleviating the poverty and underdevelopment of the South. Lovbraek presents a lucid explanation for the inability of the "like-minded group of middle powers led by Norway and the Netherlands to gain support for redistributive policies toward the South. His reasons are three: (1) the premises of the social welfare state at the domestic level could not be convincingly transferred to the international level; (2) the end of the 1970s saw the increase of economic problems in the North, which led to growing calls for protectionism; (3) the philosophy of reform internationalism was accepted by only a few government officials, limiting its appeal even within progressive middle powers.

The chapter by Bernard Wood, although interesting in its conceptualization, ends with some rather limited conclusions. Instead of only 5 middle powers, Wood expands the definition of middle powers to include 33 countries based on both quantitative and qualitative measures (gross national product and influence, respectively). After presenting an overview of past experiences of middle-power action, Wood argues that "there is scope for co-operative middle power efforts in the international economy" while admitting that middle powers, in the past, have "seemed to be almost as locked into patterns of immobility as other states" (pp. 81, 89). Wood goes on to ask a sometimes overlooked question: What impact will the decline of U.S. hegemony have on middle powers? Although his conclusions are carefully qualified, Wood suggests that international cooperation among middle powers is possible but unlikely.

Kaplinsky, in the third chapter, analyzes the impact of microelectronic technologies used by the newly industrializing countries in Asia on middle powers. He argues that as the newly industrializing countries become increasingly competitive with the middle powers in certain industries, unemployment (in the latter group) will resist easy solutions, and conflict between the two groups will increase. The result will be an increase in protectionism or "a restructuring towards more managed patterns of trade" (p. 139). Kaplinsky's premise, that the newly industrializing countries have been able to compete successfully with middle powers, contrasts with the basic argument of the rest of the book that the South will never achieve economic progress without a new international economic order.

Pratt's study of middle powers and humane internationalism is important for its treatment of a topic that has received little attention in the literature. The difficulties in persuading the North to restructure its economic relationship with the South are well elucidated but should come as no surprise. Unfortunately, the book suffers somewhat from a lack of conceptual clarity and may appeal less to a scholarly audience than to sympathetic policymakers. Although Pratt's strident criticism of the North may seem one-sided to some readers, his

opinions are clearly presented and are worthy of consideration.

Richard Payne also focuses on nonsuperpowers. He analyzes the policies of Britain, West Germany, Japan, Israel, Scandinavia, Canada, and the frontline states toward South Africa. No explanation is given for why he included these states and excluded such nations as France, India, and Australia. In his opening chapter, Payne discusses the factors that combine to influence the relations between the nonsuperpowers and South Africa. They include economic interests, ideological and geopolitical interests, domestic racial problems, military interests, domestic public opinion, relations with Africa and the Third World in general, and the (Former ?) threat of the Soviet Union. After assessing the "new realities" in South Africa, including political and economic developments, as well as the growing internal and external opposition to apartheid, Payne discusses two possible strategies that the nonsuperpowers might employ to press for the abolition of apartheid in South Africa: economic sanctions and black empowerment.

In each of the next seven chapters, the author examines the constellation of factors that have influenced each nation's foreign policy toward South Africa. Relying exclusively on English-language sources, he discusses the strategies (sanctions and black empowerment) implemented and outlines the nonsuperpower's policy toward the frontline states (those that border South Africa). Each chapter precisely follows this outline, which helps the reader make comparisons and assess the arguments. In addition, the chapters are cogently written and present an excellent starting point for further study.

Of particular interest are the chapters on Britain, Israel, and the frontline states. According to Payne, although Britain's foreign policy is largely a product of its economic ties to South Africa, other interests cannot be completely ignored. Trade between South Africa and Britain is important (British exports stood at $1,156 million in 1987), and Britain represents one of the largest sources of foreign investment. This economic link, when combined with the British government's claim that trade with South Africa protects jobs in Britain, has predictably led to an unwillingness to enforce severe sanctions against South Africa. Britain did partially agree to a European Community sanctions package in 1986, but the sanctions were rendered ineffectual due to the lack of British and West German support. Instead, the Thatcher government argued that economic sanctions would be counterproductive, because they would create greater self-reliance, reinforce the extremist Right in South Africa, and lead to greater unemployment for black South Africans. Payne concludes that British leaders may not approve of apartheid but that "they have no inclination or incentive to disturb a profitable relationship" (p. 58).

Israel, unlike any of the other countries discussed, bases its foreign policy almost exclusively on national security. In response to the 1973 war and declining African and European support, the Israeli government decided to cooperate with South Africa despite Israel's express condemnation of racism. This cooperation has been reinforced by technical and research collaboration, Israeli trade and investment activities, and the disinterest of Israeli public opinion.

Like Israel, the frontline states also pursue contradictory policies toward South Africa. Payne explains that there is a "chasm between Pretoria's neighbors' rhetoric against apartheid and their reality of extensive economic interdependence with the white minority regime" (p. 202). Economic factors dominate the foreign policy calculations of the frontline states. Dependence on South Africa takes many forms, including employment of frontline states' workers and the purchase of agricultural products, oil, and electricity. At the same time, however, South Africa is also dependent on the frontline states for markets for its manufactured products. This economic interdependence has led the frontline states to harshly condemn the apartheid regime, while at the same time opposing sanctions against South Africa.

In his conclusion, Payne argues that the United States should pay closer attention to the foreign policies of the nonsuperpowers toward South Africa; and he suggests a series of measures that the U.S. government should implement. Although his arguments may have merit, his brief discussion of the implications for U.S. foreign policy appears unrelated to the seven core chapters and adds little to the rest of the book. Another conceptual framework such as a theoretical or comparative analysis of the strengths and weaknesses of nonsuperpowers would have been more effective. Overall, Payne's study is well written and clearly formulated and fills a gap in the comparative foreign policy literature.

Herbert Ekwe-Ekwe has written a short but incisive book whose aim is "to investigate the nature of civil conflict and intervention in Africa with special reference to Nigeria, Angola, and Zaire" as representing the three dominant types of national liberation movements (p. 1). While not ignoring the presence of the two superpowers in each conflict, Ekwe-Ekwe also investigates the role of the Europeans (primarily France, Great Britain, Belgium, and Portugal); contiguous states; and regional and other local interest groups. The strength of his case studies rests on his insightful analysis and inclusion of all the important forces in each interventionist regime, rather than on a detailed presentation of the unfolding of each conflict and intervention.

After a short introductory chapter presenting the five major themes to be examined in each case study, Ekwe-Ekwe analyzes each of the three countries separately (chaps. 2–4). In the case of Nigeria, he argues that Great Britain played a key role in determining the outcome of the struggle between the Nigerian federal government and Biafra through its arms deliveries to Lagos. The Biafran call for secession was considered a threat to Britain's widespread economic interests in Nigeria (£1.5 billion), and this concern led the British government to side with the federal government, which supported the status quo. The Soviet Union, most African states, and the Organization of African Unity also favored the continuation of federal Nigeria, albeit for different reasons. Although France and Portugal gave some support to Biafra and the United States maintained a questionable neutrality, Ekwe-Ekwe argues that their limited response only served to postpone the inevitable collapse of the Biafran resistance.

In the case of Angola, the two superpowers, along with Cuba and South Africa, intervened to protect their perceived political, economic, and strategic interests. For the United States, the fear that the Soviet Union might gain a foothold in Africa and the desire to protect U.S. economic interests were the primary motivations behind its support for the (FNLA–UNITA) front. South Africa worried that the collapse of Portuguese rule in

Angola, in conjunction with a (MPLA) victory, would threaten its strategic and economic interests. These considerations eventually led South Africa to intervene militarily on the side of the FNLA–UNITA forces in Operation Zulu. U.S. and South African support for the FNLA–UNITA forces, however, was matched by Soviet and Cuban support for the MPLA. According to Ekwe-Ekwe, Soviet assistance through arms deliveries was motivated primarily by ideological reasons, not (as others have argued) strategic ones. He also asserts that Cuba's decision to intervene militarily (Operation Carlota) was made independently of the Soviet Union. Both of these assertions have been the subject of much debate, and Ekwe-Ekwe's rather brief discussion does not provide the definitive or final word.

In chapter 3, the author discusses the two attempts by the National Front for the Liberation of the Congo (FNLC) to take over the province of Katanga (presently Shaba). On each occasion, the FNLC was easily defeated for several reasons, the most important being the intervention by France and Belgium. France and Belgium had extensive economic interests in Zaire and considered the federal government under General Mobutu to be a better protector of the status quo. Egypt, Morocco, and the Sudan also supported Mobutu, while Cuba and the Soviet Union chose not to become involved. Ekwe-Ekwe concludes that external intervention was decisive in the rapid termination of the Shaba insurrections.

In chapter 5, Ekwe-Ekwe, in a somewhat confusing outline form, compares the essential features of the three interventions. While some of the propositions and assessments are of considerable interest others are both obvious and repetitious. Although this chapter would have benefited from a more concise and better-organized presentation, the analysis and conclusions, in general, remain sound. In sum, Ekwe-Ekwe has written a thought-provoking and convincing book on the critical importance of external and extracontinental forces on conflicts in Africa.

Zaki Laidi has written a scholarly study of the superpowers and Africa from 1960 to 1985 (despite the title) that merits considerable attention. In chapter 1, Laidi argues that the logic of the cold war and the struggle between the two superpowers was extended to Africa after World War II. Within a diplomatic–strategic framework, each sought both to check the other and to convince the African countries of the correctness of its ideology. Laidi argues that the United States and the Soviet Union did not achieve an understanding of the unique characteristics of Africa but, rather, projected "an eminently ethnocentric vision onto the African continent" (p. 5). However, despite concerns over communist advances, the United States did not consider Africa either "in the first perimeter of defense" (p. 31) or worthy of major economic investments.

In chapter 2, the author considers the possibility that Africa was of marginal interest to the two superpowers from 1965 to 1974. In the mid-1960s, apparent constraints on the power of the Soviet Union forced a choice between activism in the Third World and military parity with the United States (p. 32). The United States, increasingly involved in Vietnam, was also forced "to hierarchize its global objectives and relinquish some of its obligations to its Western allies" (p. 33). Given Africa's resistance to taking sides in the major East–West conflicts, these developments further diminished the interest of the superpowers in the continent.

The Angolan Crisis of 1975 (chap. 3) represents a turning point, or "line of fracture in relations between the great powers and Africa" (p. 59). Laidi discusses in some detail the ethnic divisions within Angolan society and the process by which the conflict became internationalized. In an analysis that complements Ekwe-Ekwe's chapter on Angola, Laidi outlines the process by which the Soviet Union and Cuba gradually became staunch supporters of the MPLA, while the United States and China stood behind the FNLA–UNITA forces. Unlike Ekwe-Ekwe, Laidi's analysis of Cuba's "independent" role in the conflict is more nuanced. He argues that without Soviet support, "Cuba never could have carried out an operation of such magnitude" (p. 72).

One important implication of the Angolan crisis was the "active integration of the African system into the East–West framework" (p. 74). This meant that each superpower, albeit in different modes, became more involved in the internal dynamics of its African "allies." The Soviets continued to place primary emphasis on the diplomatic–strategic arenas, while the United States also was anxious to preserve the "direct economic interests whose relative importance it had discovered" (p. 75).

In chapters 4 and 5, the author delineates American and Soviet foreign policy toward South Africa from 1975 to 1985. His analysis covers a broad spectrum of topics including U.S. diplomatic efforts to solve the crises in Zimbabwe and Namibia, the "missassessment" by U.S. policymakers in the Horn of Africa, and the change in U.S. aid policies beginning in 1976. Laidi argues that despite Carter's rejection of an absolute bipolar world, in general, the United States continued to view Africa in an East–West context, with some attention to economic interests. Interestingly, U.S. attempts to manipulate the content and direction of politics in Africa were often thwarted by complex African and European interests. For example, Kissinger's plan to solve the conflict between the white minority regime and the black majority failed dramatically due to opposition from both the frontline states and internal liberation movements.

Like the United States, the Soviet Union also experienced diplomatic and strategic reversals in Africa. Laidi argues that Soviet policy, based on the dual concepts of *a power like others* and *an ally in protest*, did not lead to substantial gains in influence. Despite numerous treaties of friendship and cooperation and a large degree of military assistance, "Soviet economic cooperation does not offer African states any clear advantage" (pp. 191–92). The "poor economic complementarity of the Soviet Union and Africa" and the "parsimony of Soviet bilateral aid" (p. 179) were key factors in the maintenance of ambiguous and precarious alliances.

Laidi concludes his study by arguing that Africa is suffering from a "loss of status" resulting from "lopsided integration" and a "new hierarchy of values in the world system"—both of which are detrimental to Africa (p. 196). In addition to the economic decline of Africa in the 1980s, as witnessed by the seriousness of the debt crisis, the superpowers have also reassessed their worldwide interests. Both the Soviet Union and the United States became less interested in ideological support and more interested in the reduction of costs. Both goals have served to diminish superpower interest in, and commitment to, the African countries.

The strengths of *The Superpowers and Africa* are manifold. The actions, motives, and implications of super-

power involvement in Africa are presented in detail. Each chapter (except the conclusion) is well organized and makes good use of primary sources to support a perceptive analysis. The final chapter, however, suffers somewhat from a lack of relevance. It is strange that Laidi would conclude with a discussion of the implications of French decline for Africa, a topic untouched upon in any previous chapter. Unfortunately, despite the title, Laidi does not examine the implications for Africa of the dramatic changes occurring in the Soviet Union beginning in 1985 or the developments in South Africa. Although there are omissions, they do not radically detract from the central arguments of the book. This book will be of interest to the specialist, but its complexity will render it less accessible to the nonspecialist or general reader.

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A Substitute for Victory: The Politics of Peacemaking at the Korean Armistice Talks. By Rosemary Foot. Ithaca: Cornell University Press, 1990. 273p. $32.50.

What if President Bush had agreed to negotiate with Saddam Hussein during Operation Desert Storm? Although this very good book is about the beginning of the Cold War in Asia and the end of the Korean war, reading it in the aftermath of the Guld War considerably enhances its relevance to the study of international relations and diplomacy today. It is an excellent text for courses in history, as well as in the politics of foreign policy decision making. Foot explains the perils of pursuing a policy that sought to gain at the conference table what could not be achieved on the battlefield. She also convincingly demonstrates how difficult it is to negotiate while fighting. Taken together with her earlier volume, *The Wrong War* (1985), the author has produced the best work of the 1980s on the Korean war and in a way that should appeal to readers whose interests extend well beyond this particular period of history.

The parallels between Korea and the Gulf are startling. Both wars began with an act of clear-cut aggression. They were costly, nasty conflicts in which civilian populations suffered enormously. The United States fought in both as part of a multinational coalition sanctioned by UN resolutions but on behalf of allies who saw the stakes of the conflict in far less global terms. The adversary was vilified to the point where it seemed almost inconceivable that an honorable settlement could be achieved by diplomacy. And both wars generated broad and substantial consensus on the need to punish such aggression regardless of how and when a deal could be struck.

But the central message here is that under such circumstances negotiations, if they take place at all, are likely to be protracted, frustrating, and replete with "tentative opportunities to end the conflict . . . left unexplored" (p. 217). Diplomacy, the author points out, was not a substitute means for compelling the aggressor to withdraw. A war had to be fought and virtually won. But the exigencies and emotions associated with the fighting also produced the tendency to obscure and minimize the chances to make a settlement sooner than the one that was signed in a tent at Panmunjom, some two years and 575 meetings after the negotiations started; for, according to Foot, both sides tended to treat each point on the agenda as if it were a piece of strategic territory to be attacked or reclaimed, rather than a basis on which to base a political settlement. This exceedingly well written and thoroughly researched book reminds us that without a policy process to coordinate military and diplomatic strategies, neither fighting nor negotiating are sufficient to produce victory.

This was the lesson of Korea. A decade later we failed to apply it in the case of the Vietnam War. And the Gulf War ended so quickly that no one can yet be sure whether it has been properly learned.

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World Justice? U.S. Courts and International Human Rights. Edited by Mark Gibney. Boulder: Westview, 1991. 175p. $39.95.

Federal Courts and the International Human Rights Paradigm. By Kenneth C. Randall. Durham: Duke University Press, 1990. 295p. $45.00

International Fugitives: A New Role for the International Court of Justice. By Barbara M. Yarnold. New York: Praeger, 1991. 149p. $37.95.

In 1980, a Paraguayan doctor named Joel Filartiga and his daughter Dolly, who were both living in the United States, where they had applied for political asylum, brought suit in New York district court against the former Paraguayan police chief, Americo Pena-Irala, having learned of his presence in New York. Pena-Irala had kidnapped Dr. Filartiga's son in Paraguay and had tortured him to death to silence Dr. Filartiga's outspoken criticism of the Stroessner regime. The district court dismissed the case, ruling that torture of a Paraguayan in Paraguay by a Paraguayan official did not violate U.S. law.

The Second Circuit Court of Appeals overruled and held a little-known federal law, the Alien Tort Claims Act (28 U.S.C. sec. 1350 [1789]), applicable law in this case. Under the Alien Tort Claims Act, aliens are permitted to sue other aliens for violations of international law in U.S. federal courts. Since the United States had not ratified any of the major international human rights treaties, this landmark case opened the door for the prosecution of international human rights cases in U.S. federal courts. Whereas a number of cases have been brought under the Alien Tort Claims Act, the Bush administration has recently let it be known in an amicus brief in a California federal court action pending against former Philippine President Marcos that it does not view this statute as applying to cases presented by aliens against other aliens for human rights violations.

Mark Gibney's edited volume *World Justice?* and Kenneth Randall's *Federal Courts and the International Human Rights Paradigm* examine the role of U.S. courts in the enforcement of international human rights, especially the human rights cases that came down in the 1980s and were known as "Filartiga's progeny." As an international human rights lawyer I found both volumes of interest, whereas I wonder if they would have the same attraction for a nonspecialist. Barbara Yarnold's *International Fugitives*, on the other hand, deals with the failure of international extradition procedures.

Yarnold's thesis is that contemporary international extradition procedures are not working and that their violation endangers international peace and security.

This is a debatable argument. Yarnold examines two case studies. The first involves the illegal kidnapping on 11 May 1960 of Adolf Eichmann in Argentina by Israeli agents in order to try him in Israel on charges of responsibility for the commission of crimes against humanity. The second involves the seizure of Manuel Noriega, who surrendered to U.S. authorities on 3 January 1990, following the United States invasion of Panama, in order to try him in the United States on drug trafficking charges. Did either of these incidents lead to the endangering of world peace and security?

In her study of the Eichmann case, Yarnold points out that the Israeli government, in order to preserve the long-standing friendship between Israel and Argentina, a month after the abduction of Eichmann, forwarded to the Argentine government a lengthy document explaining away certain issues in the case. Yarnold states that two deliberate misstatements were made in this letter: (1) that Eichmann was abducted by private volunteer groups when, in fact, these groups were supported by the Israeli government and (2) that Eichmann went to Israel of his own free will when, in fact, Eichmann was picked up by Israeli agents on a public street, taken to a private home, and then placed on a plane bound for Israel without his consent. The Argentine government responded two days later with a letter of protest claiming that its sovereignty had been violated and demanded the return of Eichmann. In the face of Israeli refusal to return him to Argentina and to risk the possibility that Argentina might choose not to extradite him, on 15 June 1960 the Argentine government filed a formal complaint with the Security Council of the United Nations, charging that the Israeli action "created an atmosphere of insecurity and mistrust, incompatible with the preservation of international law."

On 23 June 1960 when the UN Security Council considered the matter, the vote was eight to zero in favor of Argentina. Although the resolution requested Israel "to make appropriate reparation," Yarnold informs us that no such reparation was ever made and that Eichmann remained in Israel. Eichmann was tried and convicted in Israel. He was sentenced to death on 15 December 1961 and hanged on 31 May 1962. If this case teaches us anything, it is the inefficacy of UN Security Council resolutions. The government of Israel got its man, did what it wanted with him, and suffered, at most, a slap on the wrist.

Even recognizing that the United States invasion of Panama, undertaken (in part) to seize Manuel Noriega resulted in the loss of lives, can it be said that this invasion endangered world peace and security? I think not.

To remedy this state of affairs, however, Yarnold advocates that jurisdiction of international crimes should be transferred to the International Court of Justice. In addition, she suggests, jurisdiction over state crimes should be transferred to the International Court of Justice when fugitives have crossed national boundaries and are outside the state in which they committed their crimes. At present, it appears unlikely that states will cede the sovereignty requisite to allowing the International Court of Justice to decide when extradition is appropriate. Some of Yarnold's suggestions, such as the creation of an international prison "so as to avoid imprisonment in states that might transgress the international and human rights of criminals" appear a little whimsical—as, indeed, is her optimism regarding "the contemporary disintegration of the superpowers [which] may facilitate the adoption of these recommendations by the world community." For the record, one superpower claims not yet to have disintegrated.

On a more substantive note, however, it is regrettable that Yarnold did not look to international human rights tribunals for the precedents that they are already establishing in the international adjudication of extradition requests. Specifically, the European Court of Human Rights handed down a unanimous judgment in 1989 in the *Soering* versus *United Kingdom* case, in which the Court held that if the individual in question were to be extradited to the United States, the prohibition under Article 3 of the European Convention, dealing with "inhuman or degrading treatment" would be violated. Soering, a German national wanted for the capital murder of his girlfriend's parents in the state of Virginia, fled to Britain, where he was subsequently arrested. Under the terms of the 1972 United States–United Kingdom Extradition Treaty, Britain could refuse to extradite him if the offense for which the extradition was requested was punishable by death and the requesting party failed to give assurances that the death penalty would not be carried out. Under British law, Soering was found to be psychologically impaired and could not have been found guilty of murder. Soering petitioned the European Commission of Human Rights; but since the European Convention on Human Rights allows for the imposition of the death penalty, Soering based his complaint on the prohibition of "inhuman or degrading treatment or punishment," which he claimed would ensue from the "death row phenomenon" involved in the six-to-eight-year expected delay in carrying out the death sentence in the state of Virginia. This decision of the European Court of Human Rights persuaded the United States to give appropriate assurances to the British Government that no capital murder charges would be brought against Soering, who consequently was extradited, tried and convicted, and sentenced to two life terms, having successfully avoided the death penalty.

Kenneth C. Randall's *Federal Courts and the International Human Rights Paradigm* provides a wealth of knowledge on the role of the federal judiciary and related issues, such as the relationship between federal and state courts and the relationship of the judiciary to the other branches of government; but his thesis is that federal courts should assume jurisdiction over not only human rights cases but also cases involving acts of terrorism, since the victims of one are as innocent as the victims of the other and the perpetrators equally reprehensible.

Notwithstanding the moral force of this argument, Randall misreads human rights law. He argues that the UN Covenant on Civil and Political Rights "bars sovereign *and nonstate* actors alike from committing torture." In fact the UN Covenant and all other international human rights treaties only bar state actors or persons acting under the color of law. The UN Convention against Torture and Other Cruel, Inhuman, or Degrading Treatment or Punishment explicitly defines torture as pain or suffering "inflicted by or at the instigation of or with the consent or acquiescence of a public official or other person acting in an official capacity"; but the color-of-law requirement is implicit in all other treaties. Since the prerequisite for presenting a case before an international tribunal is that the violation is alleged to have been committed by a person acting under color of law, petitioners are barred from bringing cases under

the international human rights treaties against private parties.

World Justice? is an excellent compilation of articles written by international lawyers and political scientists on the subject of the enforcement of international human rights norms. I found this to be the most informative of the three books under review. Dan Bodansky's lead essay, "Human Rights and Universal Jurisdiction," starts from the premise that one cannot rely on domestic court implementation of human rights norms, since often, the government that would hear the claim is the one that is committing the violations. Consequently, human rights activists have begun to use the judicial system of one country to redress violations in another, such, as in the *Filartiga* decision. He notes, however, that the Alien Tort Claims Act has been the source of controversy. Does it create a federal cause of action for violations of international law or only confer jurisdiction on federal courts in cases where the cause of action already exists under international or domestic law?

Bodansky converts a difficult topic into lucid and elegant prose, simplifying the issue for the nonlawyer or the lawyer not familiar with this area of the law. He invokes the international law concept of universal jurisdiction to provide the legislation that would permit suits in federal courts for human rights violations regardless of where they occurred. But in order for universal jurisdiction to be exercised, he explains, the court must be ready to apply international law. A number of treaties exist that have created international crimes and have established both individual and state responsibility, as is the case with the treaties criminalizing genocide, apartheid, and torture. Bodansky distinguishes these new international norms of individual responsibility from individual responsibility for earlier offenses such as piracy or slave trading in that these newer norms can only be violated by individuals acting in an official, not in a private, capacity. Human rights offenses, such as torture, he points out, can only be committed by persons acting under color of law. This rule, which is the cornerstone of all international human rights law, is today gradually being eroded as some states, while others seek to press human rights organizations to expand their mandates to investigate human rights violations, (e.g., killings) perpetrated by such private parties as guerrilla or terrorist organizations.

In chapter 2 of *World Justice?* Steinhardt examines the "doctrines of judicial diffidence." The doctrines of foreign sovereign immunity, the act of state doctrine, and the political question doctrine are treated as representing the unwillingness of the judicial branch to compete with the other branches of government in the area of foreign affairs. The conviction that the state must always speak with *one voice* means viewing international law (according to Steinhardt) as a kind of "ritualized politics" and not as law at all. He recommends that this one-voice orthodoxy be dispensed with in cases with an international human rights component.

John Quigley, in "Enforcement of Human Rights in U.S. Courts," submits that the forcible abduction abroad of persons for the purpose of bringing them to trial in the United States (e.g., Noriega) violates international human rights law. He argues that U.S. courts should refuse to accept jurisdiction over a criminal defendant who has been illegally brought into the United States.

Editor Mark Gibney, in "Courts as 'Teachers in a Vital National Seminar' on Human Rights" suggests that the courts have a didactic role to play in society. Concerned about the incongruity in the United States' treatment of its own citizens as compared with its treatment of citizens of other countries (e.g., Panama, Nicaragua, and El Salvador), Gibney criticizes the failure of the courts to play a role in educating the U.S. population in international affairs. He points out, for example, that in 1987 when the Iran–Contra hearings began, over half of the American public did not know who the Contras were, or which side the United States was supporting. Consequently, the much vaunted support of the Contras by the U.S. public following the congressional testimony of Colonel Oliver North is not to be given much importance, since such support is ephemeral. Litigation, Gibney convincingly argues, would educate the American public because the population would be confronted in a very public way with the effects of U.S. policies: "U.S. policy needs to be publicly defended, [and] the human consequences must be made known and compensation paid to innocent civilians who suffer from the pursuit of these policies."

John Rogers, writing on international human rights law and U.S. law, notes that most international human rights law is not applicable law in U.S. courts because the United States has not ratified the major international human rights instruments. United States law does, of course, afford extensive protection against human rights abuses, he explains, by means of the Bill of Rights and the post–Civil War Amendments to the Constitution. In addition, he adds, other rights are protected by federal and state law.

Rogers argues the other side of the argument presented by Steinhardt and Gibney that the courts should defer from deciding issues that involve the foreign relations of the United States in the interest of preserving the one-voice doctrine of the conduct of international affairs. Rogers maintains that the courts should not determine whether the actions of the U.S. government are legal or illegal.

Howard Tolley, Jr., in a very informative article on the role of human rights interest group litigation in persuading U.S. courts to apply international human rights norms, describes the different groups that have litigated human rights claims in federal courts. He reveals the little-known fact that it was a trademark lawyer named Peter Weiss, of the Center for Constitutional Rights, who unearthed the obscure Alien Tort Claims Act of 1789 and converted it into a viable vehicle for the presentation of human rights claims. He also points out that many of the "celebrated legal successes" in the 1980s were Pyrrhic victories, given that the courts handed down generous awards (e.g., the $10 million awarded in the *Filartiga* case) but that none of the defendants ever paid. In conclusion, Tolley notes that political factors may ultimately matter more than legal doctrines—"as the NAACP discovered in its eighty year quest for racial justice."

Bert Lockwood in the very creative "Toward the Economic *Brown*" predicts that within the next 35 years the U.S. Supreme Court will come down with an "economic rights" case of the impact of the *Brown* decision on basic needs or subsistence rights. Lockwood has argued elsewhere that international human rights law played an uncredited role in setting the stage for the Supreme Court's attack on American segregation that led to the 1954 *Brown* decision. Similarly, he expects international human rights law in the economic and

social area to lead to a *Brown*-like decision in the area of economic rights. His candidate for the constitutional "window" that the Supreme Court could be expected to invoke "for the insinuation of international economic norms" is the privileges and immunities clause of the Fourteenth Amendment of the Constitution. Lockwood's article, in my view, is the most original and creative piece in this volume. Since my own research has taken me into examining the intent and possibilities inherent in the privileges-and-immunities clause in regard to protecting fundamental human rights, I found this argument particularly attractive.

The final article, by Anthony D'Amato, discusses the importance of the interests of the individual when compared with the historical primacy of the state in international law. D'Amato reviews the case of his client (*Frolova*), in which an American woman sought the release of her Russian husband from the Soviet Union. Human rights law, he suggests, has enabled international morality to catch up with international law.

Inter-American Commission on Human Rights Organization of American States

CHRISTINA M. CERNA

Soviet Policies in the Middle East: From World War II to Gorbachev By Galia Golan. New York: Cambridge University Press, 1990. 319p. $44.50 cloth, $14.95 paper.

In the period prior to the collapse of the Soviet Union and the resignation of its leader, Mikhail Gorbachev, Western observers of Soviet policy toward the Middle East were divided into three major groups. The first, represented by the author of this volume, Galia Golan, has asserted that Soviet policy towards the Middle East since World War II has essentially been defensive in nature. The second (which I endorse) has argued that Soviet policy toward the region, at least since the mid-1960s, has been offensive in nature, aimed at eliminating Western (particularly American) influence from the region—unsuccessfully, because of the independence of the states in the region and their ability to manipulate the Soviet Union. Finally, a third group of analysts argued (at least until 1989) not only that the Soviet Union's policy toward the Middle East was offensive in nature but that it had been successful in spreading its influence over the region.

Golan's book analyzing Soviet policy toward the Middle East since World War II closely follows the "defensive" approach reflected in her earlier writings. It is a clearly written, well-argued overview of Soviet policy that despite its lack of notes, would be a useful undergraduate text in Soviet foreign policy or Middle East politics courses. Golan compensates for the lack of notes by a "Guide to Further Reading" for each chapter, as well as a detailed bibliography.

The book is roughly divided between Soviet policy toward the Arab–Israeli conflict (Soviet policy toward Egypt, Syria, the Six-Day War, the 1967–73 interwar period, the Yom Kippur war, the Arab–Israeli conflict, the 1982 Lebanese war, and the Palestinians and the PLO) and the Persian Gulf (Iraq, Iran, Islam, South Yemen, and the Arabian peninsula). It also has a concluding chapter on Gorbachev's Middle East policy current through early 1990.

Throughout the book, there is a clear emphasis on the Soviet Union's need for military bases and facilities as a major motivating factor in its Middle East policy. Thus, the Soviet quest for influence in Egypt in the mid-1960s was due in part to a need for bases for the Soviet Mediterranean fleet, which was deployed against U.S. Polaris missile-carrying submarines. Similarly, Moscow's shift of interest to the Persian Gulf in the 1970s is seen as closely linked to the U.S. deployment of Poseidon and Trident missle-firing submarines in the Indian Ocean. Golan also notes the basic caution with which Moscow approached the Arab–Israeli wars of 1956, 1967, and 1973, asserting, quite correctly, that Soviet threats in all three wars were issued "after the major fighting on the ground was virtually finished and the actual need to implement the threat was most unlikely" (p. 92).

If one starts from a different perspective, however, a number of the author's assertions about Soviet policy become open to question. Thus, Soviet assistance to the Syrian army in its move into Jordan in September 1970 can be seen as a low-risk attempt to overthrow King Hussein of Jordan, then a close ally of the United States. Similarly, the Soviet Union could have profited from its client South Yemen's invasion of North Yemen in 1979, given North Yemen's alignment with the United States at the time. While both of these cases are somewhat ambiguous, Moscow's complicity in violating the July 1970 Egyptian–Israeli ceasefire by sneaking antiaircraft missles up to the canal (an incident Golan skips over) was not. Nor can Soviet military assistance to the Arabs during the Yom Kippur war or its applauding of the Arab oil embargo against the United States be called defensive actions.

One might also raise a few questions about Golan's interpretation of several Middle Eastern events. Thus, Camp David, rather than the Sinai II agreement, should be seen as the "nadir of Soviet–American relations in the Middle East" (p. 101). Similarly, to assert that the United States refused the Soviets even token participation in the Arab–Israeli peace process from 1975 onward (p. 103) overlooks the Soviet–American joint statement of 1 October 1977 in which the two superpowers agreed to reconvene the Geneva Conference by December 1977. Although the joint statement became a dead letter because of Sadat's visit to Jerusalem in November 1977, the fact remains that from July to October 1977 the Carter administration was moving to allow the Soviet Union a role in the peace process; and the 1 October 1977 joint statement was evidence of a change (albeit only a temporary one) in U.S. policy. Finally, one could question Golan's assertion that Israel's invasion of Lebanon in 1982 had "discredited Arafat's diplomatic–political tactics, strengthening the radical, rejectionist forces in the organization (p. 134). In fact, when no Arab state aided the PLO during its seige in Beirut (Syria had agreed to a ceasefire with Israel while the PLO was still fighting) and Arafat was kicked out of Lebanon a second time (in 1983, by the Syrians), he journeyed to Egypt and began a political reconciliation with Egypt and Jordan that led first to the Hussein–Arafat agreement of 1985 and finally to the PLO's agreement to a two-state solution to the Arab–Israeli agreement and recognition of Israel's right to exist in 1988. While Golan argues that Moscow tried to dissuade the PLO leadership from formally declaring itself in favor of the two-state solution (p. 277), in fact, Moscow had long seen a two-state solution as the optimum one for the Palestinian–Israeli

component of the Arab–Israeli conflict; and it had repeatedly urged the PLO leadership to adopt such a position—particularly under Gorbachev, who wanted to eliminate Third World areas of conflict that might endanger U.S.–Soviet relations.

Given the collapse of the Soviet Union and the emergence of the Central Asian Muslim republics as quasi-independent states of the new commonwealth, Golan's analysis of the situation (p. 208) in these republics is of great significance, in light of fears in some Western circles that Islamic fundamentalism might sweep the region: "The Soviet Muslim problem was an ethnic one, especially among Muslims of Turkic origin (who constitute the largest group of Soviet Muslims). . . . This was not a matter of religious revival or fundamentalism, in which Soviet Muslims showed little interest. It was, rather, a matter of national sentiments of which Islam is an historically essential component" (p. 208).

In sum, despite some difference in interpretation, I think Golan's book is a most worthwhile contribution to the literature on Soviet policy in the Middle East.

Baltimore Hebrew University ROBERT O. FREEDMAN

Foreign Policy and Ethnic Interest Groups: American and Canadian Jews Lobby for Israel. By David Howard Goldberg. New York: Greenwood, 1990. 197p. $45.00.

The $36 Billion Bargain: Strategy and Politics in U.S. Assistance to Israel. By A. F. K. Organski. New York: Columbia University Press, 1990. 315p. $30.00.

It has been frequently stated that the influence of the pro-Israel lobby groups on U.S. policy toward Israel and the Middle East, especially the American Israel Public Affairs Committee (AIPAC), has often been so great as to have virtually controlled American policy in this region. However, although the two books reviewed here by Goldberg and Organski utilize different research designs and data, their conclusions are roughly similar in showing that what has passed for common knowledge about this subject has been based on a misreading of the facts and that the AIPAC's influence has never been that great.

Goldberg's study systematically compares two organizations that have attempted to influence governmental policy toward Israel, AIPAC in the United States and the Canada–Israel Committee (CIC) in Canada. He examines the AIPAC's behavior during eight situations (the Yom Kippur War, the Kissinger diplomatic offensive, Carter's diplomatic strategy, antiboycott legislation, the F-15 controversy, the Airborne Warning and Command System sale, Lebanon and the Reagan Plan, and the Palestinian disturbances) and the activities of the CIC in seven (the Yom Kippur War, the PLO in Canada, antiboycott legislation, the initiation of the Jerusalem pledge, the reversal of the embassy imitiative, the Lebanon War, and the Palestinian disturbances) (pp. 6–7). Then, each group's behavior is compared in terms of six factors that assist in explaining to what extent the lobby groups have had an impact on governmental policy: the group's behavioral attributes and organizational characteristics, the structure of the state's foreign policy decision making, the domestic and international political environments, and the policy issue itself. Finally, Goldberg utilizes a crude but servicable scale in order to rate the influence of each lobby group during every situation.

What does the author find? Four of his conclusions will be noted here. First, neither the AIPAC nor the CIC were as influential as often has been stated, but the AIPAC was *more* influential. Second, in order to be influential, an ethnic interest group must organize itself to fit the constraints of the governmental system in which it operates. Third, the American system is more open to ethnic group involvement in foreign policy issues than is Canada's. Finally, the extent to which a national government, especially its top executive, cares deeply about an issue and is determined to see a chosen policy put into effect is critical.

Organski's *$36 Billion Bargain* examines the influence of the pro-Israel lobby (including the AIPAC) on the granting of American aid to Israel. His first part covers four major elements of the American domestic scene: presidential support for aid to Israel, American attitudes regarding Israel, the pro-Israel lobby's effect on the behavior of Congress, and the fit of U.S. policy toward Israel (including aid) to the American government's Middle Eastern policy and overall competition with the Soviet Union.

The second part of Organski's study looks at a number of aspects of international politics that have affected America's assistance to Israel. This portion of the book contains a lengthy discussion of the meaning of the term *assistance*. It also presents data comparing U.S. assistance for Israel to U.S. aid given other countries in the world as well as comparing all forms of U.S. assistance to Israel with same types of assistance for the other countries in the Middle East. It challenges many of the oft-heard statements about one-sided U.S. assistance for Israel.

Organski's findings are interesting and important. For example, he shows that U.S. governments, especially since 1973, have viewed assistance to Israel as clearly being in the interests of American foreign policy in toto and in the Middle East. The author also shows that although the AIPAC is a capable organization, its requests have merely tended to parallel the preferences of U.S. presidents, not determined them. When a president strongly wishes to go against the AIPAC's desired policy, he is readily able to do this and succeed. Furthermore, the author convincingly shows that American assistance to Israel has been an excellen bargain for the United States in that it has been moderate in its extent, well utilized by Israel, and helpful in achieving a number of U.S. foreign policy goals. In few other places, states Organski, has American assistance been better spent.

These excellent studies' widely differing approaches and types of evidence produce fairly similar conclusions about the influence of the pro-Israel lobby in the United States. The pro-Israel lobby, whatever its skills, organizational strength, and finances, succeeds only when its preferred policies also are viewed as in America's interests by many other elements of the U.S. political system. But it furthermore appears to have been in the self-interest of all of the groups involved in making or attempting to influence U.S. foreign policy in the Middle East to create and defend the false image of an exceptionally powerful pro-Israel lobby. Despite the high quality of these books, it is unlikely that they will effectively challenge existing common knowledge regarding the AIPAC's great influence. Nevertheless, these books should be required reading for anyone

interested in the effects of ethnic group influence and especially that of the AIPAC on U.S. foreign policy.

University of Virginia JOSEPH M. SCOLNICK, JR.

Cooperation among Nations: Europe, America, and Non-Tariff Barriers to Trade. By Joseph M. Grieco. Ithaca: Cornell University Press, 1990. 255p. $39.95 cloth, $13.95 paper.

Joseph Grieco abbreviates, synthesizes, and (perhaps) oversimplifies current writing on international politics into two categories identified as *realist* and *neoliberal institutionalist*. He examines how the two schools approach problems of international cooperation, given their shared assumption of anarchy as characterizing relations among states. His analysis finds neoliberals relatively optimistic about prospects of institutional cooperation and mainly concerned with the issue of cheating on agreements but believing that such issues can be solved by arrangements agreed upon by states acting as *rational egoists* (*independent maximizers* to economists).

Realists are described as pessimistic about the prospects of cooperation with regard to both the issues of cheating and concerns (generated by their image of anarchy) that some partner in a relationship may gain greater advantages than others. In this realist analysis, to fall behind some possible future adversary, even while gaining benefits from cooperation, is to risk future domination. Hence, states acting on realistic assumptions are expected to act as *defensive positionalists*, reluctant to enter or honor cooperative arrangements.

The author subjects these two approaches to a logical analysis that concludes that realism is more consistent with the condition of international anarchy as here defined. He takes for granted agreement by both schools that realism both dictates and describes strategies with regard to national defense (p. 12). Then, in the more substantive sections of the work, Grieco examines data on United States–European Community interactions in relation to *six codes* designed to reduce *nontariff barriers* to trade negotiated in the Tokyo Round of (GATT) deliberations. That wealth of carefully explored and outlined data is used to test hypotheses drawn from the two theories. The use of these data on the actions of allied, developed democracies regarding economic arrangements (low politics), rather than data on strategic issues of national security (high politics), is asserted to bias the case in favor of the neoliberal approach.

The conclusion of this empirical test is that neoliberal and realist approaches are equally able to explain positions taken on the four more effective codes but that realism offers a more adequate explanation of actions on the two less effective codes (technical standards–government procurement policies and subsidies in relation to countervailing measures). The topics on which realist theory has the advantage involve strategic issues that were to be excluded from the test (p. 182).

Indeed, nontariff barrier issues generally cut across the artificial bounds of high and low politics. Nontariff barrier issues related to food self-sufficiency and the geographic distribution of industrial capacity still have important security dimensions for nations so recently at peace. If the author were really looking for "cases involving trade (or other economic) relations among the advanced democratic states" to test the theory (p. 14), then something like the United States–Canada trade agreement would have been more appropriate.

The author finesses the apples–oranges problem of examining United States–European Community interactions by asserting that positions taken by the latter coalition were those of its dominant partners. The issue might have been resolved as easily by assuming that the actors were agents of highly pluralistic societies. The author notes that well after the codes have been operating, their economic consequences remain undocumented and "difficult to estimate and, thus, to compare" (p. 158). If that remains the case, it would seem unduly difficult for agents to have anticipated the consequences for their national power positions that realist theory is said to require. By contrast, however, the data discussed make it clear that consequences of a code were obvious for particular economic interests (winegrowers or pasta-makers) in the nations involved. The emerging branch of political economy literature on endogenous protection, such as Magee, Brock, and Young's *Black Hole Tariffs and Endogenous Policy Theory* (1989) strongly calls into question the validity of this realist dichotomy between international and domestic politics if not the entire concept of the unitary state.

Further, there is are implicit presumptions that each accord or potential accord within GATT would necessarily induce freer trade and that any agreement that would promote freer trade would necessarily generate net economic benefits for all GATT signators. Neither of these assumptions is universally accepted. Protection is fungible. Giving up degrees of freedom in areas not extensively used (or exchanging one type of protection for a more politically expedient one) is simple and may look like progress toward freer trade and still lack quantitative impact.

One does not have to be a proponent of strategic trade in the Krugman sense to reject the second assumption. Twenty years ago, a top mainstream trade economist, Harry Johnson, stated, "The case for free trade, frequently asserted with considerable dogmatism in the past, appears in contemporary international trade theory as an extremely qualified proposition, dependent on the maintenance of international monetary stability, on efficient representation of alternative social opportunity costs by money costs and prices in the domestic currency, on the social acceptability of the resulting distribution of income or the adoption of a social policy with regard to income distribution and on the possible need for international income transfers" (quoted in *Journal of Economic Literature* 8(1970): 438–39). Even if there is still agreement on the benefits of free trade, the theory-of-the-second-best states that a partial move toward free trade from a distorted starting point may not be welfare-improving.

Grieco's study concludes with several well-conceived hypotheses for further inquiry but without indicating sets of events in which they can be tested. Given that Europe has long been a cockpit for warring nations and an inspiration for realist theory, it seems relevant to explore the emergence of a European Community able to function as an agent in the negotiations discussed. The problem for realist theory as described here would seem to be to explain not the resistance and obstruction (emphasized in defense of the theory) but such cooperation as did occur.

I would suggest two further events to analyze in light

of the competing theoretical perspectives: the emergence and strengthening of other regional groups and the implications for cooperation among nations in a world increasingly characterized by a concentration of military power and a diffusion of economic power. The United States is willing to enter into a trade agreement with Mexico that is likely to produce larger gains for Mexico at a potential cost for some U.S. social groups in the short run. The reasons relate not just to multinational corporations and cross-border alliances but also to the perceived cost of the alternatives. The United States sees increased access to lower-cost labor as being the only way to avoid further losses in manufacturing competitiveness to Asia and the enlarged European Community. Such forces for cooperation are ruled out in the simple two-country game-theoretic frameworks that underpin both schools. On the second point, realist theory implies that concern with relative gains is strongest among states of moderate power and weakest for either all-powerful or impotent states. Thus, we can imply that the prospects for military collaboration (the actions against Iraq) are improving with the end of the cold war but that prospects for economic cooperation are deteriorating (as typified by the GATT stalemate) in a multipolar economic world.

Grieco has a gift for simplifying and categorizing complex issues. In many instances, useful teaching and learning tools emerge, as well as some testable implications for analysis. Yet the reader must realize that much of the original flavor and some caveats of the original works may be lost in the process. His need to see things in black and white, abstracting from situations and eras, leads to claims more strident than any others in the current debate. For example, "This book attempts to demonstrate that realism is logically superior to neoliberal institutionalism." Again, "If neoliberalism has difficulties in accounting for U.S.–EC code interactions, . . . its shortcomings as an analytical approach will have been highlighted. . . . Hence the [nontariff barrier] code experience may approach the status of a crucial experiment" (pp. 11, 14). Contrast this with a statement like the following, from Gilpin's *Political Economy of International Relations*: "The clash between liberals and nationalists over the benefits and costs of specialization, although partially based on differing economic and political objectives, also rests on differing assumptions regarding the nature of international economic relations. Liberals consider these relations to be essentially harmonious; nationalists believe that conflict is inevitable. . . . Neither assumption is valid in itself. Rather, its validity rests on the larger configuration of global economic and political conditions at a particular time" (1987, p. 188). It is encouraging to see that some realist scholars see the world as large enough to hold a multiplicity of ideas and too round to be fully comprehended from a single point of view.

Institute for Economic Development and Policy ROBERT K. MCCLEERY

Artificial Intelligence and International Politics. Edited by Valerie M. Hudson. Boulder: Westview, 1991. 422p. $55.00.

This anthology is a valiant effort to make sense of a yet immature branch of political science. For this reason, the book is well worth reading. It provides little in the way of research findings, nor is it a coherent collection of essays. But it is glimpse into a discipline undergoing change, a tribute to the interdisciplinary nature of political research, and a case study in its own right about the emergence of new tools and new perspectives for the scientific study of things political.

I will focus on four questions: Why is the application of artificial intelligence to politics an immature science? What does it offer to the study of the political human being? What are its basic tools? And why is so much artificial intelligence work focused on international politics?

Most of the essays collected in this volume draw upon the vocabulary of artificial intelligence, and most are examples of computational modeling. These efforts are collectively immature in the sense that those using the vocabulary and tools of artificial intelligence arguably know more—but do only marginally better—than those without that vocabulary and set of tools. Future work will certainly change this calculus for the better.

There are other signs of the field's immaturity. The articles present several implicit or explicit typologies for categorizing differences within the computational modeling community, yet none seems to capture or bring together the diversity found in these pages. Perhaps the most useful distinction made in these pages is also the simplest, the oldest, and the least specific to artificial intelligence modeling, namely, the difference between static and dynamic models—the latter having some capacity for in-process "adaptation" and "learning."

The most telltale sign of the field's immaturity (as reflected in the book) is the authors' apparent need to recommend a domain of inquiry, a method of inquiry, and findings from that inquiry. Many, although not all, of these articles seek to recast the political human being as an information processor. Many simultaneously seek to demonstrate how computers and higher-level structured languages can be used to model their revised political human being. Some articles even conclude with a set of findings that are to add to the discipline's cumulated wisdom. This triple exercise would be roughly akin to a behavioralist simultaneously recommending political psychology, explaining the use of SPSS or SAS in social research, and then listing what others should treat as revealed truth. It is an immature science whose domain of inquiry and tools of inquiry are at least as problematic as its sparse findings.

What about its domain of inquiry? Three recurring themes about the political human being as an information processor emerge: the human being is a good information storage system, at best a modest information retrieval system, and a poor calculator. The latter, of course, pits these computational modelers against their rational choice cousins.

What follows from these fundamental themes is more questionable. If the end of political science is to understand the political human being's limitations, then attention to the oft-mentioned standards of process validity (looks like the real thing) are as important as replicating the political choices flowing from those processes. But if the end of political science is to help the political human being transcend human limitations, then replicating human folly is important only as a starting point for its improvement. There are other forms of myopia in these pages. In an article with admittedly no focus on process validity, Schrodt argues that his project "seeks to apply

pattern recognition in a world of behaviors produced by pattern recognizers" (p. 187). Surely, we can all identify a few pattern recognizers who, rather than being condemned to repeating event sequences, seek to change those sequences. The article does, in any event, review an algorithm for processing event sequences in order to discriminate between war and nonwar crises.

What are the basic tools of artificial intelligence? It is not the use of computers, per se. Nor is it the use of high-level structured languages such as LISP, which in turn might be written in decidedly low-level languages like C and Assembler. Together, computers and appropriate programming languages do permit the researcher to create a technological instance (instantiation, if you will) of various images of human information processers and the work that they do. As noted, these same tools can also be used to add strengths where humans fall short, particularly in strategic optimization.

The volume contains an example of an extraordinary use of computers and computational languages. Mallery's article on semantic content analysis is as interesting as his reliance on jargon is unfortunate. Beneath discussions of eidetic representation, lexicalist semantics, sentential constraints, and so on lies an imaginative computer program for processing text files into semantic structures and passing these through classification systems such as Lasswell and Namenwirth's value dictionary. The program has a growing library of classification schemes; but these classification schemes are provided to, and not produced by, the program. Still, a considerable amount of work remains before RELATUS even comes close to realizing its "creativity-respecting foundations for the textually oriented social sciences of the future" (p. 119).

Finally, why has so much artificial intelligence and computational modeling so far been concentrated in the field of international politics? Not by accident did the editor choose to focus on that subfield, for remarkably little artificial intelligence work exits outside of it. In part, this concentration flows from the subject matter of international politics and its previous methodological traditions. Quantitative international politics has long included dynamic mathematical modeling, and the art of modeling forms a common bridge between the older mathematical, and the newer computational, endeavors. The subject matter of international politics also lends itself more immediately to this nascent science. Plato sought to understand the just individual by looking at the just state; and the study of the political human being as an information processor is easier in the "reconstructed logics" of foreign policy than in the average human being's "logic-in-use."

All told, artificial intelligence in the study of international politics is not a fad. As the paradigm takes shape, its conceptual and methodological moorings will become less problematic, and the work it can do will become more compelling.

University of Illinois, Urbana–Champaign STEVEN THOMAS SEITZ

Paradoxes of War: On the Art of National Self-Entrapment. By Zeev Maoz. Boston: Unwin Hyman, 1990. 368p. $39.95.

This book approaches the causes, management, and implications of international war as a series of related paradoxes. To set the stage for his approach, Maoz convincingly argues that war is usually treated with linear logic that, while useful, cannot accommodate many nonlinear aspects of the conflict process. The purpose of his work is to explore the "paradoxical" nature of war. When conflict is viewed as a set of unraveling paradoxes, Maoz contends, many nonobvious implications hidden in the record come forth with clarity. Maoz's theoretical objective is to replicate the process used by Kenneth Arrow to unravel the paradox of voting proposed by Condorcet, which dictated the research agenda in the field of voting and helped uncover major insights never before suspected.

Maoz develops several theoretical handles to generate war paradoxes and to account for misunderstood loops that emerge before, during, or after war. The analysis of contradictions found in wars are derived using rational and cognitive perspectives. Maoz argues, with some validity, that the merger of rational and cognitive approaches provide competing yet complementary perspectives that, combined, can account for the wages of war. From a rational perspective, we are told, paradoxes occur because actors do not control the rules of the game or the choices of other actors. From the cognitive perspective, paradoxes arise because imperfect understanding of conditions lead actors to behave suboptimally. While such differences may be real, one would have liked to see (at least for each paradox) an integration of perspectives. For example, the cognitive logic that guides the behavior of an individual descending a tree who anticipates that branches in the next sequence may break, can also be effectively represented by sequential game theory (as shown by the recent work of Robert Powell, Bruce Bueno de Mesquita and David Lalman). To provide a coherent theoretical perspective, it would be useful to distinguish the case in which the two approaches produce similar results from that in which they differ substantively so that much can be learned by employing both. Maoz does not provide such an integration. Instead, both perspectives are elaborated side by side throughout the book, leaving the reader to decide when results are contradictory, mutually exclusive, or complementary. This detracts somewhat from the otherwise very clear presentation of paradoxes on war, since some confusion is generated when very complex behavior is given a dual explanation.

Maoz argues that paradoxes do not result from deliberate behavior but emerge as the unexpected, unintended consequence of actions that lead to paradoxical loops and force elites to adopt ever more undesirable positions in conflict. Consider how such paradoxes and their sequencing affect the study of war. Maoz shows that actors with superior capabilities do not always emerge victorious in the power paradox. He draws on quantitative and nonquantitative research alike to demonstrate the lack of consistent explanation for the failure of the United States to win in Korea when it held a near monopoly of nuclear power, as well as the failures of the United States in Vietnam, the Soviet Union in Afghanistan, China in Vietnam, and Israel in Lebanon to bring a successful conclusion to each conflict when each held nuclear preponderance over opponents (p. 232). There is a link among paradoxes. When the paradox of power applies, one frequently encounters the sunk cost paradox that delayed American exit from Vietnam and Israel's withdrawal from Lebanon long after the original goals in such war could not be achieved (p. 283).

Moreover, the loser's paradox reinforces the effects of the prior two. Maoz shows that "Phoenix factor" effects after major wars reemerges in the Middle East where Jordan (the loser) benefits substantially from defeat and partition while Israel (the winner) does not. Maoz informally relates paradoxes to each other and to linear findings picturing the process of war but does not elaborate a theoretical framework to account for these sequences.

After reading a book so full of insights, one seeks an overarching structure to simplify the study of war. Such a structure is not forthcoming, yet the foundations for a nonlinear analysis of war are clearly delineated. Maoz contributes a new, systematic perspective on war that suggests much promise. This is a solid contribution incorporating new theoretical insights in a systematic attempt to answer the key unobvious questions surrounding the paradoxical process that leads to the waging of wars. The drawback is that having persuaded the reader that paradoxes do exist, Maoz does not provide a theoretical or practical handle to identify when such paradoxes will emerge, what consequences they will have, how much will they distort expected results, and (most importantly) how policymakers can handle these unexpected departures that lead to consequences undesired by all.

Maoz must be commended for opening an important, seldom-traveled avenue with enormous promise for understanding the process of war. On balance, Maoz has made a significant contribution to the study of war. This clearly written, concisely argued, and effectively documented record of unusual paradoxes associated with the waging of war opens new avenues of inquiry and adds sensitivity to the treatment of this complex process. The linkages between and among paradoxes and linear explanations are informal, leaving much to be explored. Analysts interested in international war will find in this book a rich vein of insights whose interconnections will no doubt be explored for some time to come by graduate students concerned with war. For these reasons, I recommended this volume for middle-level courses on conflict and consider the book required reading for graduate courses on conflict.

Vanderbilt University JACEK KUGLER

As Moscow Sees Us: American Politics and Society in the Soviet Mindset. By Richard M. Mills. New York: Oxford University Press, 1990. 308p. $29.95.

The publication of the Mills's book at this time gives rise to a fundamental question. In the face of the intellectual revolution unleashed by Gorbachev, what do Soviet Amerikanisty (Americanologists)—whether hard-nosed conservative or emancipated liberal—reveal about the way "Moscow sees us"? Mills spells out the main features of what he refers to as the Soviet intellectual *mindset* regarding American politics and society. Soviet Amerikanisty, he argues, remain wedded to a basically Marxist view of the United States. He starts with a discussion of "state monopoly capitalism," examining the relationship between economic classes and political institutions, that is, how the distinctive relations between the "rulers and the ruled" affect U.S. politics, the presidency, elections, political parties, and the Congress. With increased intellectual and political contact with the outside world and greater familiarity with American political science, he correctly argues, Soviet analyses have become analytically more sophisticated. Thus, unlike traditionally oriented Soviet writers, current observers are willing to examine previously unintelligible features of U.S. political life, such as the weakness of the American Left and the virtual irrelevance of the Communist Party of the United States of America (CPUSA).

Accepting as a given his analysis of the increased Soviet understanding of the American political scene, Mills is most arresting when he observes, "No substantial changes have taken place in the way the Soviets view American domestic society." The persistence of these views is puzzling. Since the mid-1980s, the class analysis discussed by Mills has come under serious attack in Soviet academic circles. The repudiation of Marxism by the reformist intelligentsia has led to the wholesale rejection of Soviet textbooks and the abandonment of traditional interpretations of Soviet history. The Gorbachev leadership itself has been highly critical of conservative ideological orthodoxies and dismissed their relevance on numerous public policy issues, including foreign policy. Finally, as a backwash to the failed coup last August, the communist worldview lost whatever remained of its intellectual authority Mills could not have anticipated the most recent developments; however, the trend has been there since the introduction of glasnost in 1986.

Why do such views persist? Of what consequence are they? How do they influence policy? Most studies assume policy relevance. "Intercultural misperceptions," argues Mills, have been a serious barrier to improved relations. Reality, I would argue, suggests otherwise. In actual fact, the mindset described by Mills has been of marginal significance. The improvement in U.S.–Soviet relations since 1986 has been shaped more by Gorbachev's anti-ideological "new political thinking" than by an assessment of Presidents Reagan and Bush as "instruments" of the American political–economic elite.

Judgments regarding the relevance of the perceptions apparently still held by the Amerikanisty must be qualified by the fact that they have been rather easily set aside. The author himself notes that the Soviets have been eagerly studying the working of American bourgeois democracy. In their efforts to set up democratic institutions of their own, contingents of Soviet officials have for several years now been visiting the United States in ever-growing numbers seeking guidance as to how to organize their new political structures—legislatures, political parties, public opinion polling, etc. They have inquired about the process of recruiting congressional staffers, the functions of the Library of Congress, the efficacy of U.S. antitrust legislation, effective procedures for legislative monitoring of the defense budget, and so on. Well before the coup, a Soviet security official was quoted in the press as suggesting that his colleagues take part in exchanges with U.S. police agencies to study how to manage mass demonstrations without violence.

However critical Soviet analysts have been in their views of bourgeois political institutions and practices, Moscow is eager to learn from the experience of the United States. Soviet academic analyses of American politics and society, it would seem, have been of marginal consequence to the Gorbachev leadership. Needless to add, orthodox judgments regarding the class

essence of American "monopoly capitalism" have become irrelevant in the face of Soviet economic collapse. This was vividly brought home by none other than the erstwhile general Secretary of the CPSU last September in his response to a proposal by Treasury Secretary Brady to create a special training center for "capitalists" in Moscow, whose graduates would be trained to help put the Soviet Union's centralized, planned economy on a free-market basis. According to press accounts, Gorbachev responded enthusiastically to the Brady proposal, offering use of the recently vacated building of the party's Central Committee for this purpose. Clearly, the risk of political domination by the bourgeosie and exploitation of the working class are a small price to pay for an economy that works.

One problem is that Mills takes his source material too literally. The writings he has analyzed more often tell us about the ideological environment that has until recently dominated the Soviet scene than provide authentic intellectual judgements regarding how Moscow sees us. Mills hints at this in his discussion of Aleksandr Yakovlev's *On the Edge of an Abyss* published in 1985. Yakovlev's presentation, which embodies a bitter denunciation of postwar U.S. foreign policy, caused some consternation in U.S. policy circles at the time, given the author's prominence as an advisor to Gorbachev. The book, Mills suggests, may be a "smokescreen." Quite so. Yakovlev may not, in fact, look kindly on the U.S.; but it was apparently to deflect attention from his real political agenda, that is, radical political reform, that he felt obliged to adopt the rhetoric of the most orthodox critics of the United States. With the political collapse of the party theocracy and its legitimizing mythology, we can anticipate a respite from such ideological contortions and, hopefully, a less crippled approach to U.S. politics and society.

U.S. Department of State MORTON SCHWARTZ

Foreign Policy under Carter: Testing Multiple Advocacy Decision Making. By Alexander Moens. Boulder: Westview, 1990. 201p. $29.00 paper.

To those familiar with Alexander George's *multiple advocacy* model of decision making, declaring the Carter administration as a test case may seem an oxymoron. The central role George envisioned for an "honest broker" (preferably the national security adviser) to safeguard an open and balanced policy process contravenes the common interpretation of Carter's national security advisor Zbigniew Brzezinski as a vociferous advocate who sometimes skewed the policy process to favor his own views.

Alexander Moens challenges this conventional wisdom and argues that Brzezinski was in fact an honest broker for the first eighteen months of the Carter administration. In Moens's view, it was only the failure of Secretary of State Vance, Secretary of Defense Brown, and others to offer a wide range of policy options that pushed Brzezinski from broker to advocate in the second half of Carter's presidency.

Moens bases this view on case studies of Carter administration decision making on four issues: "deep cuts" in SALT II in early 1977, the Ogaden War in 1977–78, normalization of U.S. relations with China in 1978, and the fall of the shah of Iran in 1978 and 1979. He argues that these cases trace Brzezinski's evolution from "reasonably neutral management" of SALT, to a precarious combination of advocacy and brokerage on the Ogaden War, and finally to outright advocacy and circumvention of an open policy process on China and Iran. The reason for this transformation, Moens maintains, was that a consensus among top officials presented Carter with an excessively narrow range of options, with which Brzezinski increasingly disagreed. In particular, with no other strong advocate of a harder line toward the Soviet Union, Brzezinski inevitably slipped into an advocacy role that suited his ideology and temperament.

Moens's provocative interpretation is made more persuasive by his effective use of secondary sources and interviews with key officials, including Brzezinski and Brown. Unfortunately, he did not interview Carter or Vance. Thus, his criticisms of Vance and qualified praise for Brzezinski require caution. Moens also lacked the internal documents that would make or break his case. It is unclear, for example, whether Brzezinski's daily intelligence briefing to Carter or his cover memos on materials going to Carter constituted advocacy and compromised his ability to be an honest broker from the start or not.

This book nonetheless provides a useful addition to histories of the Carter administration and the writings of Alexander George, David Hall, and Roger Porter on the multiple advocacy model. Moens reaffirms these authors' views on the tension between the roles of broker and advocate; and he extends them in arguing that even the most honest broker's attempts to bolster underrepresented views will slip into disruptive advocacy. He thus disagrees with suggestions that the broker can rebalance the process by direct advocacy or by bringing in outside advocates. This conclusion may be too categorical, given the constructive role of the "wise men" who advised Johnson to deescalate the Vietnam war, as well as the similar role that members of the Congress, the media, and public interest groups have served in advocating underrepresented views to the president.

To a greater extent than most analysts, Moens presents the multiple advocacy model as the best of all possible worlds, though he is well aware that it can only work if it suits the management styles of the president and his top advisers. He deliberately sets aside the question whether mistaken perceptions led to flawed decisions that no process could have corrected; and apart from a few brief references to the Reagan and Bush administrations, he makes little attempt to update his thesis. In these issues, one could either commend Moens for not getting distracted or criticize him for key omissions. He argues, for example, that it is all but impossible for the national security advisor to be a broker without becoming an advocate, but he does not comment on whether current National Security Advisor Brent Scowcroft has or has not succeeded at this.

Moens's final prescription deserves careful and critical consideration. Because he sees the roles of broker and advocate as incompatible, he recommends designating the national security advisor solely as an advocate and "roving adviser" without primary responsibility for chairing committees, managing crises, or implementing policies. As for the broker role, he calls for reviving the position of executive secretary of the National Security Council. The danger is that a "roving advisor" could become a loose cannon, and relations with the executive secretary could become strained. Would the secretary

really reign in the NSC? Would both players have enough status and staff to serve their roles? On these and other issues, Moens provides a needed challenge to accepted views; but until more documents and insiders' details become available on the Carter administration and scholars scrutinize the apparent successes of the Bush administration's decision processes, the jury must remain out.

Georgetown University ANDREW BENNETT

Friends but No Allies: Economic Liberalism and the Law of Nations. By Stephen C. Neff. New York: Columbia University Press, 1990. 345p. $37.50.

This book presents a history of the international law of commerce. Neff builds his narrative around three "experiments" in free trade that produced successively more liberal commercial law: the nineteenth century, the twentieth-century interwar period, and the postwar world. He also provides more detailed discussion of three contemporary topics: international law on economic warfare, economic nationalism in the Third World, and Third World efforts to construct a more "social democratic" international order. The narrative is clear and well written, and Neff connects legal developments to their economic and political contexts. While focusing on legal issues, he presents a good overview of the economic and political developments of each period.

Neff argues that the results of each experiment depended on the struggle between the forces of free trade and those of economic nationalism. He treats these forces as ideas, sets of legal theories. Although he clearly ties these ideas to their political and social contexts, his focus is nevertheless very different from the more common interpretation that these ideas are smokescreens for material interests that gain or lose from free trade. Largely because the analytical approach remains implicit, he never justifies this analytical choice.

One reason he does not justify this choice is that his approach is atheoretical. Although familiar with the theoretical literature in political science and related fields, he chooses not to contribute to it. In a book spanning several centuries, this leaves much of the narrative hanging in air, without clear causal analysis. For instance, Neff stresses the personal influence of Cordell Hull over American trade policy in the 1930s and 1940s. Without denying Hull's role, I would have liked Neff to explain how a man such as Hull comes to power and remains there: Is it Hull's genius, or does he reflect a certain economic interest or political coalition? Neff's disinterest in theory and relative inattention to causal analysis means that we do not learn the answers to such questions.

His lack of theory is also reflected in a sometimes weak knowledge of the economic issues involved in the subjects he studies. For instance, he makes the repeated claim that nonindustrial countries benefit less (or perhaps not at all) from international trade. Clearly, this flies in the face of the classical law of comparative advantage. While there exist theoretical positions Neff might use to make his case, whether contemporary strategic trade theory or traditional dependency theory, Neff does not provide even a preliminary justification for his position.

This is problematic because Neff's normative position would appear to be close to that of the classical liberals whose positive theory he is unwilling to accept in its entirety. In fact, the book would be stronger throughout if Neff had presented his normative position explicitly. He clearly sides with the liberals, but we never find out why.

Its limits notwithstanding, *Friends But Not Allies* will familiarize political scientists to a legal perspective on the international political economy that they may not frequently see. Neff writes at a level appropriate for advanced undergraduates, and the book would be useful for courses desiring a legal angle on international economic relations.

University of Rochester ROBERT PAHRE

War, Peace, Survival: Global Politics and Conceptual Synthesis. By Robert C. North. Boulder: Westview, 1990. 298p. $48.50 cloth, $17.95 paper.

What causes war? In his past work Robert North (and Nazli Choucri) have typically identified "lateral pressure" as the cause of imperial and territorial expansion among pre–World War I European states. In that period—and perhaps even today—a country's "lateral pressure" upon other states is apparently directly correlated with its power. This does not mean, however, that nations expand wherever and whenever their power permits. Uncertainty and the "power paradox" (the greater power a nation acquires, the more insecure it may often become) may dampen such outcomes. Still, North's views are like those of the "power transition" school in that intersections (crossing points, collisions) of interest and power with other states may well cause war.

The present work, however, modifies and extends North's previous findings. In particular he contends that beta countries are likely to be expansionist. This conclusion requires some explanation. North breaks countries down into categories, reflecting their population and economic growth, technology, per capita wealth, territory, and resources. Alpha countries are those which possess high technology, a relatively stable population, large stocks of resources, a high per capita gross national product, and a relatively large territory, such as the United States and (until recently) the Soviet Union. Beta countries, in contrast, are characterized by population pressure (large population relative to the size of the territory), increasing technology but inadequate resources relative to population, and a high per capita gross national product. North lists Great Britain, France, and the Netherlands as countries that were in the beta stage when they undertook their overseas colonial expansion; Germany and Japan assumed a beta mantle before World War I and again prior to World War II. North writes, "A society with this profile is under continuous pressure to expand its trade, or if that recourse is blocked in one way or another, to expand its territory" (p. 127). Gamma countries are those like contemporary Japan, Germany, France, and Britain, that is, high-technology countries that offset any modest population pressure by a growth in technology, economics, and trade. Delta nations are exemplified by the Nordic and small countries of Western Europe, which face no population pressures and have ample technology. Epsilon countries are typical population-pressed LDCs like Bangladesh, India, and China. They are positioned to be challengers like betas but are too poor (low per capita gross national product) to go to war. Zetas are nations with little population pressure (and

therefore no temptation to expand) and are very poor (e.g., Chad and Bolivia). Finally Etas are countries with little population pressure, developing technology, high resources, and a high per capita gross national product. The oil countries of the Middle East are representative of this category.

If we take this depiction seriously, a theory of war is clearly implied. The countries that would be expected to be most peaceful would be the delta group, in which population pressure is low or nonexistent, technology scarcely needs to compensate for it, and trade is at a high level, providing an alternative outlet for any other expansionist demands. From North's point of view, it is perhaps not surprising that these are the states identified by John Mueller as early converts to the peaceful camp. Gamma countries are somewhere between deltas and betas: they house a large population within a confined territory and only sublimate their potentially expansionist drives through foreign trade and high-technology growth inside the state. Any major economic setback, such as domestic depression or a major cut in international trade, would pose questions for their peaceful orientations. Epsilon countries will also become potential expanders when their growing wealth makes this possible. The challenge for the world is to neutralize this tendency through further domestic high-technology growth and expanding trade. Another interesting implication is that foreign countries may wish to reduce population pressure in epsilon nations by taking immigrants from them, as the United States has done in recent years. In the nineteenth century—perhaps from this point of view—the then population-pressed European states exported their excess citizenry to the open spaces of the New World. In part, North is aware of these implications and certainly recognizes that world population growth presents a formidable problem when it is not accompanied by offsetting technological development. He does not, however, seize upon the many policy-relevant implications of his analysis.

Is the analysis correct? He may have identified one strand of theory that seeks to explain conflict, but he misses many others. The realm of ideas and ruling intellectual conventions that contribute to either peace or war are left unexplored. But national metaphors are potent forces. The idea of liberalization and democracy is now moving to the fore and is having a great effect, even upon the frequency of war (see the work of Michael Doyle). Nations that conceive their role to be that of trading states behave differently from those of a military orientation. In the past, military and biological metaphors foisted erroneous "expand or die" notions upon late-nineteenth-century European states. North also misses the stabilizing role of nuclear weapons as a restraint upon conflict. He seems to think that weapons of mass destruction only exacerbate frictions among nations.

Finally, his analysis is ultimately inconsistent in theory itself. If the factors that make for war and expansion ("lateral pressure") are physical and economic ones, it is difficult to believe that new conflict resolution techniques can deal effectively with them. Only a cessation of those pressures permits peace and survival. In the final section of his work, North introduces a fourth, or global, level of international interaction. Instead of fruitfully developing this, however, he unfortunately goes on to make recourse to many of the same factors that he considered under the third, or international, level.

Ultimately, the work contains some intriguing ideas that might have been carried further in specific cases but disappoints in its overall theoretical presentation.

University of California, Los Angeles — RICHARD ROSECRANCE

Who's at the Helm? Lessons of Lebanon. By Raymond Tanter. Boulder: Westview, 1990. 262p. $24.95.

Raymond Tanter, an experienced academic observer of Middle East affairs, has now written an insider's account of U.S. foreign policy decision making during the Lebanon crisis of 1981–83. A professor of political science at the University of Michigan and senior staff member on the National Security Council from 1981 to 1982, he benefits from his perspective as an academic, as well as a participant in the policy process. His study exhibits both the strengths and weaknesses—the insights, as well as the strains—produced by that mingled perspective.

Tanter sets out to answer three main questions: Which officials should regulate political and military affairs? Why do leaders make decisions to combine or not to combine political and military actions? What is the impact of crisis on process and choice? (p. 2). His focus is on interagency politics, on the struggle for control of Mideast policy in a Reagan White House depicted (in a nautical metaphor that pervades the book) as "adrift." Because the president preferred a hands-off policy management style, we are told, no one was "at the helm"—or rather, there was a bitter struggle over who would take the helm between *globalists* and *regionalists* (pp. 19–23). Indeed, there is a strong impression that matters can be simplified even further, to the Manichaean competition between two men: globalist Secretary of State Alexander Haig and regionalist Deputy National Security Advisor Robert McFarlane, representing good and evil, respectively. McFarlane comes in for sustained and severe criticism for mismanaging "the intelligence and policy process for his own bureaucratic ends and to the detriment of national security" (p. 4).

Thus, the answer to the question who should regulate political and military affairs is that the president and his principal foreign policy advisors, the secretaries of state and defense, should. This is sensible enough, but his suggestion that the national security advisor "should operate as an honest broker not as an independent policy formulator" (p. 226) is problematic at best. That office has been a major locus of foreign policy formulation since it was occupied by Henry Kissinger. Moreover, the heavy criticism of McFarlane's performance may, with cause, be dismissed by some as merely a judgment on personality, not of the office or its role in the policy process per se.

Tanter's observations of real decisions are often keen and insightful and advance understanding of the question about combining political and military actions at least in the specific context of the Lebanon crisis. But his overall conclusion—that presidents should strive to be more aware of "how difficult it is to combine diplomacy and force" (p. 226)—while unarguable, is also no more than a truism. The answer to the question on the impact of crisis is also his best-demonstrated general conclusion. As a result of Reagan's failure to lead his policy team, Tanter argues, strategic opportunities were missed vis-à-vis both the Soviet Union and the Arab–

Israeli conflict. No advantage was taken of Israel's invasion of Lebanon to enhance regional security or push back the reach of Soviet influence. Tanter notes (agreeing with other literature on crisis decision making) that a primary effect of a crisis on the decision-making process is to permit the White House to lead in a way not normally possible. (There is less low-level bureaucratic resistance to presidential leadership in a crisis.) His criticism of Reagan's failure to seize the opportunities presented by Lebanon thus carries considerable weight. And he goes further, asserting that Lebanon was a precursor to the Iran–Contra affair. As he puts it, "If Lebanon represents a misuse of the interagency process, then the Iran episode indicates a disuse of that process. . . . The legacy of Lebanon is Iran" (p. 2).

Readers familiar with the by-now-significant body of literature on the U.S. foreign policy decision-making process will find much that is familiar in this book; but beyond yet another "case study" example, they will not find much that is new. Specialists in Mideast affairs, on the other hand, should welcome the book as a valuable compendium (much of it based on insider interviews and personal observation) of the Reagan administration's actions and decisions during the Lebanon crisis. That should hold true despite the book's opaque prose style and occasional flights of purple fancy, like "[Secretary Haig] had outfitted the entire sixth floor of State in his ideological attire. But, upon [his] abrupt departure, they shed this global garb in favor of tighter fitting, regional raiment" (p. 36). Overall, the book will be of use to specialists, since it is rich in detail and anecdote if at times cluttered, as well.

University of British Columbia CATHAL J. NOLAN

Reserve Forces and the British Territorial Army: A Case Study for NATO in the 1990s. By Wallace Earl Walker. London: Tri-Service, 1990. 190p. $45.00 cloth, $20.00 paper.

Civilian-based Defense: A Post-military Weapons System. By Gene Sharp. Princeton: Princeton University Press, 1990. 152p. $19.95.

The effective end of the cold war and its military edifice in the form of the NATO–Warsaw Pact military confrontation in Europe has had numerous effects on both the international system and the domestic politics of the former competitors. With the rationale for large, standing forces-in-being in Europe to blunt a Soviet surprise invasion of Germany now effectively dissolved by events, the size and shape of standing military forces in the future has become a lively topic of debate.

It is a debate that encompasses almost all the NATO countries. The economic burden of maintaining large active forces has created considerable pressure to reduce the size and expense of forces in the future to allow the redirection of resources to other priorities. The question—hotly debated both within and outside the services—is how to reduce forces without critically compromising the ability to ensure the national security of member states of the alliance and beyond.

These two books stand at opposite ends of the periphery of that debate. The Walker book, a case study of the experience of the British Territorial Army—Britain's primary reserve force—attempts to extrapolate the British experience to other NATO members. Its primary purpose is to raise, and attempt to answer, the question, How much and what kind of role can reserve forces play in the future? Since active forces are diminished in size

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and the temptation to rely more on the reserve component builds in a number of countries (most prominently, the United States), it is a germane question.

The Sharp book stands in rather vivid contrast at the far end of the spectrum. More a polemic and advocacy than a work of scholarship in the traditional sense, it argues for a process that would eventually eliminate military forces and replace them with a system of "civilian-based" action where defense against military aggression would be accomplished through such peaceful means as passive resistance.

In the short run of advising policymakers, the Walker volume is clearly the more useful. The recently concluded Desert Storm operation, in which American reserve forces were mobilized and committed for the first time since the Korean War, has raised some serious organizational, military, and political questions that must be answered for a future in which reductions in active component forces means that any large operation will require an enhanced level of reserve participation.

The British experience with the Territorial Army is parallel and instructive to the American. The Territorial Army has been looked down upon by active forces, has been involved in political wrangles over role, mission, and even levels of funding, equipment, and training, and these are all themes that recurred during Desert Storm. The comparative lessons of the Americans and the Territorial Army may prove of some value for dealing with the future.

The Walker book can be read on two distinct levels. First, it can be read as a study on the problems of reserve forces and their uses in war. Walker, in this regard, provides interesting and provocative insights into such practical problems as the deployment and mobilization of reserves and their contribution to the overall combat power of an armed force. In this respect, he challenges some assumptions made within the American military establishment. Second, the book can be read as a study of organizational behavior, including a strong discussion of bureaucratic and grassroots politics. It is hard to disagree with Walker's conclusion that the reserve forces issue is "a rich and unexplored territory" for future study.

The Sharp book is totally unlike the Walker volume. Undoubtedly influenced heavily by Richard Falk's view of conflict and war, Sharp sets out to explore, and provide a framework within which to implement, his idea of civilian-based defense. The core is that ordinary citizens, by simple acts of nonviolent resistance, can act as an effective alternative to traditional military sources. He cites numerous instances in the past of how this has been accomplished; and he would, I suspect, argue that the successful resistance of the Soviet population to the August 1991 attempted coup represents the kind of action he advocates.

The weapons that Sharp argues can be effective are not traditional arms but, instead, psychological, political, and economic weapons, achieved through a process of what he calls *transarmament*.

Sharp is entirely up front in his assertions. In the preface he states the two premises on which his thesis resides: "that civilian-based defense policies against internal takeovers and foreign aggressions can be developed, and that dictatorships and oppression can be prevented and disintegrated by the capacity to wage powerful nonviolent struggle." If one accepts, or is at least receptive to, these premises, Sharp's argument and plan may be convincing. If one is skeptical of the premises, however, there is little here that will cause a conversion.

University of Alabama DONALD M. SNOW

WINNER OF THE CHILD MAGAZINE BOOK AWARD FOR EXCELLENCE IN FAMILY ISSUES

WHEN THE BOUGH BREAKS

THE COST OF NEGLECTING OUR CHILDREN

SYLVIA ANN HEWLETT

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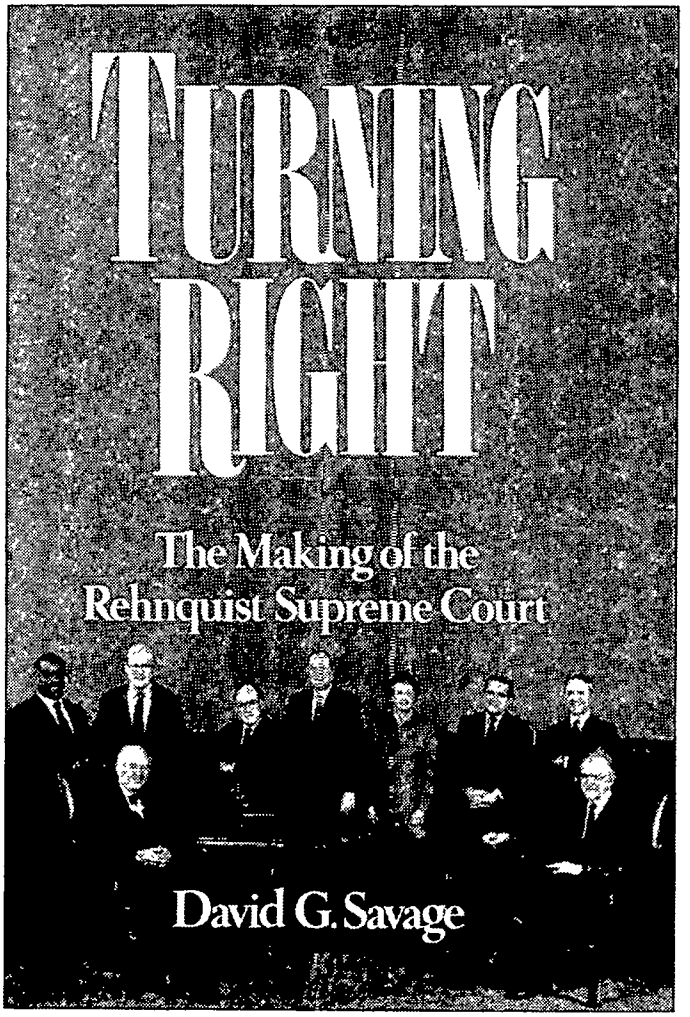
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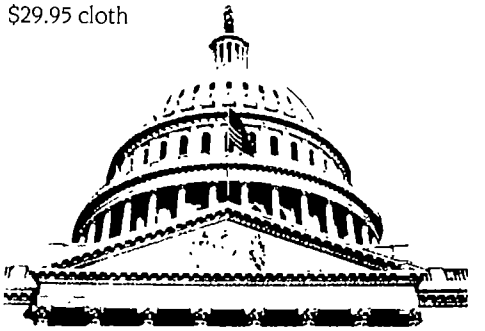
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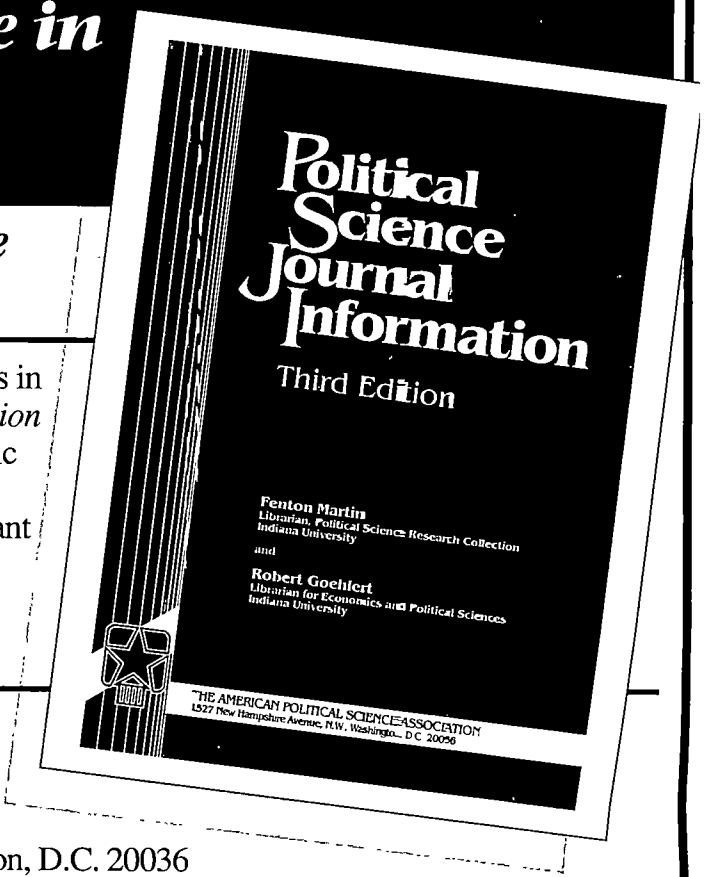
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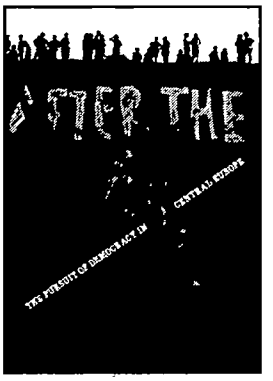
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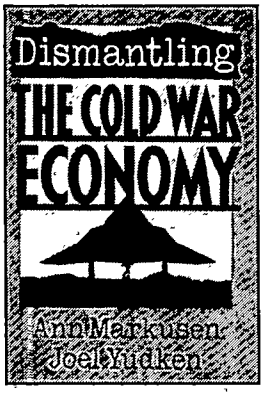
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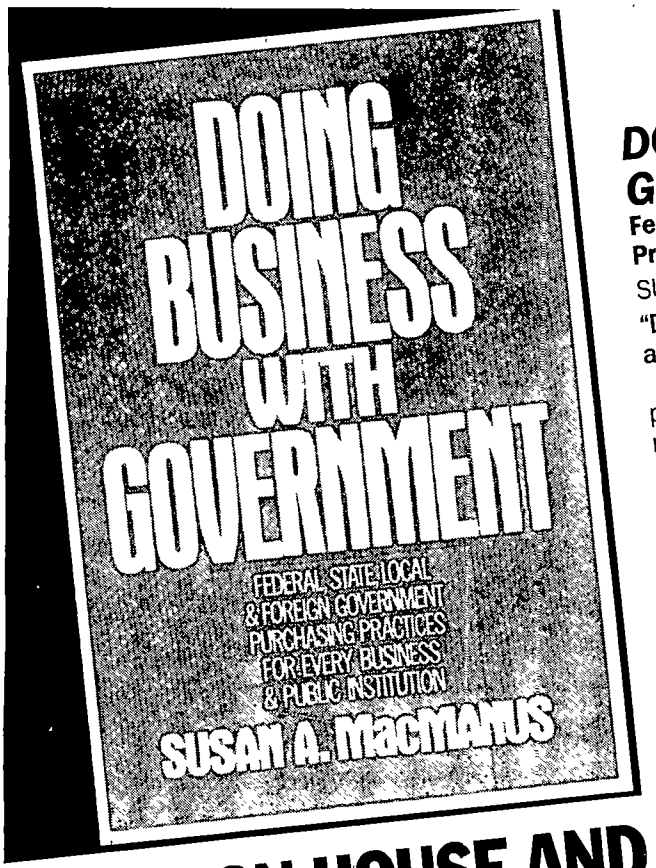
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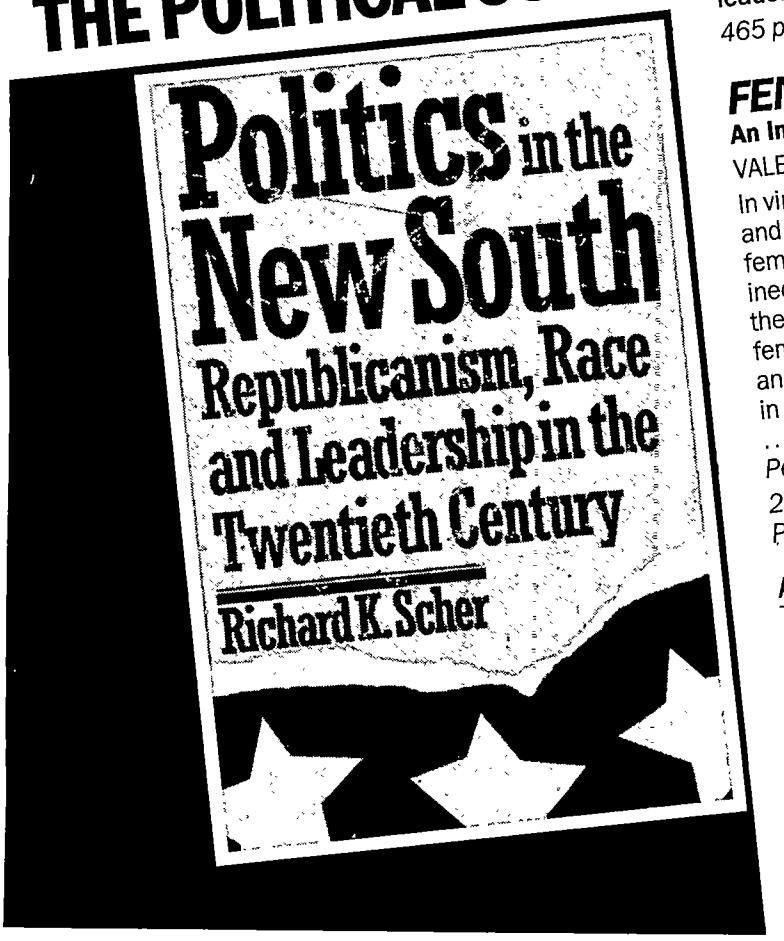
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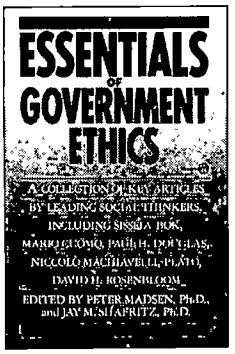
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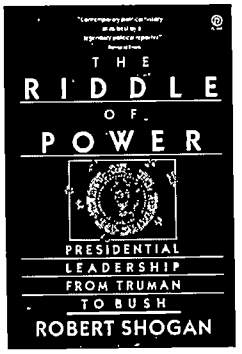
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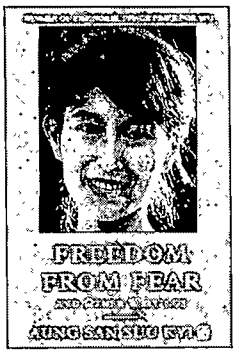


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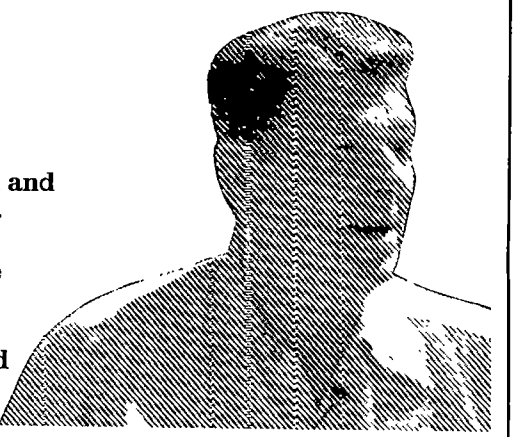
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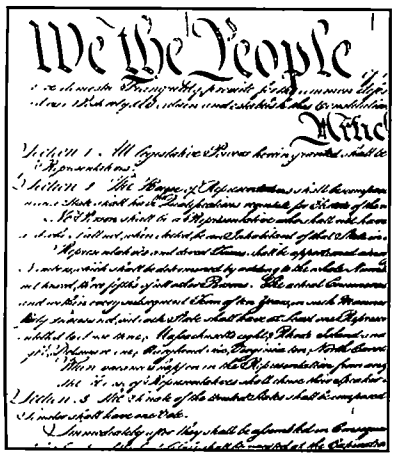
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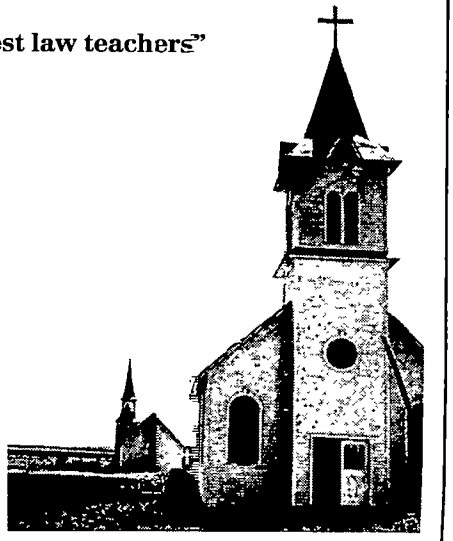
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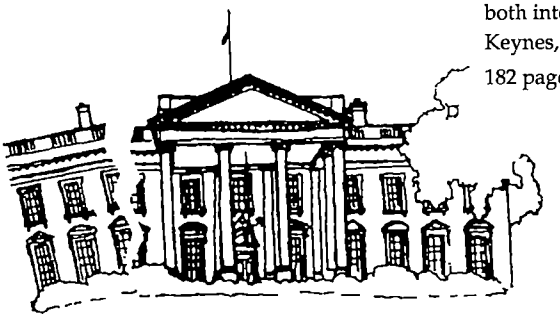
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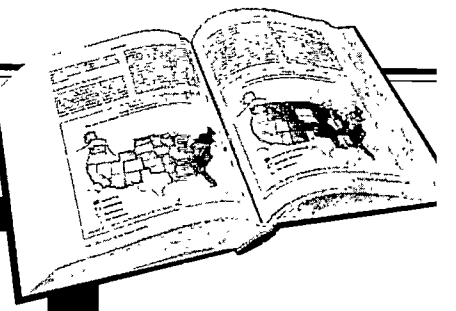
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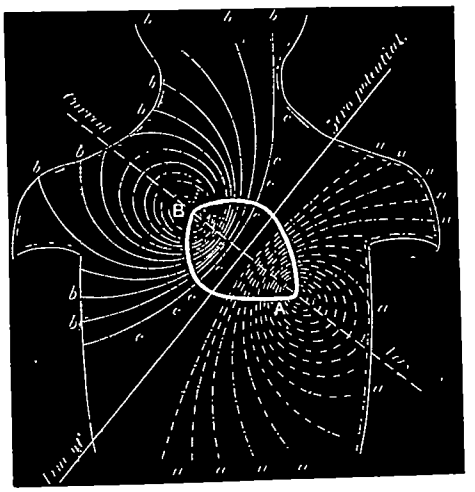
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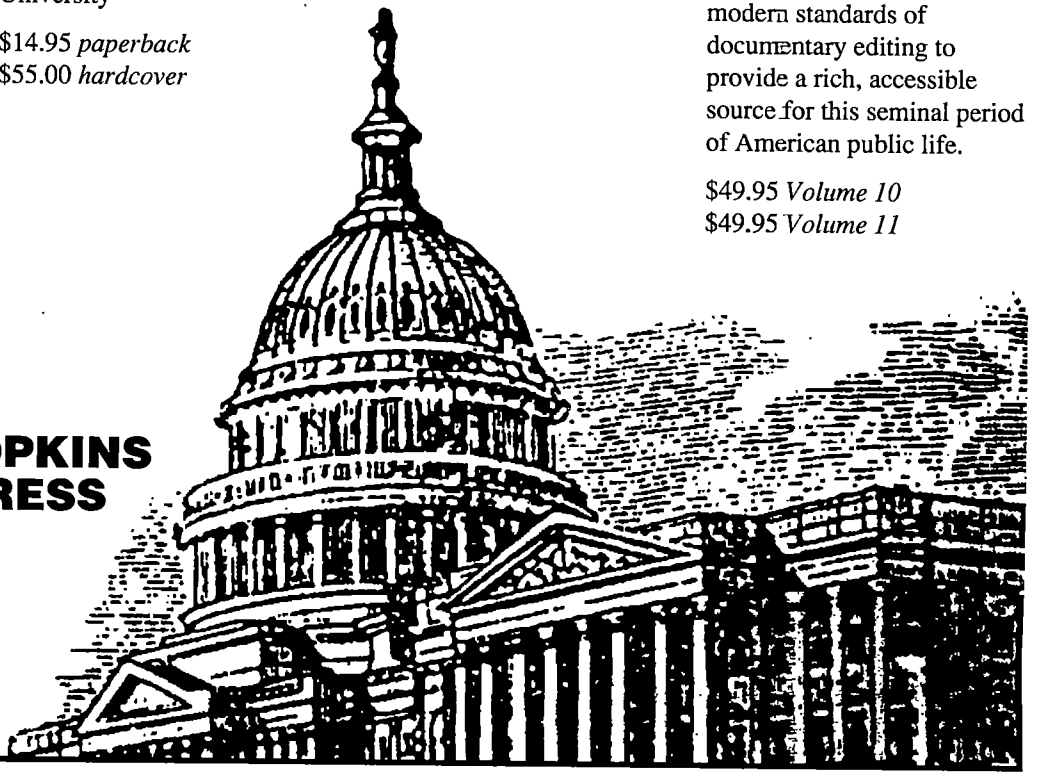
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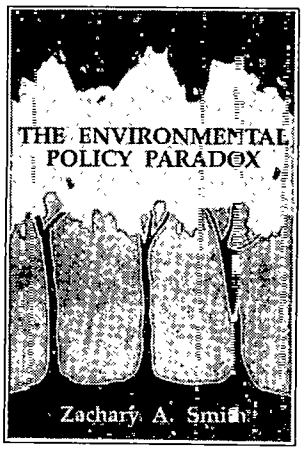
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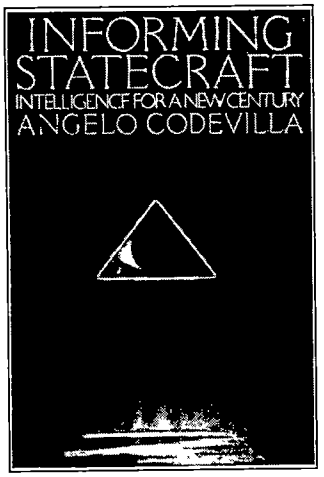
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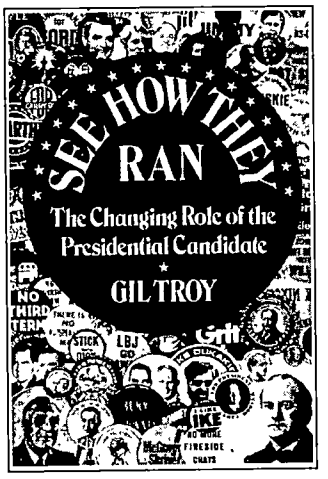
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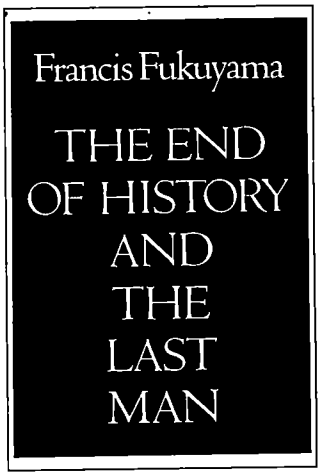
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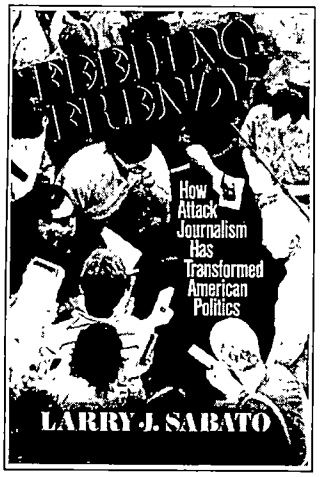
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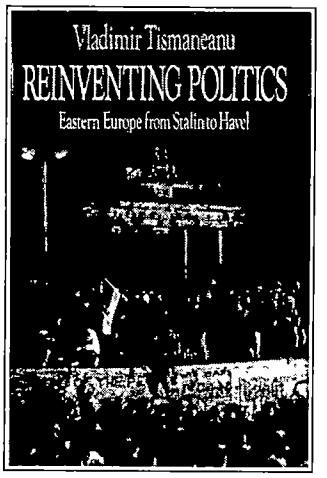


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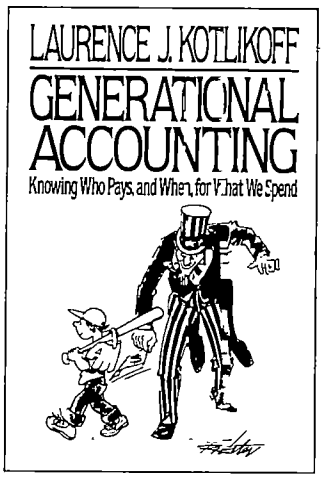


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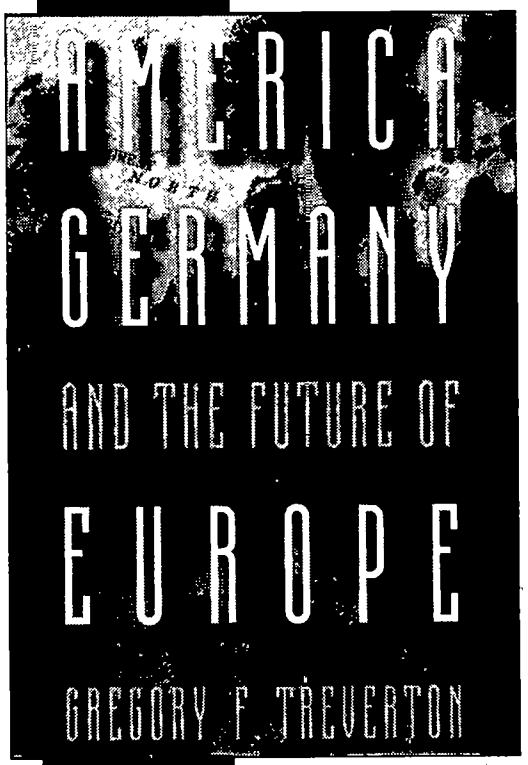
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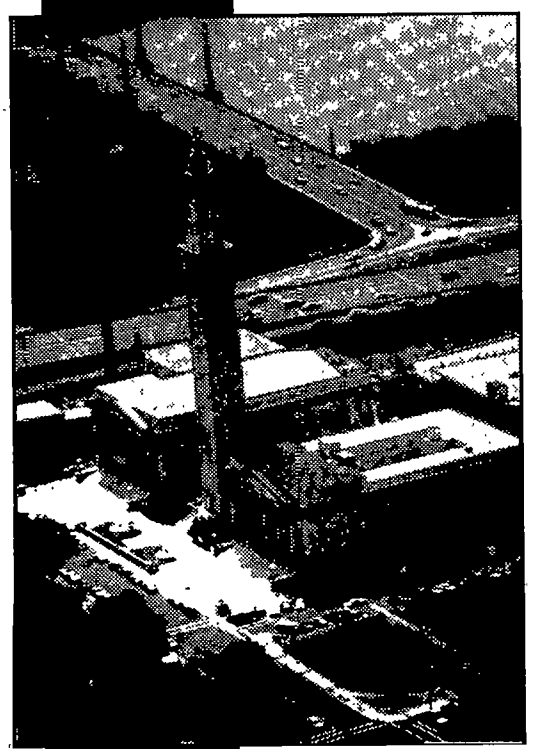
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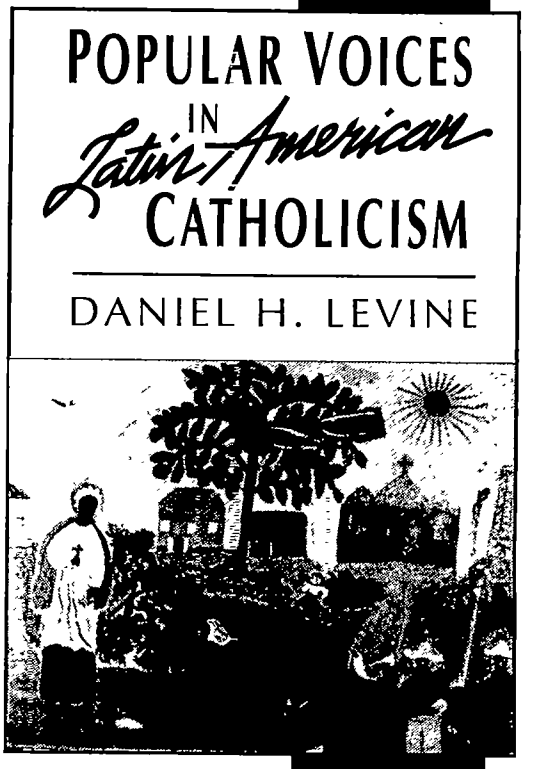
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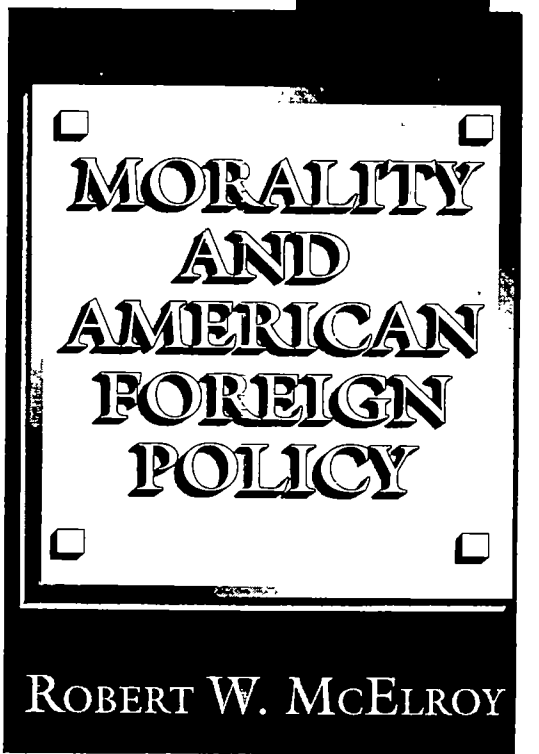
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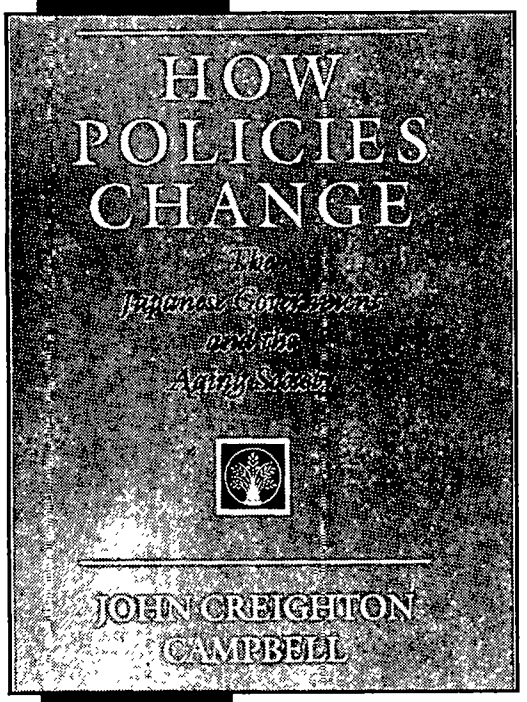
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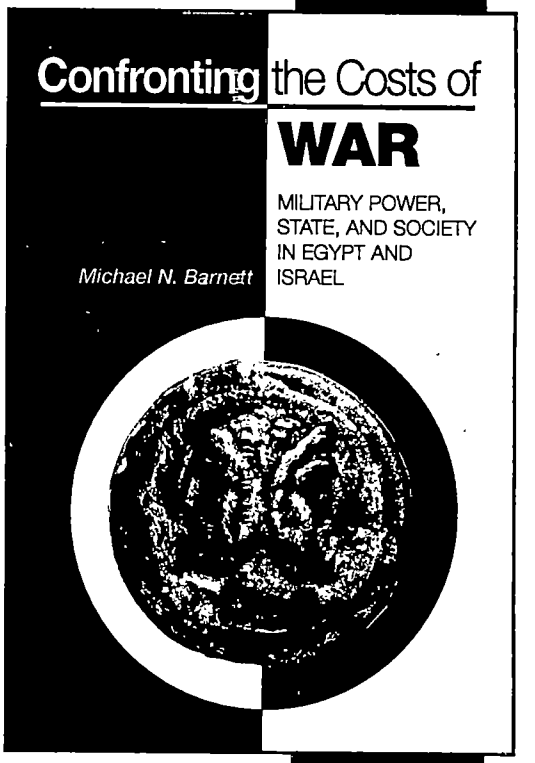
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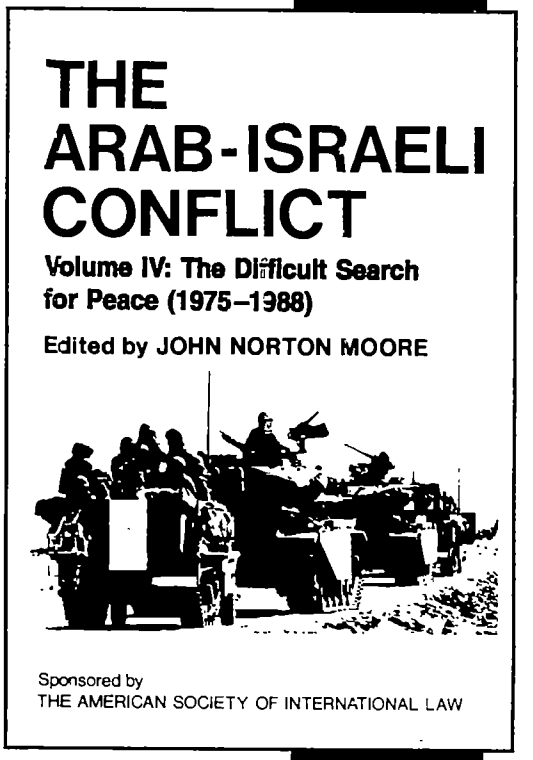
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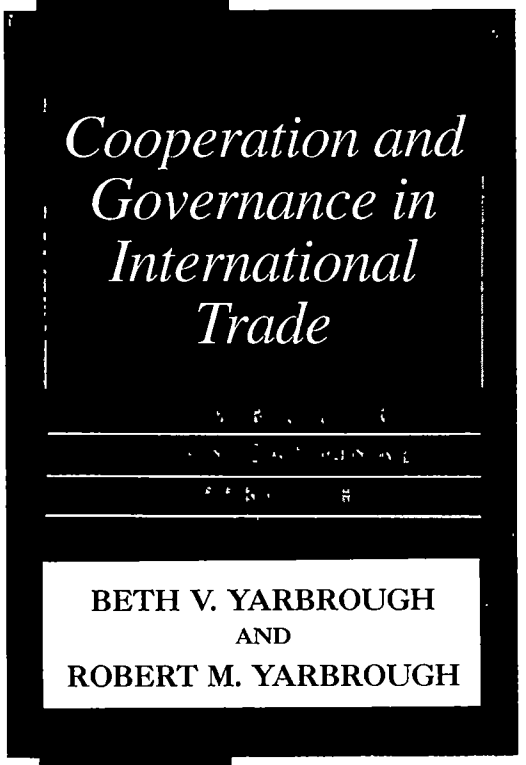
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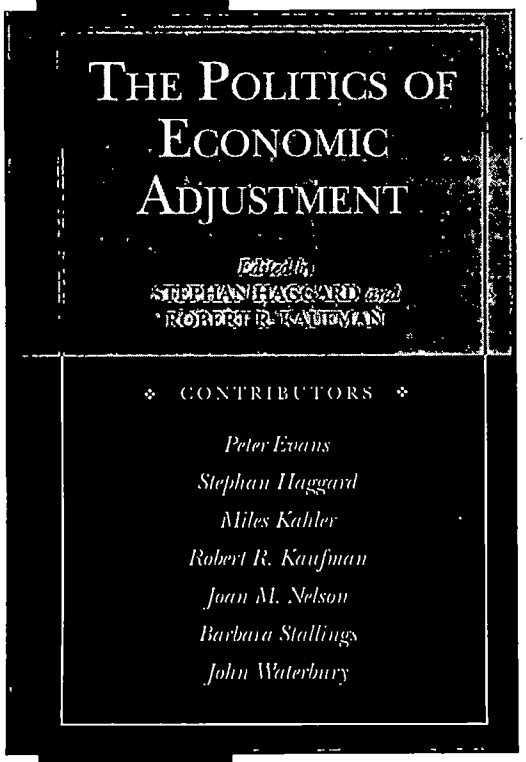
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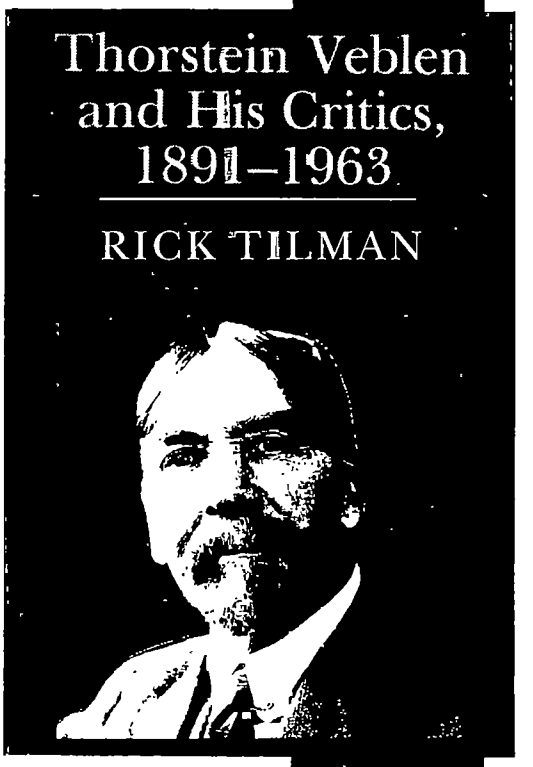
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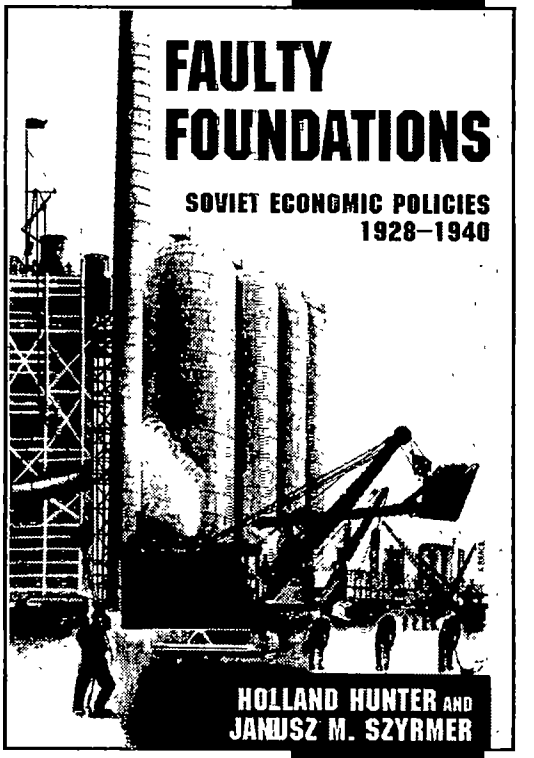
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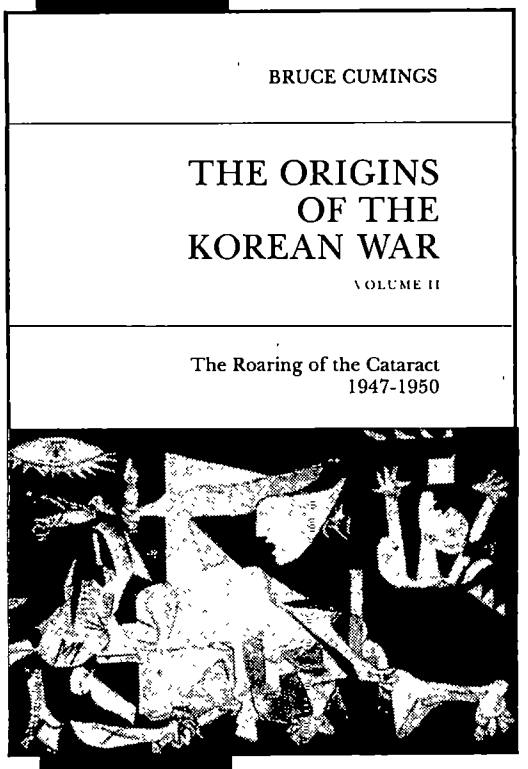
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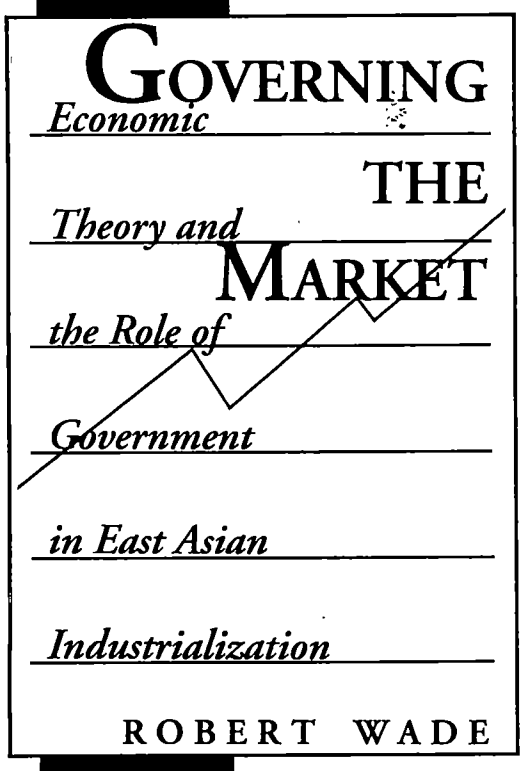
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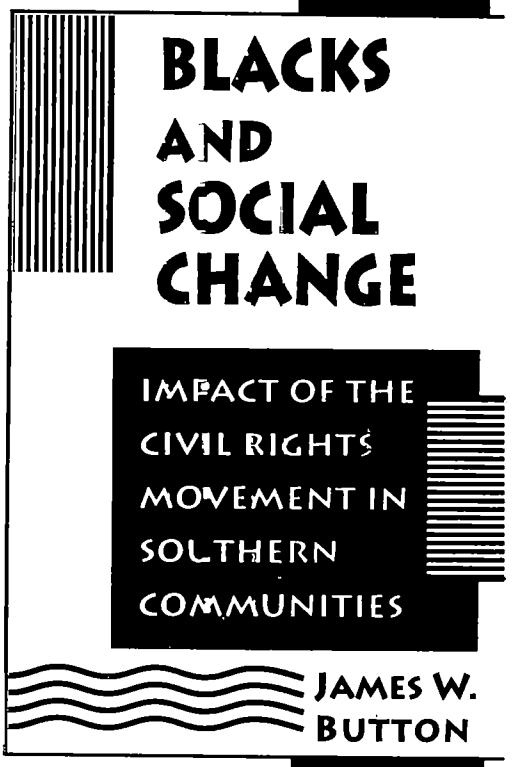
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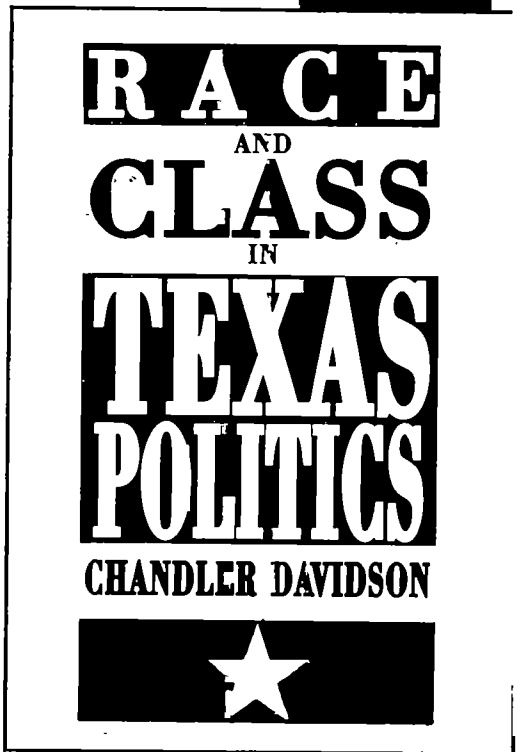
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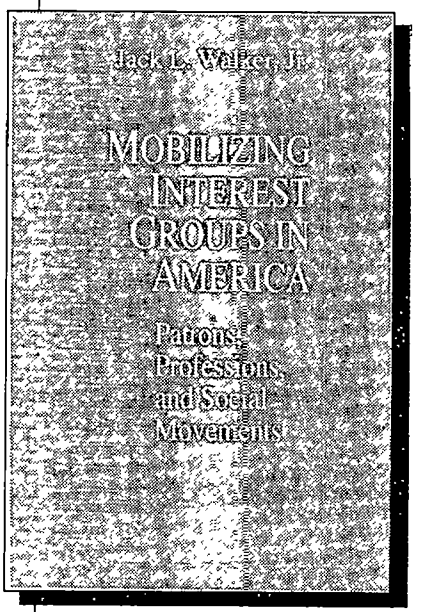
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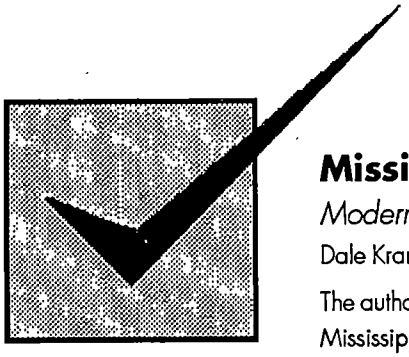
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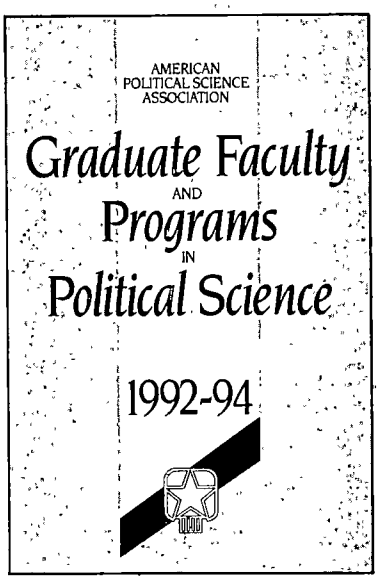
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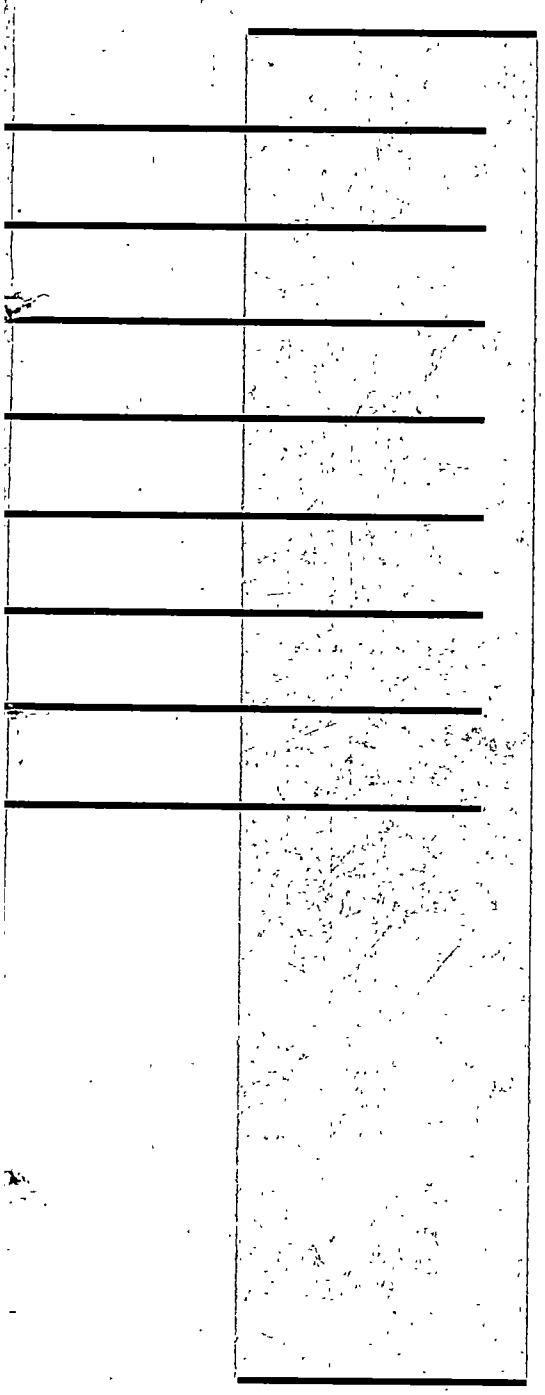
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ARTICLES

PEASANTS OR BANKERS? THE AMERICAN ELECTORATE AND THE U.S. ECONOMY

MICHAEL B. MacKUEN *University of Missouri, St. Louis*
ROBERT S. ERIKSON *University of Houston*
JAMES A. STIMSON *University of Minnesota*

The usual model of electoral reaction to economic conditions assumes the "retrospective" economic voter who bases expectations solely on recent economic performance or personal economic experience (voter as "peasant"). A second model assumes a "sophisticated" economic voter who incorporates new information about the future into personal economic expectations (voter as "banker"). Using the components, both retrospective and prospective, of the Index of Consumer Sentiment (ICS) as intervening variables between economic conditions and approval, we find that the prospective component fully accounts for the presidential approval time series. With aggregate consumer expectations about long-term business conditions in the approval equation, neither the usual economic indicators not the other ICS components matter. Moreover, short-term changes in consumer expectations respond more to current forecasts than to the current economy. The qualitative result is a rational expectations outcome: the electorate anticipates the economic future and rewards or punishes the president for economic events before they happen.

Economics moves political behavior. With hard times, administrations lose support; with good times, they gain it. We know this to be true. But when we ask how—by what processes—the political translation of economic experience occurs, the answers are far less certain. We do not know how the electorate experiences movements in the domestic economy or even why it cares. After decades of attention, we are little beyond introspection in understanding the processes by which citizens come to perceive economic movements. We do not know how they generalize from that experience to political evaluations. We do not know how they reach politically relevant conclusions about the political–economic future from the experiences of yesterday and today.

Beyond the causal dynamics lie matters of deeper consequence. In particular, we are interested in the quality of intelligence that governs the translation of economic experience into politics. Consider two caricatures: peasant and banker. The peasant judges the government by present personal experience. He or she eschews abstraction and, instead, relies on what may be seen and felt directly, on direct personal experience. The future is imagination, the present is reality. Turning to politics, the question is simply put: "What have you done for me lately?" The banker, in contrast, is indifferent about the past except as it portends the future. The banker judges the government by its ability to shepherd the future. Ignoring current conditions, the banker attends to matters of systemic consequence that indicate the government's wisdom, rather than its appetite. The banker asks, "What are your prospects?"

Clearly, these are matters of degree, rather than absolutes. No individual—and certainly no society—will act purely as peasant or as banker. It is our intent to evaluate the extent to which the U.S. political economy reflects the intelligence of peasant or banker. We wish to see how far political judgments are driven by directly experienced personal economic conditions on the one hand, and by expectations about the nation's economic future on the other. The character of intelligence that dominates the political economy is of apparent import on its own. In addition, however, our understanding of it should contribute mightily to a more general theoretical view about how industrial democracies use information to govern themselves.

Understanding that citizen economic perceptions affect political judgments only raises the question, What sorts of economic information drives those subjective perceptions and thus drives politics? In particular, are the economics of politics based in a reality that people can directly experience, as measured in assessments of their family's current economic well-being or do the economics of politics reflect a more distant and abstract view of national conditions or even expectations about national conditions in the future? In the jargon of current theoretical debate, one central question is whether political evaluations are moved by "pocketbook," or by "sociotropic," economic perceptions. This question is intertwined with another: Are political evaluations moved by retrospective, or by prospective, economic evaluations?

In the political science literature, the model of the economic voter has undergone considerable evolution. In an earlier day, the voter was understood to respond to economic conditions; but the psychology of the response was not mapped much beyond the general notion of stimulus and response (see, e.g., Key 1966). Good economic fortune led the economic voter to reward the incumbent, and bad times led the economic voter to punish. Whether this response was

instrumental or affective was rarely at issue. In the extreme, one could imagine the response of the economic voter as purely an emotional reaction in terms of anger or gratitude, void of any cognitions about the economic future. (On affective reactions to the economy, see Conover and Feldman 1986 and Peffley 1985).

Most contemporary discussions of economic voting treat economic voting as an instrumental act. As Fiorina (1981) has forcefully argued, the retrospective voter can use personal economic circumstances to form expectations about the voter's personal economic future under the current incumbent. In this sense, the retrospective voter is really prospective in nature. The economic voter may be a "peasant" in the sense of being guided by little more than personal experience but still use this limited information instrumentally as the best available guide to future economic reward.

As Kinder and Kiewiet (1979, 1981; Kiewiet 1983) have emphasized, voters often react to their perceptions of the national economy ("sociotropic voting"), rather than to personal economic circumstances. A sociotropic voter can use the current health of the economy as a signal of the incumbent's economic competence that will influence the voter's economic prosperity in the future. The sociotropic prospective voter meets some of the preconditions of the "banker" of our model.

Whether guided by their own pocketbooks or by their perceptions of national conditions, the behavior of instrumentally retrospective voters is roughly in accord with the economists' model of "adaptive expectations" (see Alt and Chrystal 1983). According to the adaptive expectations model, people modify their expectations about the future by extrapolating from a weighted average of current and recent values. An important limitation of this version of the economic voter, however, is that although prospectively oriented, the economic voter is totally myopic. Unable to look beyond current conditions, the myopic retrospective voter does not react to the future implications of current policy nor even to economic forecasts. Easy to fool, the myopic economic voter is a crucial ingredient in most models of the political business cycle (e.g., Nordhaus 1975; Tufte 1978).

In recent years, many economists have become attracted to "rational expectations" models of economic behavior. Without necessarily buying controversial "rational expectations" arguments about macroeconomic policy, we can imagine a "rational expectations" model of the economic voter. The heart of rational expectations economics is the notion that decision makers incorporate all available information, responding to events when they are anticipated, rather than waiting until they occur. Applied to economic voting, rational expectations would result in an electorate that responds to messages about the future economy, rather than extrapolates from current conditions. In the aggregate, we can imagine an electorate guided by the same intelligence as the economic forecasters. This electorate would discount the current economy, because current conditions (except for possible surprises) were built into previous forecasts. The rational expectations version of the economic voter will respond with little gratitude for past prosperity independent of future economic promise. In this sense, the rational expectations economic voter acts very much like a banker, rather than a peasant.[1]

MICRO ANALYSIS AND THE UNDERSTANDING OF ECONOMIC VOTING

The main evidence that the economy has important political consequences comes from macro analyses of both election outcomes and the presidential popularity time series. What we know of the psychology of the process, however, comes from the micro analysis of voter surveys. Two findings stand out in this literature: (1) voters respond more to their perceptions of the national economy (sociotropic voting) than to their personal financial experiences (pocketbook voting); (2) whether pocketbook or sociotropic, economic voting is strongly prospective, so that when surveys attempt to ascertain economic expectations independent of retrospective judgments, prospective expectations clearly drive out retrospective evaluations as predictors of vote choice (Kiewiet 1983; Kuklinski and West 1981; Lewis-Beck 1988).

But the findings of survey research are not at all tidy. For instance, prospective expectations appear to be only weakly connected to retrospective evaluations (Conover, Feldman, and Knight 1987; Kuklinski and West 1981). Except for extrapolation from current and recent conditions, we know little about the sources of peoples' economic expectations. Other identified sources of economic expectations include the individual's partisan bias and the persistence of past expectations (Conover, Feldman, and Knight 1987). Do citizens respond to economic forecasts? We know little about peoples' exposure to economic forecasts, although some scattered experimental evidence suggests that people do take forecasts into account when they are exposed to them (Ansolabehere, Iyengar, and Simon 1990; Olshavsky and Jaffee 1981).

Using survey research to draw inferences about the psychology of economic voting is made difficult by the limitations of micro analysis. For instance, as Kramer (1983) reminds us, very little micro change in family finances is government-induced. The result is a severe downward bias to estimates of the political response to government-induced changes in family economic circumstances. As Kramer also observes, given a survey cross-section, variations in evaluations of the economy can arise only from variations in perceptions, rather than variations in actual economic performance. These perceptions are prone to partisan rationalizations. A worrisome hypothesis is that respondents sometimes select opinions about the na-

tional economy or even their own pocketbook on the basis of consistency with their partisan views (Sears and Lau 1983; but see also Lewis-Beck 1986).

Partisan rationalization may be a special problem for direct evidence of prospective voting. Arguably, to ask respondents to make economic forecasts is to invite "doorstep" opinions, virtually made up on the spot. When asked in the context of a national political survey, respondents may be even more prone to manufacture economic forecasts from their vote choice than they are to bend their perceptions of current conditions in the direction of their vote choice. For this reason, micro survey evidence of prospective voting must be treated with particular caution.

CONSUMER SENTIMENT AND ITS COMPONENTS

The present study offers a time series analysis of the connections between (1) objective economic indicators, (2) aggregated economic cognitions, and (3) presidential approval. The economic cognitions are the Index of Consumer Sentiment (ICS) and, crucially, its components. The ICS and its components have been measured as part of the Survey of Consumer Finances and Survey of Consumer Attitudes and Behavior by the University of Michigan's Survey Research Center since 1953. The ICS is known to be responsive to the national economy (Katona 1964, 1975) and usefully augurs the economic future, as well (Fuhrer 1988; Matsusaka and Sbornone 1992). Its components have previously been shown to predict presidential approval (R. Shapiro and Conforto 1980). Of particular use here, the individual items comprising the ICS are similar in format to the economic items used in the National Election Studies. (See Lewis-Beck's [1985] innovative analysis of individual-level ICS data.) Exploiting the aggregate measures of these variables, we can learn something about the aggregate psychology by which the economy affects politics.

The ICS is comprised of six items. Citizens are asked to evaluate

1. current family finances "Would you say that you (and your family living there) are better off or worse off financially than you were a year ago?"
2. current business conditions (technically not a component of the index, but asked in virtually all Consumer Sentiment Surveys) "Would you say that at the present time business conditions are better or worse than they were a year ago?"
3. current buying conditions "Generally speaking, do you think now is a good or a bad time for people to buy major household items?"
4. next year family finances "Now looking ahead—do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?"
5. short-term business expectations "Now turning to business conditions in the country as a whole—do you think that during the next 12 months we'll have good times financially, or bad times or what?"
6. long-term business expectations "Looking ahead, which would you say is more likely—that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?"

The wording of the questions suggests clear-cut measures of pocketbook (items 1, 3, and 4) and sociotropic (2, 5, and 6), as well as retrospective (1–3) and prospective (4–6) evaluations. Indeed, the Survey Research Center combines items 4–6 into a separate Index of Consumer Expectations. Each item (and the index as a whole) is scored on a 200-point scale representing the net balance of positive and negative opinion, with 100 representing the neutral point.

Evaluation of current buying conditions (item 3) does not cluster with the other items and contributes little to our understanding of political attitudes. Also, the two measures of business expectations are too highly correlated (.95) to include both. When the two are in the same regression equation, the long-term measure always dominates short-term expectations. Accordingly, we drop the latter and keep the former. This leaves four aggregate measures of economic opinion: (1) mean perceptions of current family finances, or *Personal Retrospections*; (2) mean perceptions of current business conditions, or *Business Retrospections*; (3) mean perceptions of next year's family finances, or *Personal Expectations*; and (4) mean long-term *Business Expectations*.

These variables are measured on a quarterly basis since the fourth quarter of 1952. Except for occasional missing data, our data are an almost continuous series from the third quarter of 1954 to the second quarter of 1988. The paths of our four aggregate measures are highly correlated in our time series. On average, they correlate at .74 when measured concurrently and at .67 over one lag. Periods of prosperity lead to generalized good feelings while recessions affect both current and prospective evaluations of self and society. Yet the extent of this covariance is not overwhelming. The different series react differently to economic stimuli by taking different paths through time. (For a thorough demonstration, see Mebane 1988.)

With macro analysis, we examine the net responses of the national electorate, rather than individual voters. A compelling advantage of macro analysis is that idiosyncratic sources of variation in economic judgment cancel out. Judgments whether the economy will improve or falter, for example, may be too noisy for worthwhile analysis at the individual level. But their noise cancels out in the aggregate, to provide the powerful measure of collective judgments of the economic future. Moreover, these data from economic surveys are decidedly free of the problem

of political response rationalization that plagues micro analysis. On balance, the aggregated time series data set we examine here offers an important degree of inferential leverage that individual-level survey evidence cannot provide. Thus, our work both complements and extends the existing individual-level analyses.

Accounting for presidential approval in terms of collective economic beliefs is only one part of the task at hand. We hope, in addition, to understand how collective economic beliefs result from economic reality. The relationships between measured economic conditions and political evaluations might behave oddly for at least two reasons.

First and most important, the objective macroeconomic measures may not be good indicators of how individuals subjectively experience prosperity and hardship. Previous work (e.g., Katona 1964, 1975; Strumpel, Schmiedeskamp, and Schwartz 1973) shows that consumer sentiment is largely, but not entirely, a function of present and past economic indicators. The *not entirely* is important because the Index is valued as an indicator of future macroeconomic movements (and is particularly good at forecasting downturns; but see H. Shapiro 1972). In addition, cross-sectional evidence indicates that citizens react to the economy in ways that bypass cognitions about economic aggregates (see, esp., Conover and Feldman 1986). We may ask if citizen economic evaluations affect political judgments in ways that would not be predicted by directly observable economic aggregates. Having direct measures of subjective experience will enable us to examine the political economic links more carefully.

Second (more perversely), it may be that the macroeconomy affects political performance in ways that do not depend directly on how citizens evaluate the economy. Citizens may translate a societal sense of well-being or malaise into politics without being conscious of its economic underpinnings. This is most likely to apply to long-run relationships. For example, a president afflicted with an economic malady might come to be seen as incompetent, and subsequent political or international events might be perceived in that light long after the economy has recovered; or a lucky incumbent might find just the opposite. Further, affairs may be strategic and thus complex. For example, vagaries in economic fortune may encourage (or discourage) dramatic political adventure and thus political success or failure.

A CAUSAL FRAMEWORK

Before considering more subtle questions, we need to clear up some elementary matters of causality. Thus, we turn our attention toward the causal framework that governs the relationship between economic conditions, economic perceptions, and presidential approval. We lay out the evidence on four theoretically crucial questions: (1) Do real world economic conditions shape economic perceptions? (2) Do both economic conditions and economic perceptions affect presidential approval? (3) Do economic perceptions affect presidential approval in a way that is independent of the experienced economy? and (4) Does approval color subsequent economic perceptions?

For these questions, we turn to an analytic technique tuned to answer causal inquiries, a standard Granger causality test. While the conceptual argument that justifies this procedure is subtle,[2] the test itself is simple. To see whether the objective economy causes economic perceptions, we regress current perceptions (say, retrospective views of personal well-being) on their own lagged values and on the lagged values of economic conditions (here, unemployment change and inflation).[3] Then, using a standard partial F-test, we test whether the coefficients associated with all the economic variables might be zero, that is, whether the objective economy might not affect subsequent perceptions. If this null hypothesis succeeds, we infer that economic conditions do not cause perceptions; if the null hypothesis fails, we infer that conditions may cause perceptions.

The essential results appear in Table 1. Each row in the table represents an estimation equation, and each column represents the potential causal effect of a set of variables. The *p*-values associated with the relevant F-tests appear under the potential causal variable and in the row identified with the dependent variable. Thus, we estimate a .00 likelihood that the objective economy does not affect Personal Retrospections. We conclude, without much surprise, that Economic Perceptions of all sorts are clearly caused by the real economy. (The equations under (1) produce *p*-values of .00, .00, .00, and .01). Similarly, examining straightforward links to presidential Approval, we find that both the real economy and Economic Perceptions affect presidential Approval (*p*-values of .05 and .00). Again, no headlines.

We may also see whether the real economy's effect on Approval channels completely through mass perceptions or whether the economy affects Approval in indirect, perhaps Byzantine, ways. Accordingly, in the section labeled (3), we test the causal effects of the real economy and of perceptions *while controlling for each other*. If the economy affects Approval through causal pathways other than perceptions, then the economic variables should pass the multivariate Granger test. With a collective *p*-value of .17, they do not. Presidential Approval responds to the economy only to the extent the economy alters public perceptions of the economy. Meanwhile, with a *p*-value of .00, economic perceptions clearly affect approval even when not caused by the objective economy. Thus, we make two elementary assertions about the causal web. First, the experienced economy affects Approval through subjective Economic Perceptions. More intriguing, those Economic Perceptions also affect Approval for reasons not evident in the objectively experienced economy. This second inference provides the stimulus for the rest of our theoretical tale.

Finally, we can eliminate a potentially complicating

TABLE 1

Granger Causality Tests (Probabilities of No Causal Effect)

DEPENDENT VARIABLE	CAUSAL VARIABLES: ECONOMIC CONDITIONS	ECONOMIC PERCEPTIONS	PRESIDENTIAL APPROVAL
(1)			
Personal retrospections	.00	—	—
Business retrospections	.00	—	—
Personal expectations	.00	—	—
Business expectations	.01	—	—
(2)			
Presidential approval	.05	—	—
Presidential approval	—	.00	—
(3)			
Presidential approval	.17	.00	—
(4)			
Personal retrospections	.00	—	.97
Business retrospections	.00	—	.16
Personal expectations	.00	—	.64
Business expectations	.02	—	.86

Notes: Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Each row represents an estimation equation. Entries are *p*-values for appropriate F-tests on block coefficient restrictions. Each estimates the probability that the set of candidate causal variables does not "Granger-cause" the dependent variable. For economic conditions, the joint test is for two items: change in unemployment rate and inflation rate. For economic perceptions, the joint test is for four items: personal and business retrospections and personal and business expectations. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5. Each equation includes two lag terms (two quarters) for the endogenous variable—to control for autoregression—and one lag term (one quarter) for the potential exogenous variables. The first two quarters for any administration are eliminated (for this Granger analysis only).

possibility. From cross-sectional analyses, we understand that individuals color their perceptions of the economy by their prior evaluations of the president—Reagan supporters, for example, being less likely to acknowledge the recession of 1982 and Reagan opponents being less likely to acknowledge the subsequent recovery. Further, individuals who think the current president a competent (or incompetent) policymaker might rationally use that judgement to foresee a promising (or disappointing) future. These phenomena, so strong in the cross-section, need not translate into our cross-temporal data;[4] they need testing. The equations (labeled (4)) test Approval's effects on Economic Perceptions by controlling for the objective economy. In fact, we discover that Approval does not shape Economic Perceptions (the relevant *p*-values are .97, .16, .64, and .86). These results make our task much easier. We may straightforwardly model relationships between the economy, Economic Perceptions, and presidential Approval in a simple recursive system. Both economic experience and other things (yet unknown) shape economic perceptions, which then affect presidential approval.

MODELING PRESIDENTIAL APPROVAL

For our time series analysis of presidential Approval, we model quarterly Approval as a function of lagged Approval (at quarter $t - 1$) plus current values of our economic variables of interest. To guard against spurious effects, our Approval equations also include a number of standard controls, for which we do not present the actual coefficients. These include dummy variables for presidential administrations and controls for Johnson's Vietnam War (troops in Vietnam), Watergate, the Iran hostage crisis, and a modest series of important events.[5]

This specification represents a distributed lag model, using the Koyck transformation, which has become a standard approach to the analysis of presidential approval (Beck 1992; Kiewiet and Rivers 1985; King 1989). Independent variables are hypothesized to affect the dependent variable immediately and then leave a residue that declines gradually over time. With the Koyck transformation, the lagged values of the dependent variable capture the effects of lagged independent variables. This allows the convenience of including only current values of independent variables in the equations. Although regression coefficients represent the effects of current values of variables on current Approval (controlling for lagged approval), the effects of the independent variables cumulate, at a rate determined by the autoregressive effect of lagged approval. Thus, the effect of any economic variable will resonate not only in the current quarter but also feed forward into the future. Similarly, the current approval level will represent the effects of both current and lagged values of the independent variables. Since lagged effects decay exponentially, current and recent values outweigh those more temporally distant.[6]

TABLE 2

Presidential Approval by Economic Conditions and Consumer Sentiment

INDEPENDENT VARIABLE	APPROVAL[a] (1)	(2)
$Approval_{t-1}$		
b	.87	.82
	(.04)	(.04)
p	.00	.00
$Inflation_t$		
b	−.39	−.17
	(.13)	(.13)
p	.00	.55
Change in $unemp._t$		
b	−1.51	.62[b]
	(.74)	(.91)
p	.04	.50
ICS_t		
b	—	.21
		(.05)
p		.00
Comb. sign. infl. rate, unemp. change	.00	.36[b]
Adjusted R^2	.933	.941
N	126	117

Note: Standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5.

[a]Each column represents a separate regression equation, each with approval as the dependent variable.

[b]Test includes coefficient with wrong sign.

THE ECONOMY, CONSUMER SENTIMENT, AND PRESIDENTIAL APPROVAL

Confirming conventional wisdom, the standard economic indicators are related to presidential approval. This can be seen from Table 2. Column 1 shows coefficients for the inflation rate and the quarterly change (first difference) in unemployment level. These coefficients are jointly significant, as one might expect.[7] But this is not the full story. Table 2, column 2 introduces the composite ICS. The ICS has a decidedly significant impact on Approval. As anticipated from our earlier discussion of the Granger tests, introducing the ICS wipes out the "direct" contributions of the economic variables. The obvious inference is that Consumer Sentiment is an intervening variable between the objective economy and Approval. The changing economy affects presidential approval because it affects Consumer Sentiment. Clearly, the economy affects approval by affecting perceptions of the economy, which are captured by the Index. Our next task is to ascertain which of the indicators comprising the index are the crucial intervening variables.

Table 3 displays the results of a race among the four chosen ICS indicators, competing to predict approval. Column 1 considers the effect of Personal Retrospections, running alone except for the control variables. As the sole measure of Consumer Sentiment in the race, Personal Retrospections perform credibly, with a showing that passes the .000 level of statistical significance. Column 2 introduces Business Retrospections to compete with Personal Retrospections. Here, we can ask, Where a gap exists between collective economic satisfaction and perceptions of the economy, which one wins? The answer is clearly the latter, Business Retrospections. Business retrospections make a significant contribution ($p = .03$) where Personal Retrospections falter with a decisively weak .63 *p*-value. Thus, in the battle between the familiar pocketbook and sociotropic measures, now taken to the aggregate level, sociotropic wins: a president achieves greater popularity by having people think the economy is booming when it is not than by having people prosper but view the economy as sick.

Next we allow the crucial prospective measures to do their work. Table 3, column 3 enters Personal Expectations to compete with Business Retrospections. We see that this new entry in the race passes the test of statistical significance and undercuts the coefficient for Business Retrospections. A reasonable explanation is that citizens reward the president for current business prosperity because of the implication that they will personally benefit later. Finally, we turn to Business Expectations. Presumably, the reason people think current prosperity will help them personally is that current prosperity will lead to future general prosperity. Column 4 confirms this expectation. The newest entry now dominates our race: with Long-Term Business Expectations entered, it alone is a statistically significant predictor of presidential approval. What the president gains from the perception of current business conditions is of little political value unless it translates into a perception of future national prosperity. In other words, a president achieves greater popularity by convincing the public that a slack economy will improve than by showering the nation with a prosperity that people worry will disappear. Moreover, since the coefficient for Personal Expectations also dwindles to insignificance with the entry of Business Expectations, a collective expectation of future personal well-being does not help the president unless it is generally attributed to a prospering general economy.

Table 3, column 5 shows the coefficients when all four indicators are raced together. Again, the measure of Business Expectations wins. With four components entered, Business Expectations is the entry that most clearly survives a test of significance. Still, we should be wary of the possibility that other worthy variables lose the race due to bumping others (multicollinearity) and not because of a lack of pace (substantive insignificance). However, the other three indicators are not even collectively significant. As a further test, columns 6–8 race Business Expec-

TABLE 3

Presidential Approval by Consumer Sentiment's Components

INDEPENDENT VARIABLE	APPROVAL[a]									
	(1)	(2)	(3)	(4)	(5)[b]	(6)	(7)	(8)	(9)	(10)
$Approval_{t-1}$										
b	.85	.84	.83	.81	.82	.82	.80	.82	.82	.83
	(.04)	(.05)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)	(.05)
p	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000
Pers. $retrosp._t$										
b	.13	.03	—	—	−.06[c]	.02	—	—	—	—
	(.04)	(.06)			(.06)	(.04)				
p	.000	.63			.35	.70				
Bus. $retrosp._t$										
b	—	.04	.02	.00	.01	—	.01	—	—	—
		(.02)	(.01)	(.02)	(.02)		(.02)			
p		.03	.09	.85	.47		.59			
Pers. $expect._t$										
b	—	—	.20	.12	.16	—	—	.11	—	—
			(.06)	(.07)	(.08)			(.06)		
p			.002	.08	.05			.07		
Bus. $expect._t$										
b	—	—	—	.11	.10	.14	.11	.16	.16	—
				(.04)	(.04)	(.04)	(.03)	(.03)	(.03)	
p				.01	.02	.000	.001	.000	.000	
Bus. $expect._{t-1}$										
b	—	—	—	—	—	—	—	—	—	.16
										(.04)
p										.000
Change in bus. $expect._t$										
b	—	—	—	—	—	—	—	—	—	.16
										(.04)
p										.000
Adjusted R^2	.932	.929	.936	.939	.939	.944	.938	.946	.945	.945
N	121	110	110	110	110	115	110	115	115	103

Note: Standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5.
[a]Each column represents a separate regression equation, each with approval as the dependent variable.
[b]Significance of Personal Retrospections, Business Retrospections, and Personal Expectations = .24.
[c]Test includes coefficient with wrong sign.

tations against each of its three competitors separately. In each instance, Business Expectations decisively wins the contest with a highly significant coefficient, while the competitor fails to achieve statistical significance. (The closest is Personal Expectations with a .07 *p*-value.)[8] Column 9 shows the coefficient for our victor, Business Expectations standing alone in the equation. For every point of change on this 200-point scale, approval moves about .15 of a point. Column 10 decomposes this result further into the effect of the lagged level of Business Expectations and the current change in Business Expectations. Both the long-term cumulation and the short-term innovation in Expectations are highly significant.[9]

Clearly, the reason presidential approval responds to the economy is that presidential approval responds to economic expectations. Controlling for Business Expectations, no other measure of economic sentiment directly affects Approval. Economic conditions affect presidential popularity only to the extent that economic conditions alter expectations of the economic future.

ACCOUNTING FOR EXPECTATIONS

Our next challenge is to offer a fuller accounting of the sources of Business Expectations. A beginning of an answer, we think, is that people are embedded in a rich system of social communication. The social communication network is full of information not only about the past and the present but also about the future. Any encounter with the nightly news will subject the viewer to the bullish or bearish views of politicians, Wall Streeters, or economic correspon-

TABLE 4

Explaining Components of Consumer Sentiment

INDEPENDENT VARIABLE	DEPENDENT VARIABLE[a] PERS. RETROSP. (1)	BUS. EXPECT. (2)	ECON. NEWS (3)	BUS. EXPECT. (4)
$Inflation_t$	−.76 (.19)***	−.80 (.29)**	−1.34 (.40)**	−.41 (.34)
Change in $unempl._t$	−5.86 (1.85)**	−.23 (2.58)	−11.11 (5.02)*	5.15 (3.02)[b]
Change in $leading\ indic._t$	−.01 (.09)[b]	.25 (.11)*	.91 (.19)***	.12 (.12)
$Pers.\ retrosp._t$	—	—	—	.08 (.10)
$Econ.\ news_t$	—	—	—	.16 (.05)**
$Pers.\ retrosp._{t-1}$	.62 (.06)***	—	—	—
$Bus.\ expect._{t-1}$	—	.79 (.05)***	—	.74 (.05)***
$Econ.\ news_{t-1}$	—	—	.47 (.07)***	—
Sign. of infl., $unempl._t$	.000	.018	.002	.020[b]
Sign. of $leading\ indic._t$	.872	.025	.000	.320
Adjusted R^2	.742	.883	.792	.889
N	126	110	101	106

Note: Entries are unstandardized regression coefficients; standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988.
[a]Each column represents a separate regression equation.
[b]Test includes coefficient with wrong sign.
*$p \leq .05$.
**$p \leq .01$.
***$p < .001$.

dents. The news stories tell of new developments, sometimes citing inflation or unemployment numbers but more often describing a war, drought, strike, currency fluctuation, or daily rise and fall of the stock market. Almost inevitably, such events are read as good or bad omens. While the particulars of such accounts may fail to register in the public's consciousness, our evidence suggests that the general sense of optimism or pessimism seeps through the system in politically important ways.

The experts who translate the economic numbers into good times and bad times attend to more than the standard news. In the business of looking ahead, they absorb news that is critical to professional forecasting but often of little direct interest to the general public. Their translations convey their sophisticated understanding to all. Without trying, the public is exposed to the best information about the economic future that exists. Merely by noting that most forecasters say good (bad) times are ahead, the public becomes subject to the causal influence of the professionals' more esoteric tools.

If this story fits the truth of the flow of economic news, it follows that we should find some of the innovations in expectations flowing from early indicators of the economic future, such as the Commerce Department's Index of Leading Economic Indicators. Leading Economic Indicators measure the economic winds that are not otherwise apparent from current measures such as unemployment and inflation. We do not assert that the public monitors the Leading Indicators directly, nor do we believe that the Index comprises the entirety of economic intelligence. Rather, changes in the Leading Indicators provide a rough and consistently available measure of shifting expert forecasts. When the Leading Indicators influence the forecasters, that influence should find its way into ordinary peoples' views.

We test this thesis with a series of regressions, shown in Table 4, modeling quarterly change in Business Expectations and (for comparison) other Consumer Sentiment components. On the right-hand side, these equations include (1) lagged levels of the dependent variable, (2) the usual suspects of unemployment change and the inflation rate, and (3) the annualized quarterly growth in the Index of Leading Indicators.[10] For a baseline, Table 4, column 1 presents the Personal Retrospections equation. Quarterly aggregate perceptions of the family pocketbook are strongly predicted from their lagged values plus unemployment and inflation. This makes common sense: unemployment means hard times,

and inflation both increases transaction costs and sometimes marks real income declines. But since Leading Indicators supposedly tap the future economy, not the present, there is no theoretical reason why current change in Leading Indicators should affect current personal economic well-being. This expectation is borne out by the nonsignificant coefficient for leading indicators in column 1. Aggregate Personal Retrospections represent a measure of current national prosperity—experienced by individuals and uncontaminated by expert commentary.[11]

Compare this pattern with the equation for our crucial variable of Business Expectations in Table 4, column 2. In this equation, the contribution of inflation is significant, while that of unemployment change is not. Bouts of inflation, of course, increase individual economic uncertainty and thus reduce confidence in the future. More to our point, however, current unemployment is not seen as a harbinger of the long-term economic future. Leading indicators, meanwhile, show a statistically significant effect, just as our social communication model predicts.

We have argued that the electorate develops its economic expectations from the economic forecasts available in the mass media. To test this proposition more directly, we can exploit another item from the Survey of Consumer Attitudes that we have hitherto ignored. Most Consumer Sentiment Surveys include an item asking respondents' perceptions of recent economic news: "During the last few months, have you heard any favorable or unfavorable changes in business conditions? What did you hear?" Aggregated, this measure of Economic News is the net balance of positive versus negative news perceived about the economy during the quarter. The question has a retrospective flavor that should prompt responsiveness to reports of current conditions (people out of work or back on the job, inflation at the supermarket, etc.), as well as professional economic forecasts. Tims, Fan, and Freeman (1989) show that this measure, when aggregated, corresponds closely with the economic news reported in the press. The question is whether the economic news that people hear reflects projections about the future, as well as summaries of the present. Table 4, column 3 shows the Economic News equation. As expected, News reflects current inflation and unemployment change. Provocatively, though, Economic News is even more strongly responsive to changes in Leading Economic Indicators.

When people make judgments about the future economy, they must rely almost entirely on two sources of information: their personal experience and the available economic news. We now have a survey-based measure of each: Personal Retrospections and Economic News. Personal Retrospections measures the aggregate sense of personal economic well-being within the electorate. Economic News measures the direction of economic news that people report hearing. We can incorporate each as an independent variable in our equations predicting Business Expectations. The results are shown in Table 4, column 4.

As expected, Economic News makes a significant contribution to Business Expectations. Including Economic News removes the direct effect of Leading Indicators (compare col. 2): economic forecasts affect expectations by passing through the Economic News. Further, neither unemployment change (wrong sign) nor Personal Retrospections contribute to Business Expectations. Collectively, people do not seem to make a connection between their—or the nation's—current standard of living and the economic future. Instead, they judge the future based largely on what they are told in the mass media.[12]

The electorate acts as if it develops sophisticated expectations based on economic forecasts rather than current economic conditions. This follows because, in the final analysis, the electorate relies most on what is reported in the news. And the news reports what the future holds.

EXPECTATIONS AND THEIR REALIZATIONS

Presidential approval is a function of the electorate's collective expectations about the economic future. These expectations are formed, in large part, by what economists and commentators assert about the economic future, rather than what the electorate senses about current economic conditions. One implication of this result is that to the extent that economists' forecasts are correct, the future realization of economic conditions should predict presidential approval at the expense of current conditions. Does this conjecture stand up to the data?

Consider the contribution of (anticipated) future economic conditions on approval. As a base, recall that current conditions clearly affect current innovations in Approval. (For convenience, Table 5, column 1 repeats the equation with Approval as a function of current economic conditions from Table 2, column 1). Both current unemployment change and current inflation are significantly related to Approval. But note what happens when future ($t + 1$) values of unemployment and inflation are added (column 2). The two future measures are (collectively) highly significant, while the current collection is now nonsignificant. Finally, column 3 shows the equation with the economic variables measured for the next quarter alone. The estimated economic effects are stronger than from the standard equation predicting Approval from the current economy.[13] These results have an obvious and important explanation. The apparent reason that current conditions predict Approval is that current conditions help to forecast the future. With next quarter's economic conditions known, the current economy does not affect Approval. Conversely, the introduction of current conditions fails to diminish the future's importance for current Approval. It is the future, rather than the present, that matters most.

There is one particular irony to this result. Political scientists have searched long and hard for strong

TABLE 5

Business Expectations, Realizations, and Approval

INDEPENDENT VARIABLE	APPROVAL[a] (1)	(2)	(3)
$Approval_{t-1}$	.86 (.04)***	.87 (.04)***	.88 (.04)***
$Inflation_t$	−.38 (.13)**	−.11 (.16)	—
Change in $unempl._t$	−1.51 (.74)*	−.97 (.88)	—
$Inflation_{t+1}$	—	−.32 (.16)*	−.34 (.13)*
Change in $unempl._{t+1}$	—	−1.57 (.96)	−2.42 (.73)***
Sign. of $infl._t$, $unempl._t$	.0002	.40	—
Sign. of $infl._{t+1}$, $unempl._{t+1}$	—	.017	.0000
Adjusted R^2	.933	.937	.937

Note: N = 126. Entries are unstandardized regression coefficients; standard errors are in parentheses. Data are quarterly, from the second quarter of 1954 to the second quarter of 1988. Approval estimation equations include (additionally but not shown) variables controlling for political events, the Vietnam war, and dummy variables for each administration. See n. 5.

[a]Each column represents a separate regression equation, each with approval as the dependent variable.

*$p \leq .05$.
**$p \leq .01$.
***$p \leq .001$.

evidence that the economy affects politics and found this strong evidence to be somewhat elusive. This search has even lead scholars to search beyond current effects to seek out possible economic effects with long lags. Some scholars have reported delayed effects that do not "kick in" until a delay of perhaps several months (e.g., Monroe 1978). Others have even claimed that when conservatively specified, the search turns up empty (e.g., Norpoth and Yantek 1983). We suggest that the search has looked in the wrong temporal direction. Instead of finding the source of current changes in presidential approval somewhere in the economic past, we should find it in the immediate economic future.

DISCUSSION: A DEMOCRACY OF BANKERS?

We began with the question, When it makes political judgments based on the handling of the economy, does the American electorate act more like a peasant or a banker? The answer now seems clear. The electorate responds with the sophistication of the banker, evaluating the president on the basis of an informed view of the nation's economic prospects, rather than its current standard of living.

Consider the difference between our new model and the traditional model of presidential approval. In conventional wisdom, current economic shocks affect approval immediately; and these effects decay over time. Alternatively, current Approval is a function of the accumulation of past and current economic shocks; that is, the most recent economic events have the greatest impact on current approval, but current Approval still shows the effects of past events. The typically strong coefficient for lagged Approval indicates that the response to economic events takes a while to decay.

The difference between this traditional model and ours is the nature of the economic shock. In the traditional model, the shock is implicitly assumed to be the *realization* of an economic change. In ours, the shock is, at least partly, the *anticipation*. The electorate is foresighted, rather than myopic. If the economy is rosy but with dark clouds on the horizon, the electorate responds to the clouds, not to the roses.

Understanding that expectations, rather than retrospections, lie at the core of political evaluations forces a new view of the political economy. When citizens are retrospective, their politics are grounded in reality—personally experienced or observed in others. When citizens act on expectations, they rely on an informed imagination. This transformation of the base of politics, from reality to imagination, suggests a serious reconsideration of the role that information—and information production—plays in the polity.

First, think about the potential for political actors to manipulate economic and political outcomes. We might imagine such perversities as a president's maintaining approval by continually convincing the electorate that prosperity is around the corner even as the economy actually declines or the reverse case of a president who is pilloried for constantly negative economic forecasts even as the economy continues to prosper.

We discount the prospects for scenarios such as these, for two reasons. First, if such practices exist, they would be manifest in the data. We would find periods, for instance, when Leading Indicators persistently mispredict subsequent prosperity or when Expectations persist in one direction even though their trailing Retrospections go the other. We find no such patterns. When one quarter's innovation in economic expectations is wrong in one direction, the next quarter's innovation is as likely to err in the opposite direction: mistakes are not consistent. As these innovations add up, the errors cancel out to produce an accumulation of expectations that closely match their accumulated realizations.

Second, it is quite possible that when the electorate modifies its approval in response to a forecast that later proves to be mistaken, the electorate will eventually correct its evaluation in accordance with reality. In other words, the electorate may respond both to the immediate surprise of revised forecasts and to the gradual surprise of errors in past forecasts. Although our analysis has concentrated on the former, we observe hints of the latter.[14] The spirit of rational expectations surely incorporates such self-correction.

Further, because the electorate is foresighted,

rather than myopic, it is less easily fooled by the kinds of short-term budgetary strategies that supposedly give rise to a political business cycle (e.g., Nordhaus 1975; Tufte 1978). Recent work on the political business cycle, in fact, assumes that the electorate enjoys the necessary sophistication to take into account the motivations of political leaders (Alesina, Londregan, and Rosenthal 1990; Alesina and Rosenthal 1989; Rogoff 1990; Rogoff and Sibert 1988).

Beyond the question of strategic manipulation, we must understand that even our bankers are at the mercy of the quality of the available information. For the electorate to evaluate presidential performance properly, economic forecasts must be accurate and readily available. Our research suggests these conditions are met but that the hold may be fragile.

Intriguing is the possibility that the economic expectations that move presidential approval are partially self-fulfilling—as if consumer confidence boosts the economy.[15] When expectations are self-fulfilling, judgments based on those expectations will always appear to be sound even when they represent little more than fantasy. For the practical policymaker, self-fulfilling expectations are two-edged: it is possible to effect economic and political change by affecting expectations alone; it is also possible to be unable to effect real change when expectations discount current acts. (For a thoughtful discussion and evidence on the matter, see Alt 1991.) For the political theorist, the matter is more profound: if political judgments are self-sustaining, by what standard can one measure the wisdom of democratic decisions?

Although we have presented evidence of an impressive economic intelligence on the part of the U.S. electorate, it is worth repeating that this result depends on the powers of aggregation. As individuals, voters show no strong talent for economic forecasting. Massive biases and seemingly random errors swamp whatever accuracy is present in individual forecasts (Conover, Feldman, and Knight 1987). When these weak forecasts are aggregated, however, the noise cancels out to leave only a signal surviving. The process of aggregating information is not unlike the process that leads a large jury to reach an accurate verdict in situations when many individual jurors do not (Grofman and Owen 1986; Miller 1986).

Our results do suggest one reason to offer special praise for the individual qualities of American voters. This is the electorate's choice of economic signal to guide its political judgments. Instead of judging presidents retrospectively, based on economic conditions as they happen, voters respond prospectively to the likely economic future. While this evidence of prospective behavior is limited to the evaluations of the economy's health and presidential approval (and thus does not directly address matters of policy and voting), it suggests richer possibilities in the political system than are often assumed. When politicians know that people care about the future, they will act accordingly. Thus, while a myopic electorate produces short time horizons for politicians' policies, a foresighted electorate may reward policies the benefits of which extend beyond the next election. If this extrapolation from our findings proves true, we shall need to recast the conventional wisdom about the dynamics that link the electorate, politicians, and public policy.

The fact that the aggregate public listens to, and moves in accord with, an informed elite analysis empowers that public to make collective political judgments quite beyond the individual talents of its members. The demonstrated importance of rational expectations expands our view of political life. The standard notion of "What have you done for me lately?" is a mean sort of politics. To the extent that the mass public's political evaluations of the present depends on how the future is changed, we have a politics that resides less firmly on the ground of everyday life and much more resolutely in the spirit of the human imagination.

APPENDIX: AN ANALYSIS OF THE INNOVATIONS IN RETROSPECTIONS, EXPECTATIONS, AND APPROVAL

Our straightforward regression analysis (reported in Table 3) shows that expectations are important even when statistically controlling for current retrospections. The conclusion is that expectations, rather than retrospections, are the proximate influence on approval. Moreover, a further analysis suggests that retrospections are rather unimportant even as an indirect cause of approval. The ordinary least squares evidence (see Table 4) shows that business expectations, which help drive approval, are quite unresponsive to retrospections.

The bulk of our evidence comes from a fairly simple set of regressions on quarterly time series data. We may buttress our argument by examining a Vector Autoregression (VAR) model of the same series of retrospections, expectations, and approval. The VAR analysis complements our work because (1) it is based on a loose specification (it does not assume that the past gets translated into the future in a particular or simple way), and (2) it focuses on the variables' *innovations* (it isolates the effects of expectations that cannot be predicted by previous retrospections, previous expectations, or previous approval). We use the subjective psychological variables (rather than objective economic measures) so that the results will sort out the retrospections–expectations ordering. We want to know not the prior sources of economic information but, instead, whether people process economic information as retrospections or expectations. We wish to see whether the innovations in retrospections or in expectations drive approval.[16] We present a brief summary of the key results in Table A-1. (A fuller description is available on request.) Look first at column 1. The entries represent the proportion of variance in Approval that may be attributed to innovations in Approval, Personal Ret-

TABLE A-1

Variance Decomposition of Approval for Three-Variable Vector Autoregression Model

	APPROVAL[a]	
ENDOGENOUS VARIABLES	(1)	(2)
Presidential approval	.27	.59
Personal retrospections	.15	—
Business retrospections	—	.12
Business expectations	.58	.29
N	96	80

Note: Data are quarterly, from the secord quarter of 1954 to the second quarter of 1988. These estimates are based on a three variable VAR. The system estimation equations include, as exogenous variables (not shown), terms representing political events, the Vietnam war, and dummy variable shocks for the first month of each administration. See n. 5. Each entry gives the proportion of variance that may be attributed to innovations in each of the endogenous variables. The estimates reflect the system state after it has "settled down"—here after eight quarters.
[a]Each column represents a separate VAR analysis. Column 1 includes presidential approval, personal retrospections, and business expectations as endogenous variables; column 2 includes presidential approval, business retrospections, and business expectations.

rospections, and Business Expectations.[17] We see that innovations in Personal Retrospections comprise 15% of the variance in Approval, while innovations in Business Expectations comprise 58%. Clearly, Business Expectations dominate. When we turn to Business Retrospections (col 2), the case is similar: retrospections and expectations generate 12% and 29% of Approval, respectively; that is, of the variance in presidential approval that stems from the economy, the major portion starts not as retrospections but, instead, as expectations.

It is thus clear that innovations in expectations, quite independent of experience, contribute mightily to Approval. To be sure, both retrospections and expectations matter: we do not wish to suggest that experience is irrelevant. Yet when people digest information about economics and apply it to politics, information about the future carries special weight.

Notes

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1. In the main, we are interested in matters of intelligence. We here wish to be careful about pushing the term *rational expectations* too far. We do *not* wish to test the public's sagacity against the economists' technical standard of "efficiency"; that is, we do not expect to find that the public uses *all* available information in its forecasts. Instead, we wish to return to the more commonsensical notion of expectations formation. By *rational* we simply mean that when guessing about the future, the public considers information beyond that embedded in immediate past experience. In politics, rather than economics, this is the crucial distinction. The plausibility of the electorate's offering political responses to rational expectations about the economy has been the subject of some interesting debate (see Alt and Chrystal 1983; Beck 1991; Chappell and Keech 1985; Chrystal and Peel 1986). We wish to join, redefine, and enrich, that debate.

2. In a philosophical sense, causality is a complicated matter. Granger tests discover whether a particular variable helps "predict" future manifestations when all other variables are controlled. Passing this test accords with commonplace views of causality. Yet our data include one clear case of finding "Granger causality" when causality is ambiguous. Without a doubt, the economic expectations data may be said to "Granger-cause" future unemployment. It is possible to believe that this is so, since consumers adjust their behavior, and producers (knowing consumer forecasts) adjust theirs. It is also possible that this causal mechanism is missing and that consumers merely properly forecast the next quarter's—or the next month's—economic climate. Granger tests are also sometimes insensitive to cointegrated causal relationships. We interpret them with that caution in mind. (For a clear exposition on causality tests in political science, see Freeman 1983.)

3. We include in these estimation equations, as in all others, (1) dummy variables for each presidential term, (2) a series of variables representing dramatic political events, (3) the number of troops in Vietnam (during the Johnson years). These additions sharpen estimates and correct for potential biases associated with underspecification. See n. 5. We have employed a short "lag window" for our tests. This decision reflects a trade-off between a need to control for complicated autoregressive schemes in the endogenous variable and a desire for statistical power in the hypotheses tests. Considerable experimentation indicates that the exogenous variables' effects can be summarized with a very short lag length—one term does the brunt of the work. Accordingly, we have included on the right-hand side of each equation the previous two quarters' readings for the "dependent" variable (to wipe out most of the autoregressive structure) and the previous quarter for the candidate "causal" variables (to avoid adding noise to the hypothesis test). In order to minimize the difficulties associated with presidential turnovers, we have had to ignore the data for each president's first two quarters (for this Granger analysis only). A more conventional causality test—one that includes lags over longer periods (allowing for a more complicated autoregressive schemes)—produces a pattern of results very similar with one exception: it appears that the "objective" economy does not "Granger-cause" subsequent approval. Such evidence has been used to support rational expectations hypotheses (Chrystal and Peel 1986). In the end, the debate centers around the allowable complexity of causal schemes. Rather than concentrate on the technical matters surrounding such tests, we here wish to look for hints of causality and to establish a plausible causal sequence.

4. On the perceptual rationalization side, understand that the psychic processes that link presidential approval and expectations are most likely to be associated with steady-state emotional attachments (such as partisanship), sentiments relatively unaffected by month-to-month fluctuations in presidential approval. On the rational expectations side, recall that we model shocks to approval, that is, factors outside of the immediate past history of approval and economy. Our results are consistent with an understanding that these changes in approval reflect transitory performances on the political stage—that they are not the sorts of changes that revise judgments about economic management.

5. We have chosen to focus on a quarterly time frame because the Survey of Consumers economic perceptions data were gathered only quarterly (at best) until 1978. After that time, we averaged the monthly readings into a single quarterly reading. (We have replicated our analyses for the 1978–88 period with the monthly data and found no surprises.) Presidential approval data are drawn from Edwards 1990. Quarterly measures of approval are the mean approval of all Gallup surveys conducted for the quarter. Our "events" series is relatively slim, since we sought to include only events that left a major impact on the quarter's average reading and not just the temporary spike of a transitory rally point.

Positive events (+1) include the Geneva Summit in the third quarter of 1955 55:3, Cuban missile crisis 62:4, Moscow treaty 72:2, Paris treaty 73:1, Mayaguez incident 75:2, Camp David Accords 78:4, assassination attempt 81:2, and Granada invasion 83:4. Two negative events (−1) are Nixon's pardon 74:4, and Iranscam 86:4. Also, quarters 73:2–73:4 are coded as −1 events due to Watergate; Carter's Iran crisis is coded 2 for 79:4, 1 if 80:1, and −1 if 81:2. The event "effects" in the Approval equations are in the range of 6 points and highly significant. In every case, we have been careful to exclude information from the *previous* presidential administration from the Approval prediction equations.

6. To be sure, this Koyck-type model is a bare-bones version of a class of dynamic models. Given our present purposes, it does the job well enough. Our analyses of more complicated models do not suggest dramatic changes in theoretical inference.

7. We also tried unemployment level and first difference in inflation rate, but these versions of the economic variables did not contribute. Neither did per capita income growth. This variable performed poorly in all specifications and was dropped from the presentation. See also Kiewiet and Rivers 1985.

8. The political impact of Personal Expectations seems largely derivative. See nn. 12 and 17. Our long-term measure of Business Expectations also decisively wins a race with the short-term measure of Business Expectations (item 5 from the ICS). When the two are raced together in an Approval equation, our long-term measure wins, in terms of coefficient size, .13 to .02, and in terms of significance, .03 to .56. We explored several other specifications, and the dominant role of Business Expectations survived each. Among other variations, we eliminated the first year of every presidential term, we eliminated the final quarter of each lame-duck presidency, we deleted lagged Approval, and we substituted two-stage least squares for ordinary least squares.

9. This simple result is important for technical reasons. This model, by incorporating a "level" and a "change" specification on the right-hand side, approximates a "cointegration" model. The results provide stronger evidence that the relationship between Business Expectations and Approval is not spurious. For a discussion of these matters, see Engle and Granger 1987; Engle, Hendry, and Richard 1983; and Hendry 1986.

10. We employ the version of the Index of Leading Indicators that was replaced in 1989. Unlike the version of the index now in use, this older version does not include any direct measurement of consumer expectations. Scores based on the 1989 revision of the index incorporate the Survey of Consumer Attitudes' Index of Consumer Expectations (Hertzberg and Beckman 1989). Therefore, this newest version of the leading indicators historical time series cannot be used here. The quarterly measure of leading indicators is here lagged two months. For instance, for the fourth quarter, we use months 8, 9, and 10. This is to make the relevant time frame of the Index of Leading Indicators comparable to the other quarterly indicators. The monthly Index of Leading Indicators is not public knowledge until late in the following month. Monthly readings of current indicators are also reported on a delayed basis, but their measurement supposedly represents the experiential basis of economic information. The Index of Leading Indicators is a proxy for available forecasts.

11. Recall that in our *aggregate* analysis, personal retrospections do not reflect the idiosyncratic personal factors (such as illness or promotion or a firm's failure) that dominate cross-sectional work. Aggregate Personal Retrospections average out the idiosyncratic; fluctuations represent changes in the nation's economic condition as experienced by individuals.

12. We can also insert Business Retrospections on the right-hand side of the Expectations equation, on the grounds that perceived current business conditions account for business expectations independent of Economic News. But, like Personal Retrospections, Business Conditions are decidedly nonsignificant in the Business Expectations equation. Even when objective indicators (unemployment change, inflation, leading indicators) are deleted from the equation, neither retrospective variable achieves a significant relationship with Business Expectations. This result is as if, collectively, the electorate does not connect the future to the present. Retrospections are correlated with expectations—but seemingly only because past expectations, which are slow to change, predict current conditions. Also investigated (but not shown in Table 4) are equations for Business Retrospections and Personal Expectations. The former variable is largely driven by unemployment and leading indicators, with inflation playing a lesser role. The latter is strongly affected by inflation, but not by unemployment or leading indicators. When added to these equations, economic news matters for Business Retrospections but not for Personal Expectations. Both Personal Retrospections and Business Expectations contribute greatly to the prediction of Personal Expectations.

13. We can perform the same tricks when substituting Personal Retrospections for unemployment and inflation. For instance, the effect of Personal Retrospections loses its significance if future ($t + 1$) Personal Retrospections are included. Future retrospections are a substitute for current expectations.

14. Note the error correction mechanism implicit in the last equation tested in Table 3, column 3. This matter, of course, is far from fully explored.

15. Economists have long debated the role of consumer expectations in predicting economic growth. The empirical demonstration of a causal link from expectations to economic growth faces a special challenge. This is because residual effects of consumer expectations—unaccounted for by economic "fundamentals"—may be an artifact of omitted variables. For recent claims that consumer sentiment either affects (or uniquely predicts) the economic future, see Fuhrer 1988 and Matsusaka and Sbornone 1992. Whether causal agents or not, consumer expectations are commonly thought to predict economic change. Witness the Commerce Department's inclusion, starting in 1989, of the Survey Research Center's Index of Consumer Expectations (of which our Business Expectations is a part) as a new component of its Index of leading indicators.

16. The VAR models each endogenous variable as a function of its own history, as well as of the history of the other variables. (Here we use a history of four quarters, since longer ranges would lose many cases to missing data.) The least squares machinery estimates the endogenous structural parameters. The "variance decomposition" numbers stem from analyzing those structural parameters to find out what portion of the systematic variance in each variable may be attributed to innovations in each of the endogenous variables (including previous innovations in the dependent variable itself). Here, to save space, we show the variance decomposition of Approval only. (For expositions on VAR analysis, see Sims 1980; Freeman, Williams, and Lin 1989.) The actual estimation equations also include Vietnam Troops and dummies for Watergate, Iran, and a set of events (as in the regression equations shown in the main text). In addition, they include a set of "shocks" for each administration's first quarter (for which full data exist). For statistical purposes, these variables are treated as exogenous to the system.

17. These results show the state of the implied system after eight quarters. The effects of innovations begin to settle down in about this time frame; that is, looking at the system after a substantially longer period will produce similar results. Because retrospections and expectations covary within a single quarter and contemporary covariance cannot produce information about causal order, it is technically necessary to identify the simultaneous equations (of within-quarter sequences) by assuming that retrospections cause expectations or vice versa. Table A-1 shows the results assuming that within the quarter, retrospections cause expectations. In these analyses (but not generally) the two different assumptions produce substantive results within one percentage point of each other. Finally, for completeness, we replicated the analysis, having Personal Expectations compete with Business Expectations. Again, innovations in Business Expectations

dominates Approval's variance (by 55%, compared to 12% for personal expectations). It appears that most of Personal Expectations' political implications are derivative.

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THE PRESIDENCY AND ORGANIZED INTERESTS: WHITE HOUSE PATTERNS OF INTEREST GROUP LIAISON

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Studies of the relationship between the presidency and organized interests generally focus on presidential assistants and their communications with the interest group community. I take a different perspective. Based on presidential strategic interests and choices illuminated for several administrations through interviews with White House officials, four kinds of interest group liaison *are identified: governing party, consensus building, outreach, and legitimization. These approaches are then empirically evaluated for the Reagan White House using interviews with Reagan's staff and the responses of several hundred interest group leaders to 1980 and 1985 surveys of national voluntary associations. Like the Carter administration after its first year, the Reagan White House initially emphasized "liaison as governing party" built on exclusive and programmatic ties to groups. A less activist legislative agenda and new circumstances later shifted the emphasis of the Reagan and Bush administrations to other forms of interest group liaison.*

President Jimmy Carter discovered early on that achieving legislative success would require more than assembling comprehensive solutions to the nation's problems and simply offering them to Congress on their merits, even when both houses were firmly controlled by his own party. At first Carter was stymied almost everywhere he sought congressional action. Not only did his administration initially lack an overall legislative strategy and a means for identifying priorities, according to a senior Carter adviser, "in the first year we didn't have Anne Wexler—she came in later—who was able to mobilize the public interest groups in support of proposals."[1]

Anne Wexler's arrival in the Carter White House in 1978 brought the much-needed skills of a political insider, adding strategic depth to the Carter senior staff. But her entree to the White House as assistant to the president for public liaison also signaled a dramatic shift in how the presidency would interact with the interest group community. The White House staff now granted Carter's major legislative initiatives a new lobbying strategy that combined direct communications with members of Congress and the promotion of indirect pressures on Congress by White House–constructed coalitions of organized interests active in congressional constituencies. This new approach was designed to fuse presidential and congressional perspectives by transforming the goals and resources of like-minded interest groups into the political assets of the White House.

A well-developed set of instrumental presidential relationships with interest groups is not, however, merely the residual product of one administration's legislative difficulties. Previous administrations had practiced (though at a less sophisticated level) the art of strategic group politics; and once firmly established by the Carter White House, such a group strategy appeared to be a feature of the Reagan administration's efforts to move its 1981 legislative agenda of budget and tax cuts. Substantive interest group interactions with the White House of one sort or another have thus become a significant feature of the modern presidency.

I investigate the character of the relationships between national membership organizations and the presidency, as well as the reasons the relationships vary both across administrations and within a single president's Executive Office. Others have begun to address these issues, focusing on the derivation and formalization of interactions between the presidency and organized interests (starting with Polsby 1978). These works typically concentrate on presidential assistants and the specific roles they have played in communicating with the interest group world.

I take a different tack. I first construct a model of presidential–interest group exchanges that is rooted in the political and institutional environment in which presidents serve, the strategic choices they make to achieve their political and programmatic goals, and their capacity to regulate their interactions with outside constituencies in a way that fulfills their strategic objectives. The model yields a typology representing four ideal types of White House liaison with interest groups, reflecting the inclusiveness and programmatic content of purposive presidential interactions with the interest group community. I then offer an empirical test of the typology for the Reagan administration, examining whether the actual relationships with the Executive Office of President (EOP) reported by the executive directors of several hundred national voluntary associations in 1980 and 1985 are consistent with the president's strategic intentions for interest group liaison. (For a discussion of these surveys, see Walker 1991, app. A.) Interviews with two former presidents of the United States and White House officials from several administrations round out both the development of the typology and the assessment of the Reagan and Bush years.

I conclude that while White House liaison with

interest groups has grown dramatically since the presidency of FDR, only a fraction of national membership organizations enjoy an active and cooperative relationship with the White House. For the most part, the groups experiencing such involvement with the Reagan EOP fit the expectations of a presidential strategy of "liaison as governing party," mobilizing ideologically compatible groups to support the president's legislative program. The considerably restricted legislative agenda of the later Reagan years and early in George Bush's term, however, at least temporarily reduced the importance of this pattern of liaison and highlighted others.

A TYPOLOGY OF WHITE HOUSE LIAISON AND INTEREST GROUPS

For the sake of clarity, I distinguish between two terms for White House–sponsored interactions with interest groups. For the interactions of the overall institutional presidency with organized interests I employ the term *interest group liaison*, which encompasses all communications between representatives of the interest group community and the president or presidential staff serving in the various offices of the Executive Office of the President. I use the more specialized term *public liaison* to describe the assorted activities of the formal Office of Public Liaison (OPL), which since 1974 has had the explicit mandate of conferring with groups. Both types of liaison may involve varying presidential liaison strategies.

Crafting a model of presidential liaison with interest groups begins with the proposition that organized interests seek access to the president in order to advance their specific objectives. Nurturing at least the perception of influence with elected officials (especially the president) helps group leaders persuade their members of their effectiveness. As the modern presidency draws more policy issues directly into the White House domain, the opportunity for groups to secure explicit communications with the president or presidential surrogates becomes an increasingly precious commodity. Presidents thus have something of value to offer cooperative group leaders.

Presidents also want something in exchange. They desire group actions that aid in the fulfillment of presidential objectives. To manage such efficacious relationships with the interest group system, however, requires a White House establishment capable of orchestrating exchanges with groups that are conducive to promoting a particular president's interests. According to Terry Moe (1985), a similar logic explains the generic institutional development of the presidency. Moe notes that presidents introduce changes in the EOP—the institution over which they have the greatest control—to enhance the congruence between the support structures they inherit from their predecessors and their own incentives and resources to make executive agencies as responsive to them as possible (pp. 236–39). Presidents lack the authority, time, and resources needed to redesign entirely the organization of the EOP; but they can implement modifications that help them fulfill their political and programmatic objectives.

Presidential freedom of action with interest group liaison is partially restricted by the political costs incurred by violating the expectations of access associated with some groups and fostered by previous institutional arrangements. Nevertheless, as much as group representatives may press for entry to the Executive Office complex (and most will have an incentive to do so), the tight security surrounding the president, the capacity of presidents to pick and choose their objects of attention, and the formal decoupling of presidential campaigns from interest group money give presidents considerable strategic latitude in deciding whom to let in the door and whom to keep out (see my earlier work, Peterson 1992, 224–25; see also Greenwald 1977, 215; Polsby 154; Scholzman and Tierney 1986, 327).

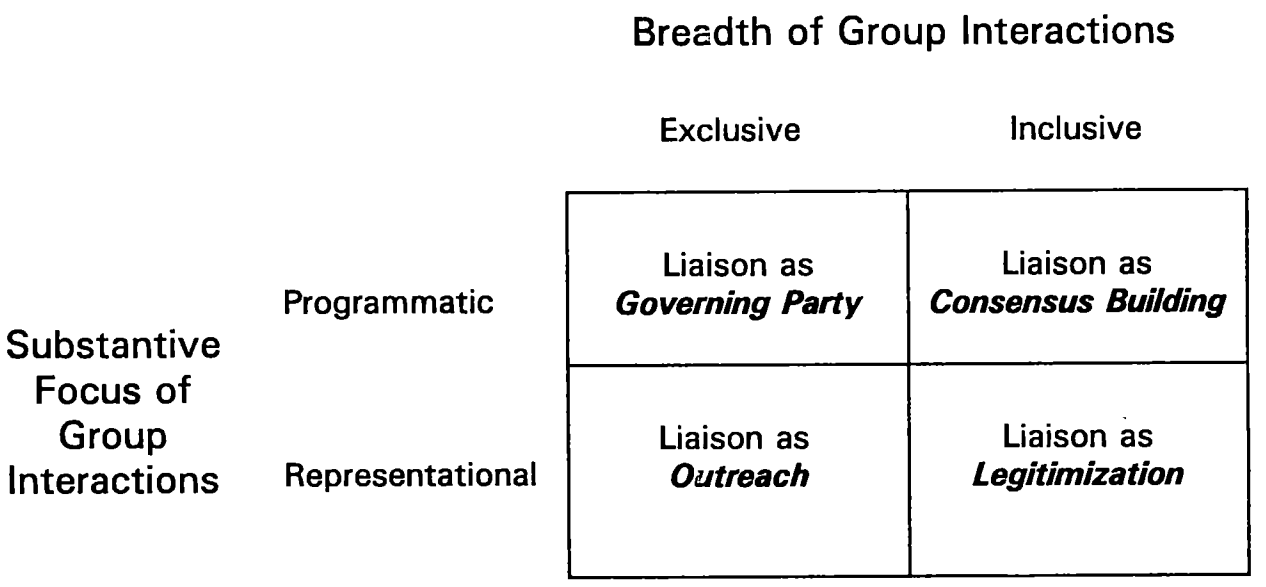
Designing an interest group liaison strategy requires presidents to make choices along two distinct dimensions. First, presidents and their staffs must determine the *breadth* of relationships to be cultivated with the interest group system, both as a general rule and for specific political or policy episodes. Interest group liaison could bring groups of all types into the White House, or it could limit the contacts to organizations ideologically compatible with the goals of the administration. The breadth of White House interaction with groups, therefore, can be either *exclusive*, targeted at a select and fairly narrow range of politically congenial organizations, or *inclusive*, reflecting a more representative cross-section of the variegated interest group community.

Second, presidents and their staffs must determine the *substantive focus* of the interactions with interest groups, yielding, for the sake of simplicity, two possible approaches: a *programmatic focus* and a *representational focus*. This choice distinguishes between policy and political motivations for interest group liaison. An administration with a programmatic focus works with interest groups to secure the passage or implementation of its policy agenda. Particular organizations attract the solicitations of the White House because of the influence they potentially wield in the legislative arena. A representational focus, on the other hand, emphasizes an administration's desire to reinforce its political standing, either among the president's electoral coalition or among group representatives at large. Attention is devoted to groups whose members are from constituencies in which the administration wishes to enhance the president's image.

Joining the two dimensions of breadth and substantive focus of group interactions produces the typology of four ideal types of liaison depicted in Figure 1. These ideal types are just that—simplified caricatures that are not likely to obtain in pure form during any administration but can be used to describe general approaches to, and actual broad patterns of, interest group liaison exhibited by the White House.

FIGURE 1

Typology of White House Liaison with Interest Groups



Actual presidential decisions about the breadth of relationships and their substantive focus rest on the institutional, political, and economic contexts facing each president and three sets of precursor choices presidents make to fulfill their objectives within the contextually defined constraints and opportunities (see Bond and Fleisher 1990; Edwards 1989; Mezey 1989; Peterson 1990; Rockman 1984; Simonton 1987). First, presidents must decide what they wish to accomplish, merging compatible objectives and resolving the trade-offs inherent in satisfying a multitude of objectives. Will they emphasize solidifying their political stature and reelectability or give first priority to programmatic accomplishments? Second, having settled on their objectives, presidents must organize the means for achieving their ends. What kinds of coalition formation strategies are best suited to the president's political or programmatic goals? Third, each president must select individuals to fill various positions in the administration, selecting appointments capable of fulfilling the primary goals of the administration while also accommodating the need to reward allies, pay political debts, and mollify defeated factions.

Reviewing situations in which each of the approaches to interest group liaison has been implemented by past presidents demonstrates how these approaches derive from the context of a particular president's tenure in office and reflect the political and organizational biases of the individual chief executive. Interest group liaison as *legitimization* (e.g., the organization-based complement of the White House Press Office's efforts to solidify the president's image and prestige among the general public) is closely associated with the initial motives for constituting the new Office of Public Liaison (OPL) during the Ford administration. The word *Public* here highlights the intention of employing interest group liaison to reach the broader public through the myriad groups to which much of the public belongs.

William Baroody, who sold the idea of a public liaison office to Gerald Ford and served as its first director, had a vision of liaison that was neither programmatic nor exclusive in character. President Ford's political stature and legitimacy as president were threatened by Ford's selection via appointment rather than election, Ford's pardon of Richard Nixon in the aftermath of the Watergate scandal, and the increasingly problematic policies for managing the Vietnam War and an economy in deep recession. As Ford put it to me, "my becoming president was totally different from any other of the thirty-seven that preceded me, so I took office under very unusual circumstances; and for the first four months, from August until January, it was primarily a holding action. . . . So it was unique, and I hope one never to be repeated." If Ford was to lead the nation free of the Watergate and Vietnam debacles, he needed to project an image that was inclusive and representational of a wide variety of interests in the United States.

Baroody planned to nurture the President's legitimacy by providing an open forum for communications between the White House and interests of all kinds (Kumar and Grossman 1986, 97). According to a member of the Ford public liaison staff:

> We had our own theory. We followed a different approach than the previous administrations, such as Colson in the Nixon administration, and the Anne Wexler approach in the Carter White House. . . . The Office of Public Liaison should not be a political office. *It should not be organized specifically to be a lobbying arm of the president.*

> . . . In the long run, it accomplished some of those goods, from the perspective of the President. [But] its primary function was to signify two-way communication—on the one hand, to communicate the President's position to various constituencies around the country; on the other, to have a route for those groups to communicate their ideas. (my emphasis)

The OPL generally avoided both legislative lobbying and involvement in the 1976 presidential election campaign, functioning more like a congressional caseworker (Copeland 1985, 26, 36). Given the fact that the economic and budgetary crises of the day led the President to avoid any new domestic policy initiatives, except in the area of energy policy, there was little need for programmatically based group ties (interviews with President Ford and his aides).

At other times, presidents have sought to establish fairly inclusive ties with the group system—but for programmatic rather than representational purposes. Examples of this interest group liaison as *consensus building* may be found in the early Johnson administration. Johnson was unquestionably programmatically inclined, perhaps even to the point of recklessness, especially during the 89th Congress when his own popularity was high and the Democrats controlled large majorities in both houses. He promoted innumerable policies and forced the largest domestic legislative agenda of the modern era on Congress without much consideration for the fine points of implementation and consequence (Peterson 1990, 95, 244). Although the mobilization of interest group coalitions by the White House was not as pervasive as in later administrations, Johnson's legislative program reflected consideration of constituency group desires, such as civil rights for black Americans, as well as "whether outside interests could be mobilized" (interviews with Johnson aides). As a senior domestic adviser to LBJ expressed it, "We were not above going to the Sierra Club and getting them mobilized, or going to various interest groups and getting them to help break up the [legislative] log jam."

Although Johnson would not shy away from confrontation, his preferred form of legislating highlighted harmony, not conflict. Diverse political perspectives were to be melded, not divided. Doris Kearns, reviewing LBJ's political life, linked this perspective to his approach to coalition building:

> He moved up in the world, but he never forgot the place where he had been born; he simply added new constituents to the ones he had originally served. Separate packages, separately designed for separate groups—this was the winning strategy as Johnson defined it. Thus Johnson built his career on a series of disparate layers; he added one incompatible constituency on top of another; he juxtaposed contradictory ideas without choosing between them. (1976, 391)

Johnson hoped to achieve the Great Society on a bed of broad political consensus. The special circumstances of Johnson's early years in the White House made that possible. As one domestic staffer commented, recalling the congruence between the White House and Capitol Hill that existed prior to the sharp divisions over Vietnam, "You know, the separation of powers is a phrase we have all heard. Well, at that time some of the separation got collapsed. It seemed we were all working on the same thing. We were mobilized together." And the programs themselves invited a consensus among disparate organized interests. Whether the policy involved housing, urban development, transportation, education, or any number of Great Society initiatives, the programs brought together otherwise potentially incompatible organizations representing all levels of government that received funds, public and private sector providers of goods and services, and program beneficiaries (Reischauer 1986). Until the economy soured and Vietnam polarized the nation, every group could have a piece of the action and be folded into the hybrid legislative coalitions, with few excluded on ideological grounds (see Redford and McCulley 1986).

The modern presidency is often portrayed as an institution where interests that are otherwise ignored have an opportunity to be heard. Some presidents have been particularly active, for example, in initiating redistributive programs intended to ease the plight of the poor, who do not figure prominently in the distributive politics so ingrained in much congressional decision making (Spitzer 1983). Interest group liaison can provide an avenue of access for similarly disaffected groups. A liaison strategy of *outreach* emphasizes providing representation to segments of the president's political coalition that otherwise lack close ties to the government establishment, sometimes to the point of bringing somewhat suspect "fringe" groups into the system. Instead of using the OPL as a means to build support for the president's legislative program, Midge Costanza, Jimmy Carter's first public liaison assistant, "was involved in symbolic interactions with groups, some of which, such as gay rights activists, had no previous White House access," hoping to make Carter's an "open presidency" (Kumar and Grossman 1984, 300; Pika 1985, 20). As Carter's legislative needs grew more pronounced, Costanza's approach drew less respect from the senior staff; and her office, figuratively and physically, moved further away from the President.

Finally, there is interest group liaison as *governing party*. Examining the staff structure surrounding the president, Hugh Heclo notices that modern presidents "did not consciously plan to create their own political parties. Yet that, in embryo, is what has come to exist in the White House. . . . If one were inventing a political party, these [White House offices] are exactly the types of offices at branch headquarters that one would want to create" (1981, 168–69). Many of the positions formally held by presidential aides are direct reflections of the growth of the interest group liaison function within the EOP, culminating institutionally in the establishment of the OPL. The White House comes to resemble a *governing* political party, however, only to the extent that its primary focus is that of partisan coalition building for the purpose of achieving programmatic goals.

Upon entering office, Jimmy Carter was none too fond of inviting "special interests" into the White House; indeed, he was quite disdainful of Washington politics in general and its embodiment in the rhythm of interest group lobbying (Kumar and Grossman 1984, 308; Jones 1988; Pika 1983, 316–17; Pika 1985, 19; Polsby 1983, 124–25). In those initial days of 1977, "Mr. Carter, not unlike President Nixon before him, evidently came to believe that he was a beneficiary of a system of governing genuinely free of mediation processes in the ordinary sense. He appeared to believe that contemporary circumstances sharply reduced the need to exhibit traditional presidential concerns with coalition building in order to govern" (Polsby 1983, 130).

"Contemporary circumstances," however, ultimately proved otherwise. While complaining that "when you don't measure up 100 percent to those so-called liberal groups [in the Democratic coalition], they demand a gallon of blood; there's no compromise with them," Carter realized that an important strategy for persuading Congress on legislation "is to bring in groups of potential supporters, and to let them use their influence on the member of Congress" (interview). As part of the cabinet and White House shake up that followed Carter's 1978 reassessment summit at Camp David, the OPL "was radically revised with the explicit purpose of serving" the legislative needs of the president (Pika 1985, 20, 26). It is not surprising that liaison as governing party finally emerged with considerable force during the Carter administration, once the "outsider" president had learned the lay of the political topography. Combining an ambitious legislative agenda with the increasing centrality of the presidency in politics and policymaking, the decentralization of Congress, the apparently diminished utility of institutional partisan ties, the expanding universe of interest groups, and a nation still polarized after civil rights battles, Vietnam, and lowered economic expectations, the emphasis on exclusive and programmatic liaison with groups was consonant with the goal of forging coalitions in a world of political disharmony and fragmented power.

Carter's public liaison staff soon became involved in every presidential-level issue; and Anne Wexler, the new OPL director, immediately set about to "lobby the lobbyists. . . to get a large constituency on an issue" important to the administration (interview with Carter aide; quote from Kumar and Grossman 1984, 302). Where active support could not be won, "we worked very hard to neutralize" likely opponents, such as the business community on synthetic fuels (interview with OPL staff member). Wexler herself often chaired the elaborate task forces assembled to organize and manage the campaigns to support Carter's major policy initiatives. The task forces typically included people from the policy, congressional liaison, press, intergovernmental relations, and political offices, both from the White House and the relevant executive branch agencies. Public liaison, as an instrument of interest group liaison, could not have been more programmatically oriented.

In the extreme, the involvement of the Carter White House with interest groups achieved almost "corporatist" proportions. An elaborate process of group consultation was initiated for budget development, as described by a senior Carter aide:

> We set up a task force with people from the OPL staff, the Domestic Policy Staff, and [Office of Management and Budget]. We then brought groups in, either together or alone. We'd ask them what their priorities were, and their justifications for their priorities. Reports would be sent to Wexler, [Domestic Policy Staff Director] Eizenstadt, and to [OMB Director] McIntyre, balancing the interest group politics, on the one hand, with the President's needs. After the budget had been tentatively determined, we brought them in again. We tried out some trial balloons to see what the problems were. McIntyre liked it so much the first year that he wanted to do it again in '81. For the budget director, there is a real paranoia that the budget numbers will get leaked. Well, leaks always occur; but they didn't have any impact. The groups backed the budget structure, and they already knew what was in the budget, so the leaks did not have a negative impact.

The group representatives were more ready to support the president's budget, even with some sacrifices, as a result of being brought into the process. Such was the state of interest group liaison at the close of the 1970s.

I shall now take this thematic approach and evaluate the interactions of the White House with the interest group system during the Reagan presidency. Two questions are considered: What does the contextual and strategic situation that greeted President Reagan suggest about the administration's likely primary approach to the interest group community? Are our expectations about Reagan's interest group liaison strategies (which are generated from the president's vantage point) confirmed by the actual interactions that took place between the White House and interest groups, as characterized from the vantage point of group leaders?

THE WHITE HOUSE AND THE REAGAN COALITION(S)

Ronald Reagan was the first Republican president in a generation to assume the formal powers of the presidency at a time when their reach could be augmented by a confluence of favorable political forces. The Democratic opposition was in disarray, the Senate passed from almost three decades of Democratic hegemony to Republican control, enough Republican candidates swept into office to make the House of Representatives subject to the will of a conservative majority, and the Republican party itself stood unified at the ramparts, ready to do battle with the "statist" legacy of the dispirited Democrats. But unlike Dwight Eisenhower, the last GOP president similarly favored, Reagan assumed office armed with

a simple but far-reaching programmatic agenda directed specifically at the institutionalized canon of the opposition party. The political environment in which Reagan entered office and the character of the goals he wished to advance had important implications for how his administration managed its relations with the interest group community.

Reagan and Liaison as Governing Party

In his approach to Congress, the courts, and virtually all aspects of politics, Ronald Reagan was arguably the most partisan president in modern American history. This partisanship extended to his perceptions of the interest group system, as well, leading to a two-pronged political strategy of crippling opponents and organizing the sympathetic. The administration launched an unprecedented and not entirely ineffective attack on interest group organizations that it viewed as both anathema to the interests of Reagan's policy agenda and representative of the inordinate influence the "liberal" establishment enjoyed in government (Peterson 1992; Peterson and Walker 1986; Wolman and Teitelbaum 1984).

For the second part of the administration's strategy, I explore the hypothesis that the Reagan White House also sought to achieve its primarily policy-directed objectives by securing the assistance of groups whose goals were compatible with those of the administration. The strategic position and purposive nature of the administration should have led to relations with interest groups that were exclusive and programmatically defined or reflective of interest group liaison as governing party. Since the aim of this kind of coalition building is to reinforce the president's political leverage over legislative coalitions in Congress, the White House should have sought to develop ties with sympathetic groups that had access to, and could influence, members of Congress through their districts. As components of an ersatz governing party, these groups linked the president and potentially allied representatives and senators by fortifying their policy convergence with the stimulus of real or threatened constituency pressure.

If this is a reasonable characterization of how the Reagan White House sought to engage the group system, then it should be manifest in the scope and nature of group involvement with the White House as retrospectively reported by the leaders of the membership organizations included in this study. Operationally, I hypothesize that four group characteristics were in evidence: (1) the White House interacted on a regular and cooperative basis with only a relatively small proportion of the available groups; (2) these select organizations expressed ideological positions on policy issues that were generally supportive of the goals of the administration and viewed the objectives of such a partisan president as relevant to their own concerns; (3) they possessed the kinds of organizational resources that made them productive members of a legislative coalition and were large enough—and endowed with sufficient and appropriate staff resources—to have a significant political impact; (4) they were politically active and motivated in ways that were helpful to the administration—specifically, giving some priority to legislative lobbying and perhaps indicating a willingness to be involved in electoral politics that would affect an organization's claim to access to a member of Congress. From the point of view of the White House, therefore, an ideal group to join the administration in liaison as governing party would have been a conservative organization, comfortable with the partisan orientation of the Reagan presidency, large and well staffed with individuals knowledgeable about legislative affairs, able to mobilize constituencies, and experienced in the art of lobbying the Hill.[2]

In the 1985 survey of national membership associations, group executives were asked, in separate questions, to "indicate whether this association communicates, consults, or interacts . . . frequently, occasionally, seldom, or almost never" with the OPL, other presidential assistants in the White House Office, the OMB, or any other office or agency within the EOP. From the responses to the four questions, I created a summary scale incorporating the highest frequency of interaction with any component of the EOP reported by each group. My interest here is not the average level of interaction between membership associations and the White House but, rather, the full extent to which at least some part of the presumably strategically coordinated White House operation penetrated the interest group world.

The Reagan White House, in fact, had regular, ongoing interactions with a rather limited number of groups in the sample of membership organizations. Only 11% of the groups reported frequent interactions with any part of the EOP. Moreover, just 8% of the associations perceived themselves as enjoying both frequent and "normally" cooperative communications with the White House. A third of the associations had no relationship at all with the White House, and many of the organizations experiencing some interactions found that the White House was "almost never" cooperative. This begins to confirm the anticipated exclusiveness of the relationship between the Reagan White House and the interest group community and also suggests that the modern presidency is not overwhelmed with interest group solicitors. That is not to say that the task before the White House staff was a simple one. Almost half of the organizations in the sample had at least occasional contact with the EOP, which translates into a considerable number of group representatives communicating with White House officials at some point during the course of the administration. That contact may be no more, however, than attendance at large, formal briefing sessions for scores of groups at a time.

The next issue is to determine whether the relative exclusivity of Reagan's communications with group leaders embodies the far more important characteristic of being related to the ideology and partisan standing of the groups. Is it the case that the more conservative a group's policy positions, the more

likely it was to work with the White House? The simple answer is a clear *yes*. Two-thirds of the groups who favored the most conservative position on the provision of federal services had at least occasional interactions with the Reagan White House; fewer than a quarter of the most liberal associations had such access. A similar pattern is found when groups are categorized according to their views on federal regulation.[3] Only 4% of the groups with very liberal views on the provision of federal services enjoyed frequent access to the EOP, compared to 28% of the very conservative organizations.

Another way to assess the ideological and partisan sensitivity of the relationship between the Reagan White House and the group system is to isolate the groups that both expressed fairly clear ideological positions and observed differences between the two political parties relevant to their own concerns. Using those two attributes, about a third of the sample can be labeled "party identifiers," based either on their views about the provision of federal services or their positions on federal regulation.[4] From this perspective there is no doubt about the link between partisan positions and relations with the White House. (The Gamma measure of association is .69 based on federal services, and .66 using federal regulation.) In the case of party identification based on federal services, 94% of the groups with virtually no White House contact are "Democratic." Of the groups with frequent interactions 61% are "Republican" organizations, a particularly dramatic figure, given the overwhelming proportion of "Democratic" groups on these issues. At the extreme, only 5% of the "Democratic" organizations experienced both frequent and normally cooperative interactions. For the "Republicans," a quarter reported such relationships; and none was either ignored by, or had uncooperative exchanges with, the White House. The pattern is generally the same for the partisan divisions based on regulatory policies: 88% of those enjoying frequent interactions with the White House were "Republican" in their orientation.

Sharing the policy positions of the administration does not necessarily make an interest group much of an asset to a programmatically inclined White House unless it is also well positioned to contribute to coalition building in Congress. Cheerleaders are always appreciated and may be rewarded with a few White House courtesies, but small, inconsequential groups are not as likely to be selected from the myriad of contenders for regular presidential attention. Even among friends, politics is often quid pro quo. To fortify its leadership role on the Hill, therefore, the Reagan White House, to the extent that it engaged in liaison as governing party, should have emphasized its relationships with groups that possessed significant resources applicable to influencing the legislative process. In some cases, that may have meant working with organizations less ideologically pure but nonetheless willing to support the administration on particular issues.

One of the most important resources available to an interest group are the staff members who constitute the working core of the organization. It is the staff that meets with White House officials, mobilizes the group's membership, and communicates with members of Congress. Larger staffs are also likely to exhibit more specialization, including among their ranks those with political and legislative experience. The probabilities are also enhanced that the group will possess more organizational slack not needed for its own maintenance and that can be committed to issues relevant to the president's coalition. Group executives responding to the surveys also tended to report more complete information about the number of staff members than about either the magnitude of their budgets or the size of their memberships.[5] The size of an organization's staff, therefore, can be treated as a surrogate for a variety of resources that would be significant in any calculus that the White House would use to evaluate the utility of groups in a coalition.

The 1985 survey reveals that staff resources affected interest group liaison independent of group ideology, although the effects are most pronounced for moderate and conservative groups. The tau-B values between resources and interactions with the EOP for conservative, moderate, and liberal groups are .22, .30, and .07, respectively.[6] Groups reporting the most frequent contact with the Reagan White House tended to be those with large staffs—over 70% for the conservative and moderate groups and 55% for the liberal groups.[7] Further, groups whose personnel had previous legislative experience typically indicated more regular access to the White House than those without such staffs. Fully half of the conservative groups communicating frequently with the EOP possessed large and legislatively experienced staffs; 51% of conservative associations that "almost never" interacted with the White House had both small and legislatively inexperienced staffs. Over 80% of the resource-rich conservative groups gained at least occasional access to the White House. And of all the organizations interacting frequently with the EOP, one-half boasted large, legislatively experienced staffs. Almost 60% of those were conservative groups by any measure, and only 10% were liberal.

Finally, for liaison as governing party, driven as it is by the programmatic needs of the president, interest groups devoid of a concern for influencing the legislative process, or prevented by law from congressional lobbying, are of little use to the White House. Therefore, Reagan and his aides should have initiated contact more frequently with the groups that gave a high priority to working with Congress in the development and enactment of legislation (or for the defeat of unfavorable bills). In addition, the impact of lobbying activity can be enhanced if groups have been willing or able to participate in the electoral process, seeking to assist legislative candidates with compatible political interests.[8]

The groups in the sample indicated on six-point scales how important (least to most) among their activities the lobbying of Congress and campaign

involvement were. Although these activity scales and the EOP interaction scale are not interval measures, I am assuming that they are sufficiently close approximations to permit the use of interval-level analysis and the reporting of Pearson-*r* correlation coefficients. The priority given legislative lobbying has a strong independent effect on the frequency of interaction with the White House for groups in each ideological category, with the values of *r* ranging from .37 for liberal groups to .54 for moderate organizations, with conservative associations in the middle at .42. Liberal groups may have claimed special consideration from the Reagan White House if their links to Capital Hill gave them some political purchase on the right issues. Campaign activity also demonstrated some impact, although it was considerably less than that of lobbying.[9] The values of *r* rest near zero (.03) for liberal groups and rise to only .20 for conservative outfits. It is not surprising, of course, that electoral pursuits did not advantage any liberal groups. Certainly, the Reagan administration looked askance at the notion of dealing too intimately with organizations that committed considerable time and resources to defeating the very members of Congress who gave President Reagan the influence he savored in Congress.

Other Forms of Interest Group Liaison in the Reagan White House

As depicted by the leaders of hundreds of national membership organizations, the relationship between the interest group system and the Reagan White House conformed in many respects to the dictates of liaison as governing party. If that approach to interest group liaison provided a *complete* picture of the link between groups and the White House, however, the surveys would not have also revealed the rather frequent communications with the EOP enjoyed by a set of liberal groups in the sample and by some of the conservative groups lacking resources or legislative relevancy. Quite clearly (as must be true of any administration in the modern era), the White House simultaneously engaged in other forms of interest group liaison. In the case of the Reagan White House, one can demonstrate that these were primarily liaison as legitimization and as outreach, each the respective product of two distinct causes: the evolution of the institutional structure and traditions within the EOP and the ramifications of personnel choices.

No matter what a president may wish to accomplish with interest group liaison, and no matter what general types of groups the White House would like to prevent from intruding on the internal processes of the executive branch, institution building and the emergence of political traditions conspire to deny the president complete flexibility when it comes to shaping and implementing a strategy of interest group liaison (see Moe 1985). Ever since aspects of interest group liaison became formalized in offices and staff positions in the EOP, certain categories of groups have benefited from specific representation in the White House establishment (Pika 1983, 318). For example, largely carried over from previous administrations, early in the Reagan administration specific White House aides had full-time, official responsibility for business, labor, and consumer groups, while informal ties linked the staff to groups representing women, blacks, Hispanics, ethnic Americans, the New Right, and the Jewish community. Similar but somewhat less intense attention was focused on the elderly, youth, and veterans (Pika 1982, 49). It does not take much imagination to understand that most of these "constituent" groups frequently have taken policy positions consistent with "liberalism" and that were contrary to the objectives of the Reagan administration. This legacy of institutional arrangements—too politically sensitive to challenge outright—gave some liberal groups access to the White House. Observing this pattern, Joseph Pika suggests that despite the recognized coalitional differences between the Republican and Democratic parties, when it comes to formal *public* liaison in the White House, "the similarities across party lines are striking" (1985, 6–7).

The presence on the White House staff of an individual with a portfolio of certain groups, however, says nothing about the *quality* of interactions those groups actually experienced. Only 44% of the liberal groups with frequent or occasional contact with the Reagan White House described those interactions as being "normally cooperative," compared to 63% for conservative groups. The panel data set provides further evidence of less-than-harmonious exchanges. The 1980 survey incorporated distinctions among groups active in several policy areas or representing various constituencies, including aging policy, as well as women, blacks, and Hispanics—all groups with special institutional access to the White House (number of cases = 49). Their reported frequency of interaction with the White House in 1985 is no different from the 1985 sample as a whole, despite the formal staff responsibilities for them. More importantly, of organizations with frequent or occasional contact with the EOP, only 38% found the interactions to be "normally cooperative."

The inclusion of some of the conservative groups in White House liaison efforts, on the other hand, highlights the importance of personnel decisions. Several of these groups gained entry to the White House without possessing the traits that would make them efficacious coalition partners. Their favored position resulted from the outreach efforts associated with particular individuals assigned to the White House staff; and this approach to liaison may have resulted from a substantively unrelated personnel selection process. According to a journalist who covered the Reagan White House, the OPL during Reagan's first year included hard-core conservatives whose views on liaison were contrary to those of Elizabeth Dole, the director of the Office. In an interview, he made the following argument: "I think she (Dole) found herself in a funny position, with no

power whatsoever. She [was] not allowed to hire her own staff, and she didn't trust or like them. They are all right-wingers. She felt she was a prisoner of her staff." These staff members placed a higher priority than others in the White House on granting access to groups on the "Right," regardless of their legislative utility, much as Midge Costanza during the Carter administration reached out to groups on the Left without weighing what contribution they could make to moving the president's program on the Hill.

Dole's departure to become secretary of transportation and her replacement by Faith Whittlesey and then Linda Chavez only accentuated the tendency of the OPL to view itself as the focal point for the advancement of right-wing issues and the accommodation of conservative organizations. One member of the Legislative Strategy Group (LSG, the core assembly of senior White House aides in the first term concerned with legislative policy development and enactment) suggested that Whittlesey, due to her ideological fervor, was less effective than Dole. As he put it, "The joke in the White House right now about Faith is, 'You want to talk to me about budget cuts, I'll talk about Central America. You want to talk to me about the tax bill, I'll talk about school prayer'. . . . As a result, she's just not in the power structure in the White House, and she is not very effective." Whittlesey may have remained out of the policy loop, but she could still offer an ear to the interest groups that shared the intensity of her views. The outreach approach taken by the public liaison staff, while serving other purposes for the president, complicated the search for signs of an overall pattern of interest group liaison as governing party.

Having argued that at least three forms of liaison were practiced by the Reagan White House, the question arises as to whether these different approaches to interest group liaison can be traced to separate units within the EOP. In the Carter White House, the OPL under Wexler was central to organizing support for the president's program, coordinating the governing party functions of liaison while others performed outreach or legitimization. Under Carter, the central features of interest group liaison were, in fact, accomplished by the formal public liaison staff. The legislative role of the OPL in the Reagan administration, however, was tarnished throughout Reagan's time in office.

When one examines the levels of interaction between groups in the 1985 survey and various components of the Reagan Executive Office structure, one fact is immediately apparent: the OPL—*the official agency designed to handle interest group relations with the president*—was no more likely to have contact with groups than were the other offices within the EOP. The busiest staff groups in the White House Office were those outside of the OPL. These individuals would include the chief of staff; presidential counsels; congressional liaison staff; the policy development, political affairs, and communications offices; and so on. Many were apparently engaged in extensive communications with programmatically effective and politically compatible organizations. Unexpectedly, the survey reveals that the OMB had as much contact with interest organizations as did the public liaison operation, undoubtedly a sign of OMB's central role in the development of domestic policy during Reagan's first term (Benda and Levine 1988; Stockman 1986).

Why was the role of the OPL so circumscribed? The earlier discussion about its ideological temper offers one explanation. Another stems from the idiosyncratic situation that characterized the initial year of the Reagan administration and the original organization of the White House. Not only was Elizabeth Dole weakened by her staff, she was also handicapped by the vicissitudes of a mixed personal and political life produced by her marriage to Senator Robert Dole. As the previously quoted member of the LSG pointed out in our interview,

> In 1981 on the tax bill, one of the major players was Bob Dole. OPL was not part of the LSG meetings. Elizabeth Dole was head of the office, and we were not going to give our strategies away or we weren't going to complain about Dole with Elizabeth there, talking about Bob doing this or Bob doing that. . . . [As a result of that and the previously discussed ideological problems,] with the exception of the right-wing issues in '81, that office was totally ineffective. With the business community in 1981, several of us in the White House were involved. On the tuition tax credits, that office was very important; but if you want to talk about the major issues, it was others of us in the White House.

As a result, according to a senior adviser, the OPL "was given its directions by Baker, [secretary of the cabinet Craig] Fuller, or [congressional liaison official] Ken Duberstein."

It is therefore not surprising that the non-OPL part of the White House Office was even more actively involved in liaison with groups than the OPL itself—and often for more programmatic purposes. One Reagan aide commented that Max Friedersdorf, head of the congressional liaison operation, "went to almost more meetings with outside groups [than] he did with the Congress." It also conforms with John Kessel's examination of the influence structures in the Carter and Reagan White Houses, in which he concludes that Wexler gave strength to OPL, while Dole, unable to overcome the institutional weakness of the liaison office, "was a nonperson on the Reagan White House staff" (1983, 459; Kessel 1984, 251).

Certainly, the public liaison office contributed to pursuing an overall strategy of interest group liaison as governing party; and the senior officials in the White House used the resources of the OPL to help organize group events and communications to advance programmatic objectives. But the non-OPL staff also had available to them a set of liaison resources outside of the OPL that facilitated their involvement in a policy-based interest group strategy, such as the mobilizing strength of the Chamber of Commerce (until relations soured after 1981) and the "steering committee" established among business associations that coordinated their support of

the administration (interview with White House journalist; Kumar and Grossman 1986, 98–99).

These overall distinctions among the *relative* roles played by various units within the EOP are also apparent in the kinds of groups with which they interacted, though these differences are muted by the complex imbrication of group relations with the White House. Almost every White House staff member in any administration has a set of previously established ties with groups. While in the White House, these aides maintain and use those associations, they interact with all kinds of group representatives regardless of their formal positions, and they often act as group advocates in the course of White House deliberations (Greenwald 1977, Pika 1985, 5; 212). That is the nature of the Washington political community, independent of whatever liaison structure may exist in the White House. Among the groups I examined, if an organization had any interaction with one part of the White House establishment, chances are that it also had some type of relationship with others in the EOP. For example, the Pearson's-*r* correlation between interacting with the OPL and other staff members in the White House Office is .62. The OMB is somewhat more autonomous, the OPL–OMB correlation being .36.

Despite these overlapping associations, there are some significant variations. Almost 30% of the organizations that had frequent contact with other members of the White House Office staff seldom, if ever, interacted with the OPL. Of those experiencing frequent interaction with the OPL, 12% rarely dealt with others in the White House Office. The distinctions are even more pronounced for the OMB. Of the groups frequently communicating with the OPL, 56% seldom or almost never met with officials in the budget office, while more than half of the groups most actively engaged with the OMB almost never communicated with the public liaison staff. These differences can be traced to the divergent parts of the interest group community being catered to by each office within the EOP and to how the different aspects of interest group liaison comported with their agendas.

Jack Walker developed a typology of membership groups that differentiates among associations based upon their ties to distinct occupational communities and their motivation by an idea or cause (1983, 1991). The typology includes four categories of organizations emanating from different sectors of the political economy: (1) groups whose members engage in the same profession or commercial activity in the profit-making sector of the society; (2) groups whose members are employed mainly by the governmental or private nonprofit sector of the society; (3) "mixed" groups, whose memberships come about equally from the profit and nonprofit sectors; and (4) citizens groups motivated by a cause or idea, whose members need not share any common occupational characteristics. (Trade unions are treated as a separate fifth category.)

There are noticeable differences in the types of interest organizations that maintained relations with various segments of the Reagan White House. The organizations that interacted frequently with the OPL constituted a much broader cross-section of the interest group world than was true of the other components of the EOP, including a fairly even representation of the profit (31%) and nonprofit (28%) sectors. More significantly, citizens groups were particularly prominent among those having the most frequent contact with the OPL, comprising nearly 40% of that category. (They constitute less than a quarter of the whole sample.) The citizens groups themselves, however, were a diverse lot, reflecting the contradictory impulses of liaison as outreach and liaison as legitimization emphasized by the public liaison office. Some were groups committed to conservative causes who enjoyed a cooperative relationship with the White House.[10] Other organizations with OPL contact were the liberal groups enjoying the benefits of institutionalized access. Even with this liberal representation, however, most of the liberal citizen groups reported that they seldom or almost never interacted with the OPL and that when they did, they found the White House to be less than cooperative. In the panel data set, 75% of these groups (organizations in environmental, consumer, and animal rights movements and supporters of social services and a less confrontationist foreign policy) reported that the election of Reagan generally disrupted their relationships with agencies throughout the government. In 1980, 57% had perceived an increase in cooperation as a result of Jimmy Carter's becoming president.

The situation for the OMB is dramatically different. Citizens groups were hardly counted at all among the groups interacting frequently with the budget office (8%), while a significant majority (59%) were trade associations and other organizations representing the profit sector of the society. Perhaps what we see here is a closer approximation of the traditional Republican coalition, made up of organizations representing business interests, large and in possession of considerable resources, conservative and Republican in the conventional sense. Their attention is focused on the brand of economic and regulatory issues that dominated the initial agenda of the Reagan administration, not the social issues pressed by the cause-oriented groups of the New Right.

More significantly, unlike the nascent organizations of the New Right, these groups had close ties to many parts of the government where officials preferred the quiet maneuvering of the backroom to the rollicking politics of open confrontation. From questions about the political activities of groups participating in the study, one can construct an index indicating the extent to which organizations emphasized the importance of *outside* political strategies, such as using the mass media and engaging in public protests, over *inside* political strategies, such as legislative and administrative lobbying. The scale ranges from −1.73 to +2.54, with negative values corresponding to a concentration on inside strategies (Gais, Peterson, and Walker 1984; Gais and Walker

1991; Walker 1991). In general, groups regularly involved with the EOP were insiders (−.56 on the index) while those with little or no White House contact clearly employed outside strategies of influence (+.63). But when one compares the groups with frequent or occasional interaction with the OMB to those that exhibited similar contact with the OPL, the insider quality of the OMB groups really stands out. They rate a −.69 on the scale, devoid of the rabble-rousers who move the mean for the OPL activists up to −.29. It should be noted, however, that the very fact of such extensive and politically based involvement with the OMB by even such insider interest groups is itself testamony to how the budget bureau has changed from an atmosphere of "neutral competence" to one of explicit responsiveness to the president (Berman 1979; Heclo 1975; Moe 1985).

A Multivariate Confirmation

A final test of the hypothesis that President Reagan pursued a strategy of interest group liaison as governing party can be accomplished by employing an ordered multinormal probit analysis to control for the simultaneous presence of the attributes I have examined. The results of the probit analysis, presented in Table 1, confirm the overarching thrust of the interest group liaison strategy pursued by the White House in Ronald Reagan's first term, as well as the variations in the liaison approaches associated with different components of the EOP.[11]

It is strikingly clear from the multivariate analysis that the conceptual elements required of the liaison as governing party approach were well represented in the Reagan White House's relationships with interest organizations. Across all units of the EOP, interactions were more frequent with organizations that demonstrated Republican partisanship (and were decidedly not unions), possessed extensive staff resources, and extensively lobbied Congress. Even the activities of the OPL conform in the aggregate to that expectation. However, an added measure of potential group resources, the existence of a decentralized organization with state or local subunits, proved meaningful only for the OPL and may have little to do with governing party activities.

The governing party thesis could easily have been disconfirmed by this analysis. Republican partisanship without a role for staff resources, lobbying activity, and contrary constituent groups would have signified liaison as outreach. An insignificant coefficient for partisanship combined with statistically significant positive associations for constituent and citizen groups (and perhaps labor unions) would have indicated liaison as legitimization. Insignificant values for partisanship and group types combined with positive coefficients for staff resources and legislative lobbying would conform with liaison as consensus building. The lack of any significant coefficients would suggest the absence of any interest group liaison strategy at all. None of these situations appears in Table 1.

TABLE 1

Probit Coefficient Estimates for Frequency of Group Interactions

GROUP ATTRIBUTES AND TYPES	INTERACTING OFFICE: ANY OFFICE IN THE EOP	OPL	OMB
Group attributes			
Republican partisanship	.481*** (.083)	.336*** (.086)	.318*** (.085)
Staff resources	.112** (.038)	.095** (.040)	.158*** (.040)
Decentralized organization	.079 (.089)	.200*** (.095)	−.087 (.095)
Active in congressional lobbying	.359*** (.029)	.212*** (.031)	.429*** (.035)
Active in congressional elections	.002 (.029)	.039 (.030)	−.069** (.030)
Group type			
Profit sector	.046 (.108)	.316** (.116)	.023 (.115)
Mixed profit–nonprofit	−.110 (.195)	−.100 (.222)	.181 (.205)
Citizen group	−.182 (.120)	.169 (.129)	−.326** (.129)
Constituent group	.137 (.201)	.386** (.205)	.081 (.211)
Labor union	−.649** (.286)	−.465 (.322)	−.562** (.320)
Number of cases	719	716	715
Percent correctly predicted	32.7	36.9	36.8

Note: Values in the table are ordered multinormal probit coefficient estimates. The standard errors are reported in parentheses.
*$p < .10$, two-tailed test.
**$p < .05$, two-tailed test.
***$p < .01$, two-tailed test.

The subtle differences across the units of the EOP that capture elements of the different patterns of interest group liaison, however, are revealed in the multivariate setting. The outreach and institutionally mandated legitimization activities of the public liaison office can be seen in the coefficients for citizen groups and constituent-based organizations. While there is a negative or statistically insignificant relationship between these types of groups and the frequency of interaction with the EOP as a whole and the OMB in particular, the coefficients are positive for the OPL. The involvement of conservative cause-oriented groups in Reagan's electoral coalition and their possible activity in congressional-level campaigns may explain the positive, although not statistically significant, coefficient linking involvement in congressional elections with OPL activity.[12]

The interest group associates of the OMB, on the other hand, fit both the model of liaison as governing party and the definition of consummate inside Washington players. Republican partisanship, staff resources, and lobbying Congress are essential attributes of groups' interactions with the OMB. The obvious politics of electioneering and the cause orientation of citizen groups, however, are negatively related to group involvement with the budget office. The coefficients for the types of groups interacting with the OMB show the importance of occupationally based associations in contrast to the citizen groups. During the first term of the Reagan administration there was no evidence of outreach and legitimization activities being conducted by budget office staff.

DISCUSSION

If the personnel in President Reagan's OMB were engaged in liaison as governing party (as they seemed to have been, in earnest), then it is a "governing party" well matched to the old definitions of the Republican coalition. The public liaison office, on the other hand, had the difficult task of pursuing the governing party strategy while simultaneously accommodating the interests of both those on the Right, historically denied ready access to government, and the liberal organizations whose fundamental tenets were at odds with the goals of the administration. I suspect that the non-OPL staff members located in the White House Office came closest to managing a version of liaison as governing party that reflected the particular coalitional alignment of Reagan's Republican party. It was more freewheeling than the business-dominated groups tied to the OMB but less buffeted by forces on both the Left and the Right that complicated the role of the OPL and undoubtedly diminished its intra-institutional influence.

If the questions about liaison activities had been asked in the 1980 survey, the results would have been similar in some respects and quite different in others. Given the programmatic orientation of the Carter administration, the attributes of liaison as governing party would have been as strong, but with "Democratic" partisan groups and citizen associations gaining the advantage. With the legislatively directed leadership of Anne Wexler as head of the OPL and the lesser presence of long-standing institutional ties of conservative constituent groups to the White House, liaison as governing party would have been rooted in the public liaison office without much distraction created by other forms of interest group liaison. Other members of the White House office would have reflected the outreach and legitimization efforts intentionally separated from the purview of the OPL (Scholzman and Tierney 1986, 326).

If the interest group survey had been administered again in 1990, the results would also have been somewhat different from 1985. By the second half of the Reagan administration, the institutional vitality of the OPL had diminished even further. With the return of fully divided government after the 1986 election, the replacement of pragmatist James Baker as chief of staff by former treasury secretary Donald Regan, the diminution of the administration's legislative agenda begun in 1982 but made complete, and the disclosure of the Iran–Contra affair, White House liaison with interest groups and its mechanisms were transformed. The public liaison staff lost their autonomy as they became an appendage of the White House Communications Office, directed by stridently conservative Patrick Buchanan. The mission of the public liaison operation shifted decisively to an emphasis on liaison as conservative outreach, not one of governing party or even legitimization.

The first years of George Bush's presidency have done little to improve the institutional situation of public liaison, even as the overall priorities of interest group liaison once again changed. The White House no longer has a director of public liaison with senior staff rank; and the OPL staff dropped to 15 people, down from the 56 in the Reagan White House (Kilberg 1991). The OPL director, now a deputy assistant to the president, remains within the fold of the Communications Office. However, Bobbie Kilberg, Bush's first OPL director, had a decidedly more pragmatist cast than her predecessors in the Reagan administration. She expressed the belief that groups representing all views should have access to the White House (Kilberg 1991).

As the political reach of President Bush has widened over that of his predecessor, and he has sought a more even balance between the programmatic and representational aspects of presidential leadership (submitting a few major legislative packages, such as the clean air bill and the drug initiative, while also nurturing ties to the Right and offering conciliation toward traditional liberal groups), interest group liaison functions therefore serve primarily, with some exceptions, the purposes of both consensus (perhaps compromise) building and legitimization (see Duffy 1989). One approach had even the president in separate two-hour sessions meeting directly with business representatives and environmental groups to discuss the Clean Air Act. The other had the OPL organizing, in a typical week, South Lawn functions for 900 educators, 1,600 Hispanics, and 262 German–Americans, as well as eight major policy briefings and five group photo opportunities in the Oval Office (Kilberg 1991).

As changes occur in the larger political context and in presidential needs or objectives, the character of liaison activities and their prosecution adjust accordingly. For now, they seem quite stable. Unless President Bush suddenly develops a more ambitious legislative agenda, especially in issue areas devoid of the potential for consensus, the Republican equivalent of Anne Wexler, steeped in the notion of governing party, will find little to do in the remainder of what some have called Ronald Reagan's "third term." It will take a future more activist Republican

president or a Democratic administration to create once again the impetus for change.

Because of the recent significance of liaison as governing party and the continued practice of the other forms of liaison, considerably more is involved here than simply tracing the activities of one White House office among many. As I have demonstrated, liaison between the White House and the group system ranges far beyond the duties of the OPL alone. White House liaison with interest groups writ large has become a central factor in the overall presentation of the president within the legislative process and within the polity as a whole.

Modern interest group liaison also has much to do with how we should interpret the dynamic relationship between interest groups and the party system and between the interrelated group and party systems and the society they together reflect (Peterson 1992). The president, as political party leader and as recently emergent facilitator of group coalition building, serves as a point of convergence for these institutions. As the grand (or at least incessant) speechmaker and increasingly a routine purveyor of group interactions, the president also serves to merge the mass public with the organized public. The interest group liaison capabilities of the White House, supplementing the president's traditional party ties and contemporary command of public attention, add to the instruments available for exercising presidential leadership in the modern age.

Notes

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1. From a personal interview. Many other quotes in the article are from personal interviews conducted by the author as part of a larger study examining presidential–congressional relations (Peterson 1990). Those interviewed include former Presidents Jimmy Carter and Gerald Ford, 63 individuals with experience in the Executive Office of the President in the 1953–86 period, 33 persons with experience on Capital Hill during the same period, and 9 journalists. Interviews were completed with three-quarters of those contacted. All respondents, excluding Presidents Carter and Ford, were promised that the interviews were not for attribution.

2. These characteristics are similar to the factors suggested by Pika as sources of variation in group proximity to the president, including their "degree of organization, experience in national affairs (sophistication), . . . availability of agency or political allies," and the much harder to measure "quality of leadership" (1985, 13).

3. The 1985 survey included questions asking the respondents to state the level of services provided by the federal government that their associations supported in 10 policy areas and the level of federal regulation that they considered appropriate in 9 policy areas. I combined the responses to these questions to create two summary ideological scales, one for the provision of services and one for regulation. Each scale has the categories *very liberal, liberal, moderate, conservative and very conservative*. The liberal and conservative labels may be somewhat misleading, however. A group favoring the present level of services in 1985, for instance, preferred a level of government involvement that was lower, as a result of the changes in budget priorities advanced by Reagan, than a few years earlier. The panel data set created from the 1980 and 1985 surveys shows that of the groups desiring the 1985 status quo in the overall provision of government services, 42% reported wanting less in the way of government services when they completed the 1980 survey at the end of Jimmy Carter's term. Fully 60% of those pleased with the "present level" of services in 1980 desired increased federal activity by 1985. The status quo, or "moderate," position in 1985 may be interpreted more accurately as a "conservative-leaning" position.

4. In the 1985 survey, the groups were asked to rate on a scale of 1 to 5 (*poor* to *good description*) whether their organization "perceives an important difference between the two major political parties on those issues most relevant to its goals." Responses of 3, 4, or 5 were treated as indicating a sensitivity to partisan politics. Those groups were then divided into "Democrats" (*very liberal* and *liberal*) and "Republicans" (*very conservative* and *conservative*) for each kind of federal intervention, with Republicans being defined as groups that favor less provision of federal services (except in the case of defense) and less federal regulatory intervention.

5. For those who provided information about all three resources, the product moment correlation between the number of staff members and amount of revenues is .72; between size of staff and membership it is about .45 (with all values logged).

6. The ordinal categories of each dimension used for generating the tau-B statistics are *frequent, occasional, seldom,* and *almost never* for interactions with the EOP and *large staff with legislative experience, large staff without legislative experience, small staff with legislative experience,* and *small staff without legislative experience* for staff resources.

7. The small–large distinction is made by dividing the sample at the point of the median staff size of 10.

8. The role of electoral activity, unfortunately, cannot be given an adequate test with the data from the interest group surveys. The respondent organizations are typically the tax-exempt nonadvocacy wing of multifaceted organizational enterprises. As a result, due to regulations restricting the activities of organizations possessing such tax status, relatively few groups (about a third in the sample) admitted to engaging in electoral activity. Only 6% gave it the highest priority, contrasted with the 47% who assigned that priority to legislative lobbying. The more useful information would be to determine which of the groups have affiliated political action committees and whether such affiliations make them more attractive to the administration.

9. See n. 8.

10. Undoubtedly this set of conservative "groups" is underrepresented in the sample, and would include a considerable number of nonmembership organizations not captured in the surveys. Many conservative groups probably also have access to the public liaison office through various attorneys and consultants, such as Robert Gray, who are the hired hands of representation (Berry 1984, 194).

11. The explanatory variables have the following values: *Republican partisanship* −1 = "Democratic" groups, 0 = neither party, 1 = "Republican" groups, based on the ideology and party questions discussed earlier. *Staff resources* 1 = small staff with no legislative experience, 2 = small staff with legislative experience, 3 = large staff with no legislative experience, 4 = large staff with legislative experience; small staffs have less then 10 full-time equivalents. *Active in congressional lobbying*

and *active in congressional elections* 0 = not engaged 1 = one of the least important activities, 5 = one of the most important activities (2–4 being continuum points). *Profit sector, mixed profit–nonprofit, citizen group,* and *labor union* are dummy variables created from Walker's (1983, 1991) group typology (with nonprofit sector groups excluded). *Constituent group* 1 = originally identified in the 1980 survey as a black, aging, Hispanic, handicapped, or women's group, 0 = all other groups (dummy variable). *Decentralized organization* 1 = has state or local offices, chapters, headquarters, or other similar subunits; 0 = does not have subunits (variable added to the probit analysis).

12. See n. 8.

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CORPORATE ELECTORAL ACTIVITY, CONSTITUTIONAL DISCOURSE, AND CONCEPTIONS OF THE INDIVIDUAL

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Recently, constitutional scholars have begun exploring how far constitutional discourse is governed by competing liberal and republican conceptions of the just or the good society. At the same time, political theorists have placed in doubt how far this debate rests on the metaphysical dichotomy between a radically autonomous and a radically encumbered individual. Scholars, however, have yet to investigate to what extent metaphysical conceptions of the autonomous or the encumbered individual underlie and determine strands of constitutional discourse. I analyze the deep structure of constitutional discourse in one important area—the legitimacy of prohibitions on corporate electoral activity. I show that the constitutional argumentation recurrently mounted against corporate electoral activity rests on a particular version of the radically encumbered individual and that the major constitutional defenses of corporate electoral activity assume a particular version of the radically autonomous individual.

Recently, contemporary political–theoretic debate between liberals and republicans has moved in two notable directions.[1] It has both entered the domain of constitutional scholarship and attenuated its connection with metaphysics.

Rejecting "any sharp division between legal and political . . . discourse" and moving beyond homogeneous liberal portraits of American political thought, constitutional scholars have begun asking "whether . . . 'republican' or 'liberal' conceptions" have actually been more "dominant in U.S. public acts in particular eras" (Smith 1985, 7, 6; see also Smith 1988, 90–91; Shklar 1990 and see also Michelman 1988, 1494–95 and Sunstein 1991a, 3. (More generally, see Ackerman 1984; Hirsch 1986; Horwitz 1987; Mashaw 1988, 1685; Michelman 1986; Powell 1988, 1706; Sherry 1986; Sunstein 1988, 1540). Contemporaneously, political theorists have begun questioning the relevance, for the liberal–republican debate, of the "stark" metaphysical "dichotom[y]" between "a richly socially constituted and embedded self, and an autonomous chooser maximizing preferences" (Rosenblum 1987, 153; see also Kymlicka 1989, 55; Neal and Paris 1990, 422). For many years, republican political thought was assumed to rest on a self "embedded in various social roles and communal relationships," liberalism, on a self "cut loose from all social ties, literally unencumbered, each one the inventor of his own life" (Kymlicka 1989, 9; Walzer 1990, 9). But with the publication of several recent essays by John Rawls (1975, 1985), several commentators have come to argue, along with Rawls himself, that liberalism does not presuppose a metaphysics and in particular, that liberal theory "should not be understood as presupposing a . . . metaphysical conception of the person as identified with the individual's capacity for rational autonomy" (Gutmann 1985, 313; Margaret Moore 1990, 460; see also Bernstein 1987, 543). And for their part, republicans now speak of selves only "partly defined by the communities we inhabit" and of "the capacity of the self through reflection to participate in the constitution of its identity" (Rosenblum 1987, 163; Thigpen and Downing 1987, 645).

As the liberal–republican debate in political theory has lately come within the compass of constitutional law, so the metaphysical conflict between the autonomous and the encumbered self has recently grown detached from the liberal–republican debate in political theory. I do not aim either to account for these twin evolutions or to speculate about connections between them. Rather, I am interested in pursuing a question centering on the third side of the political theory–constitutional law–metaphysics triangle. Does the "stark dichotomy" between an abstract and an embedded self, even if no longer pertinent to the liberal–republican debate among political theorists, nevertheless assume a role in the evolution of American constitutional law? Have different versions of the autonomous and encumbered individual—not simply (as many constitutional scholars are now beginning to suggest) liberal and republican theories concerning the good or just society—played a part in determining the course of recent constitutional discourse? In asking this question, I am pursuing a variation of the scholarly enterprise that analyzes competing constitutional views in terms of the theories they presuppose (Smith 1985, 6). But instead of showing which "versions of liberal or illiberal *political* thought are important strains in American constitutional discourse today" (ibid.; my emphasis), I will point, within constitutional discourse, to particular versions of (what might until recently have been called) liberal or illiberal *metaphysical* thought.

The question how far constitutional argumentation presupposes autonomous or encumbered conceptions of the individual cannot, of course, be answered exhaustively or conclusively.[2] Indeed, as Smith notes, if one is attempting a "qualitative interpretive narrative that shows [certain] structures of thought

and argument to be visible in [constitutional] texts," one can do so only piecemeal, by "[focusing] on a few major cases that seem representative or determinative of most judicial actions in a given area, instead of documenting how those structures are visible in all or most of the relevant cases" (1988, 91, 102, 104–6). Accordingly, I am going to examine how competing conceptions of the self—autonomous versus encumbered—have largely governed one important strand of recent constitutional discourse, namely, the debate over the constitutionality of limitations or prohibitions on corporate electoral activity.

DEFINING SOME TERMS OF DISCUSSION

Before examining the crucial role that metaphysical conceptions of the self have played in constitutional discourse over corporate electoral activity, it will be helpful to narrow the meaning of two key terms—*corporate electoral activity* and *constitutional discourse*.

The term *corporate electoral activity* refers to corporate contributions and expenditures in candidate elections and referendum campaigns. It is to be distinguished from other, broader (and less controversial) forms of corporate political activity, such as lobbying legislators, advocacy advertising, and so forth. Debate over these latter varieties of corporate electoral activity has not assumed a significant constitutional dimension.

The term *constitutional discourse* refers to both the opinions of judges *and* the commentary of legal scholars (Smith 1985, 5). Following Smith's suggestion that scholars of constitutional discourse focus on a few "representative or determinative" cases in a given area, the discourse I examine draws centrally (but by no means exclusively) on two major cases: *First National Bank of Boston* v. *Bellotti* (1978) and *Austin* v. *Michigan State Chamber of Commerce* (1990). In *Bellotti*, the Supreme Court struck down a Massachusetts law prohibiting direct corporate expenditures in referendum campaigns. In *Austin*, it upheld a Michigan law prohibiting direct corporate expenditures in candidate elections.[3] Across the vast range of the debate, however, few scholars and judges in fact draw any distinction between the two types of corporate electoral activity; the same arguments are marshaled for—and against—each. Indeed, the patterns of reasoning underlying *Bellotti* and *Austin* are flatly inconsistent; the very arguments that prevailed in the one were vanquished in the other. Judges and scholars thus depict the constitutional issue as one that concerns the essential legitimacy or illegitimacy of corporate electoral activity per se. Viewing it as a question that lends itself to resolution in toto, only one way or the other, each takes a side.[4]

Prior to *Bellotti* and *Austin*, the Court had used various procedural evasions to avoid "squarely addressing" the constitutional merits of the issue in its decisions (Birnbaum 1979, 164).[5] These procedural evasions, of course, did not stop individual judges from generating much substantive dicta in the area or scholars, much substantive commentary. More recently, with *Bellotti* and *Austin*, the Court has traded in procedural evasion for a kind of official equivocation: currently some forms of corporate electoral activity are protected, others not. Right along side the Court's official equivocation, however, individual judges and scholars have remained personally univocal. As a result, two cogent and conflicting schools of thought have emerged—on Court and off—concerning the legitimacy of corporate electoral activity. The unclarity of the Court's official record on the subject only testifies to the intractability of the issue and to the strength that these two schools of thought have shown over time. Together, they comprise the "constitutional discourse" that I shall examine.

I shall show that the deep structure of constitutional discourse over corporate electoral activity resolves itself into a fundamental metaphysical disagreement over the nature of the individual or the self. To see this, it will be necessary first to set forth in a schematic way the four most common arguments that judges and scholars have historically made against corporate electoral activity and to accompany them with the four responses habitually offered by those who defend the corporation's right to electoral involvement.[6] Of course, the disagreement between those who favor, and those who oppose, corporate electoral activity varies in nuance depending on the particular corporate form or instrument under discussion. A corporation may be large or small, widely held or narrowly held, nonprofit or for-profit; and it may engage in electoral activity directly or through a political action committee (PAC). These various possibilities raise considerations that I shall take into account. I shall then show that each of the four constitutional arguments made against corporate electoral activity rests, fundamentally, on a shared assumption that as political actors, individuals are essentially embedded in or constituted by their contextual—social and economic—roles, relationships, interests, and determinants. And I will show that each of the four arguments mounted in defense of corporate electoral activity rests, fundamentally, on a shared assumption that as a political actor, the individual remains essentially autonomous of—or unencumbered by—contextual surroundings.

CONSTITUTIONAL DISCOURSE OVER CORPORATE ELECTORAL ACTIVITY

The First Argument

According to the first recurring judicial and scholarly argument against corporate electoral activity, corporations possess a capacity to advertise, or "hype," their political views to an extent that no private individual or association can hope to match. Electorally active corporations will therefore necessarily exert a disproportionate—indeed, a distorting—amount

of influence in the political marketplace, drowning out the voices of those who argue competing positions (*Federal Election Commission* v. *Massachusetts Citizens for Life* 1986, 257; Sunstein 1988, 1576–77; Sunstein 1991a, 30). Wherever corporate electoral advertising is allowed, political ideas will no longer be judged principally by their merit but will, instead, gain acceptability in proportion to the loudness with which they are propounded (Cox 1980, 70; Cox 1982, 418).

Those who make this "distorting influence" criticism, it should be noted, explicitly confine it to large corporations and acknowledge that it applies with much less force to *small* corporations. Small corporations, after all, cannot deploy substantial economic resources in the political marketplace (*Austin* v. *Michigan* 1990, 661). On the other hand, this "distorting influence" criterion has been used to condemn not just direct corporate electoral activity but also electoral activity by corporate PACs. Electoral contributions and expenditures by corporate PACs, of course, draw on funds raised from the voluntary contributions of individual shareholders and employees. Electoral contributions and expenditures by corporations per se, by contrast, draw on corporate treasury funds. Nevertheless, it is frequently urged that the corporate PAC is just as capable as is the corporation itself of bringing large sums of money to bear in the political marketplace, drowning out the merits of competing positions (Wright 1982, 619).

The constitutional defenders of corporate electoral activity habitually respond that in forming their political views, most Americans are fully capable of discounting the economic throw-weight that corporations can place behind their political ideas. Most are mature enough to judge those ideas (along with all others) solely on the merits, unswayed by the wealth-bought hype behind them. As Justice Scalia flatly asserted in his *Austin* dissent "people are not foolish but intelligent, and will separate the wheat from the chaff" (*Austin* v. *Michigan* 1990, 695; 706). Justice Powell, in *Bellotti* urged that "the people in our democracy are entrusted with the responsibility for judging and evaluating the relative merits of conflicting arguments. . . . If there be any danger that the people cannot evaluate the information and arguments advanced [by corporations], it is a danger uncontemplated by the Framers of the First Amendment" (*First National Bank of Boston* v. *Bellotti* 1978, 791–92). Naturally, then, on such a standard any "legislation seeking to minimize the impact of material inequality on politics" must be seen as paternalistic—as "impugn[ing] the capacity of autonomous individuals to fend for themselves in the marketplace . . . of ideas" ("Corporation and the Constitution" 1981, 1854; see also Winter and Bolton 1973, 12). Arguments like Cox's "reflect a basic distrust in the capacity of individual citizens to discount the greater volume of political advertising in reaching their decisions" (Fleishman 1973, 455). Corporate electoral activity, its defenders thus assert, is not likely to addle the political judgment of individual Americans. Nor, they insist, can public opinion be moved in any appreciable way by the sheer firepower of great economic wealth (*Austin* v. *Michigan* 1990, 680, 705).

The Second Argument

According to the second recurrent argument against corporate electoral activity, the government, in granting the corporation special legal privileges such as limited liability, perpetual life, and favored treatment of assets, thereby "provides indirect assistance for any political expression corporations may undertake. This is a violation of the doctrine of [government] neutrality" ("Corporate Political Expression" 1978, 168; see also *Austin* v. *Michigan* 1990, 658–59). Whenever corporations involve themselves in politics, the government, notwithstanding the political diversity of the public it is obligated to represent, is thereby indirectly favoring some political positions (i.e., those of corporations) as opposed to others. It is bestowing society's collective privileges on—and is therefore helping to advance—political views that may well be supported by only a part—indeed, perhaps a very small part—of the public at large (*FEC* v. *Citizens for Life* 1986, 238, 267; *FEC* v. *National Conservative Political Action Committee* 1985, 490). To permit corporate electoral activity is thus to unfairly establish a "state-created amplification system, available only to a relative few" (Patton and Barrett 1981, 511). As Justice White urged in his *Bellotti* dissent, "Massachusetts could permissibly conclude that not to impose limits upon the political activities of corporations would have placed it in a position of departing from neutrality, and indirectly assisting the propagation of corporate views, because of the advantages its laws give to the corporate acquisition of funds to finance such activities" (*First National* v. *Bellotti* 1978, 809–10). The principle of government neutrality, out of a respect for American political pluralism, therefore requires incorporated businesses to stay out of electoral politics.

Of course (as its exponents acknowledge), the principle of government neutrality provides no grounds for objection to electoral activity undertaken by *unincorporated* businesses, which do not enjoy comparable government-sponsored advantages (Michigan State Chamber of Commerce 1989, 95a–96a). On the other hand, the principle has been used to condemn electoral activity by corporate PACs. True, the funds that the corporate PAC expends in political campaigns are not drawn from corporate treasury funds and hence cannot be attributed to any state-granted privileges. Nevertheless, the monies required to support the activity of corporate-PAC fund-raising itself, as well as the PAC's day-to-day operating expenses, are allowed to come from the corporate treasury. For this reason, corporate-PAC electoral activity, like direct corporate electoral activity, has been criticized for intruding the state-advantaged corporate form into the political process, with an attendant violation of government political neutrality (Mayton 1980, 380–81).

In response, those who defend corporate electoral activity have historically pointed out that many individual Americans (and not just corporations) receive assistance from the government, in the form of tax concessions, jobs, welfare, and the like. Just because the government advantages these individuals *economically*, however, we do not conclude that the government is somehow speaking through them *politically*, violating its neutrality and favoring their political views to the detriment of those who receive no government support. "Those individuals who form that type of voluntary association known as a corporation are, to be sure, given special advantages—notably [limited] liability," Justice Scalia argued in *Austin*, but so are other associations and private individuals given all sorts of special advantages that the State need not confer, ranging from tax breaks to contract awards to public employment to outright tax subsidies. It is rudimentary that the State cannot exact as the price of those special advantages the forfeiture of [their] First Amendment rights" (*Austin* v. *Michigan* 1990, 680). The connection between the various types of economic support that the government provides to individuals and associations and the particular political utterances they may issue is an attenuated one. Certainly, that connection is too attenuated for the government to be violating its political neutrality every time it hands out a welfare check or lets a contract to a politically active person or association. Indeed (so this argument would suggest), the government supports a great many such individuals and associations, each of whom may take a different political position. If it is true to say that the government speaks politically whenever it assists someone economically, then it is also the case that it is continually neutralizing any previous violations of neutrality.

It is important to explore this line of argument a little further. Defenders of corporate electoral activity, arguing by way of analogy, point out that as a society, we unproblematically deem a welfare recipient's or a government contractor's political expression to be his own and not the government's (see Shiffrin 1980). In similar fashion, we consider a hospital doctor's political expression to be personal, not corporate (Ashdown 1991, 782). To believe otherwise is to diminish the idea that all individuals possess fully developed, self-contained political identities that begin and end with them and that do not indirectly represent, embody, or implicate and are not determined by, the identities of those who support them economically. In its *Austin* brief, the American Civil Liberties Union urged in defense of corporate electoral activity that "an individual who is wealthy because he made a better mousetrap [cannot] be prohibited from spending [his wealth] on political speech, [even] if he is a Socialist and most rodent-hunters are Republicans" (1990, 13, 23–24). Similarly, a corporation that is wealthy because of state-granted privileges cannot be prohibited from spending its wealth on political speech, even if it supports Republicans and most citizens of the state are Democrats. The corporation and the state, like the mousetrap inventor and his customers, are free-standing political entities, whatever their economic links may be.

As a society, then (so it is urged), we believe it illegitimate to consider individuals' and groups' economic sources of support in assigning them rights and status as political citizens. Whatever their economic underpinnings may be, they are irrelevant to—and hence can place no constraints on—individuals' and groups' identities, privileges, and operations as political entities. And the same applies, it is argued, to corporations. "The benefits granted by modern government are so pervasive that Justice White's argument might justify control of most individual and group expression," the *Harvard Law Review* concluded in its review of the *Bellotti* decision; hence "the argument reaches too far and must be rejected" ("Corporate Political Expression" 1978, 169). For these reasons (so the defenders of corporate electoral activity habitually argue), we should not prohibit corporate electoral activity on the mistaken grounds that such activity represents a violation of government neutrality.

These first two constitutional disagreements surround the question of whether corporate electoral activity intrudes into the political marketplace (1) great private wealth and (2) the political power of government. As such, they are concerned with the effect of corporate electoral activity on the world external to the corporation, the forum in which society conducts its public debates. The final two arguments, by contrast, concern the effect of corporate electoral activity on the world internal to the corporation, that is, on shareholders and how they exercise their political rights.

The Third Argument

Those opposed to corporate electoral activity traditionally urge that whenever corporations take positions in campaign politics, shareholders as a whole are being made to contribute to political positions that only some of them (indeed, perhaps none of them) may support (*First National* v. *Bellotti* 1978, 812). In that they necessarily provide economic assistance, however unintended, for any electoral activity that management chooses to undertake, shareholders are in effect deprived of a measure of control over their own political expression. The regulation of corporate electoral activity, it is argued, then becomes requisite if we are to protect the "substantial interest each individual has in his own political activities" (*United States* v. *Pipefitters Local Union No 562* 1970, 407). For in finding themselves publicly associated with corporate electoral positions that they personally oppose, dissenting shareholders may well deem their own political voice, their own political support, to have been hijacked: "The group voice, by its very nature, tends to drown out the individual voice, and to identify the dissenter, in the public eye, with the views of the group he opposes" ("Comment" 1962, 789). Critics of corporate electoral activity have thus

repeatedly characterized shareholders as "captive speakers" (*Austin* v. *Michigan* 1990, 674–75; "Constitutionality of the Federal Ban" 1974, 153; "Political Speech" 1977, 1037). In a similar vein, Alexander Heard once observed that "membership in any organized group possessed of political interests commits an individual to indirect involvement in the political process, [and the] character of this involvement will be determined by those who dominate the group. There is no escape from politics for the individual" (1960, 210).

In short, with electorally active corporations, managers will continually "impose their political views upon a constituency of diverse convictions" ("Political Contributions" 1979, 416). To legitimize corporate electoral activity is to violate shareholders' rights to free political expression. It is to jeopardize their right to control their own political utterances and involvement and their right to support and be associated only with those candidates and causes they believe in.[7]

This argument, as well, has been modified and extended in a couple of noteworthy ways. To begin with, it does not apply with equal force across the domain of all corporations. As some of those who mount it acknowledge, it may apply with less force (or at least, less reliably) to certain types of closely held or private corporations than to widely held or public corporations (*First National* v. *Bellotti* 1978, 794–5). In the case of such closely held or private corporations, any corporate political expression is arguably more likely to represent the political voice of all shareholders. By the same token, political expression by corporations formed expressly for "ideological" purposes, such as the NAACP, has been deemed even less troublesome (*First National* v. *Bellotti* 1978, 805; *FEC* v. *Citizens for Life* 1986, 238). Yet while private or "ideological" corporations may not fall victim to this particular attack on corporate electoral activity, corporate PACs, despite the voluntary nature of their funding, *have* been attacked for misrepresenting the political positions of their members. Critics point out that the corporate PAC is usually oligarchical in structure. Managed, as it is, by corporate executives to serve corporate interests, the corporate PAC will necessarily take positions that some of its politically diverse contributors would oppose and about which they will very likely remain ignorant. Corporate PACs, it is thus urged, also inevitably misrepresent the individual political voices and collective political diversity of their contributors, just as electorally active corporations do those of their shareholders. As one legal scholar has put it: "The corporation can . . . retain 'control' of the PAC, with the authority to select candidates who will be recipients of PAC funds. . . . The participants are not 'pooling' their resources to support their consensus candidate. They are merely contributing in the blind; the corporation picks the candidate. The participants are not united by common political goals" (Mayton 1980, 391–92; see also Sabato 1984, xiv and Sorauf 1984).

To this argument, the defenders of corporate electoral activity typically respond that just because an individual may temporarily be associated with a corporation, having bought its shares for personal economic purposes, we do not, as a society, therefore habitually read into the shareholder that corporation's ongoing political views. We recognize that it is the nature of a free market economy that "all monies to some extent are diverted from their sources to other uses," but we do not routinely impute those uses to the sources, especially if there are "a number of steps in the diversion" (Ashdown 1991, 782; see also *Austin* v. *Michigan* 1990, 707). We traditionally regard the purchase of shares, even in an electorally active corporation, as a purely economic act, not a political gesture (Ashdown 1991, 783). We know it to be an exercise of economic calculation, not political speech; we understand that "the shareholder's primary motivation in investing is to obtain an economic benefit, not to make, or refrain from making, a political statement" (Prentice 1981, 639). We are thus capable of distinguishing any given individual's politics from those of the economic entities with which the individual might be associated—or, as one proponent of corporate electoral activity has put it, "the shareholder is not meaningfully . . . making a speech whenever the corporation acts politically," since "the corporate voice is not the individual's voice" (Associated Industries of Massachusetts 1978, 23). Indeed, most shareholders invest in a number of corporations, each one of which may take a different political position. If we believed that individuals always expressed themselves politically through their economic activities (so this defense of corporate electoral activity suggests) we would have to regard such individuals as politically incoherent.

It is thus understood that we do not express ourselves politically in everything we do; nor, especially, in every transitory, secondary, pecuniary association we make (Epstein 1969, 258). To believe that we do is to diminish those occasions on which individuals consciously, directly, and with real intent and meaning take political stands. As a society, we are committed to the idea that all citizens have fully developed political identities and voices to which their economic activities and associations are—and should be considered to be—irrelevant and immaterial (King 1962, 869, 874). Electorally active corporations thus do not "drown out" the voices of dissenting shareholders, identifying them with the views of the corporation. Individual shareholders' political voices and expressions are simply too strong and independent for that to be a concern. Their individual political "identities" remain clearly distinct from those of the institutions that surround them. When we are understood to express ourselves politically, it is full-throatedly through our various civic activities and conversations, not unintentionally and indirectly through our nonpolitical activities.

The connection between the shareholder's purely economic act of investment and the corporation's political utterances is thus simply too remote and indirect for us to deem the corporation to have

hijacked anything resembling the shareholder's voice. "Any argument that the . . . shareholder is being forced to speak when corporate funds are used for political expression is unsound," one proponent of corporate electoral activity has thus concluded, because the "use of corporate funds for such expression does not represent a personal endorsement of that view by the shareholder" (*International Association of Machinists* v. *Street* 1961, 740). As another has observed, "Various organizations, as well as governments, spend money in a manner opposed by certain members, yet this is not considered as the speech or expression of these individuals" (King 1962, 875). Money is not speech, it is urged, nor is investment endorsement; and when we act as shareholders, we do not act as citizens. We speak, endorse, and live our political lives elsewhere, in another sphere. Corporate electoral activity thus does not hijack shareholder political support and hence does not offend shareholder political diversity.

The Fourth Argument

According to the fourth recurrent argument mounted by the opponents of corporate electoral activity, shareholders typically invest in a corporation on the basis of its profitability. They thus acquire a significant economic "disincentive" for divesting should they disagree with any political stands it happens to take (*Austin* v. *Michigan* 1990, 653). Thus, the widely held electorally active corporation not only is likely to politically misrepresent various shareholders at any given time but will (assuming it is profitable) economically constrain them to remain with the corporation over time. To use Albert O. Hirschman's (1970) serviceable terminology, not only is shareholder "voice" co-opted by electorally active corporations, but shareholder ability to "exit" is curtailed. In a world of electorally active corporations, it is argued, shareholders' economic interests will become a drag on their ability to act freely and frictionlessly (i.e., on the basis of principle and conviction) in the political sphere. "Many individuals faced with the loss of a favorable investment," one legal scholar has written, "will choose to sacrifice their political ideals. Some will maintain their investments simply out of an inertia that may compromise even their most firmly-held convictions" (Nicholson 1980, 1005; see also Brudney 1981, 257–64). Or, as Justice White put it in his *Bellotti* dissent (*First National* v. *Bellotti* 1978, 818), "The State has an interest . . . in enabling individuals to exercise freedom of conscience without penalty."

This final argument has, like the others, been adapted to take into account the different forms and instrumentalities of electorally active corporations. On the one hand, one often finds it modified by the acknowledgement that electoral activity by *nonprofit* corporations—in which the politically dissenting shareholder who chooses to divest faces little or no economic penalty—may be acceptable even if electoral activity by for-profit corporations is not (see, generally, *FEC* v. *Citizens for Life* 1986). Corporate PACs, on the other hand, are frequently criticized for using the threat of economic "penalties" to extract and retain the political support of their "members"—that is, the employees and shareholders who contribute to them. Corporate PACs, in other words, are criticized not only for being politically oligarchical in their structure but for being economically coercive in their practices, as well (see, e.g., Sabato 1984, xiv, 64–66). The corporate PAC is said to confront both employees *and* shareholders with the threat of suffering financially in a variety of indirect ways if they do not surmount their contrary political convictions and contribute: "Employees do not make [PAC] contributions voluntarily," it is said, "but out of 'fear of disfavor' or 'hope of gaining in employer favor' " (Handler and Mulkern 1982, 58). And although "shareholder volition in making contributions is not as easily coerced as employee volition," PACs nevertheless pressure shareholders to contribute by appealing to—even manipulating—their concerns about their economic interests (Brudney 1981, 272). "When corporations . . . are permitted to solicit contributions from their . . . members" through the PAC system, it is argued, "the danger of coercion arises" (Vandegrift 1980, 470).

Defenders of corporate electoral activity have traditionally responded that as political actors, individuals are quite capable of transcending and even flouting their own economic interests (American Civil Liberties Union 1990, 20). They are capable, in other words, of acting freely and frictionlessly on the basis of political conviction even when faced with an economic disincentive for doing so. Thus, even supposing that corporate electoral activity politically misrepresents dissenting shareholders, those shareholders can easily leave: "The shareholder invests in a corporation of his own volition and is free to withdraw his investment at any time and for any reason"; the corporation's members "voluntarily join it and can voluntarily get out of it" (*First National* v. *Bellotti* 1978, 794; *International Association of Machinists* v. *Street* 1961, 788). These comments affirm a belief in the shareholder's capacity to act—politically—completely on personal volition notwithstanding any countervailing economic interests. According to this belief, it is simply false to say that an individual's shareholding in an electorally active corporation rises to the level of meaningful coercion (Associated Industries of Massachusetts 1977, 23). Selling one's stock for political reasons, Justice Scalia argued in his *Austin* dissent, thus "does not ordinarily involve the severe psychic trauma or economic disaster that [the Court's] opinion suggests" (*Austin* v. *Michigan* 1990, 687). The ability to act independently and voluntarily in the political sphere remains as undiminished for the shareholder as for any other citizen (Prentice 1981, 639). Allowing corporate electoral activity does not mean that shareholders' political convictions will be compromised by their economic interests.[8]

EMPIRICAL SOCIAL SCIENCE AND CONSTITUTIONAL DISCOURSE

Taken together in all their manifold forms, the four disagreements discussed essentially comprise constitutional discourse over corporate electoral activity. Before showing that a particular disagreement over the nature of the self lies at the root of that discourse, however, it is important to address the relationship between constitutional discourse over corporate electoral activity and recent empirical work in the area. One might want to know, for example, whether empirical scholarship can help resolve (or explain) some of the four constitutional disagreements over corporate electoral activity. It is also worth asking whether the study of constitutional discourse over corporate electoral activity might fruitfully reorient (or complement) various strands of empirical scholarship. Here, two bodies of empirical work, in particular, are pertinent: empirical studies of corporate electoral activity and empirical studies of the beliefs and motivations of those, such as judges, who dominate constitutional discourse.

At best, empirical studies of corporate electoral activity currently bear an attenuated relationship to constitutional discourse in the area. In principle, at least some—and perhaps all—of the four constitutional subdebates over corporate electoral activity are amenable to some kind of empirical resolution. As a practical matter, however, "the new attention to the corporation by political scientists has not yet produced a rich stock of empirical knowledge about what business firms do in politics" (Eismeier and Pollock 1988, 2). Empirical scholars disagree over the extent to which corporate electoral activity in actuality "distorts" the workings of the political marketplace, for example, just as they contest the extent to which corporate PACs effectively "coerce" potential members into contributing (see, *First National* v. *Bellotti* 1978, 780; Handler and Mulkern 1982, 57–58, 97–98; Shockley 1985). Even if some of these questions were to be resolved empirically, it is by no means certain that the Court would pay much heed. As Sorauf observes, "The Court [has] never developed ways of relating its doctrine [in the area of corporate electoral activity] to realistic burdens of proof. It set—and allowed trial courts to set—standards of empirical proof for the governmental interests behind legislation that were beyond reason and beyond the standards of informed scholarship" (1986, 119). More generally, "those who study and discuss constitutional law almost never bother with such empirical questions," even though "what they say almost always assumes answers to them" (Sunstein 1991b, 36; see also Segal 1986, 941, 953). To explain and account for judicial and constitutional-scholarly ideas, then, we need to look elsewhere—not in the correspondence of those ideas to the state of empirical knowledge in any given field but in their coherence with deeper ideological preconceptions.

None of this means, however, that there cannot in principle be a fruitful complementarity between empirical scholarship and constitutional discourse in the area of corporate electoral activity; for the deeper, more determinant ideological preconceptions undergirding constitutional discourse may, themselves, be amenable to empirical investigation. The interpretive analysis of constitutional discourse over corporate electoral activity—in particular the exposure of its deepest speculative assumptions—might, thus, usefully redirect some empirical energies. In reconstructing constitutional argumentation, scholars of discourse lay bare the key (and often unexpected) empirical premises assumed by judges and legal scholars. In so doing, they signal to social scientists a new set of questions that must be empirically addressed, if we are ever to move a particular strand of constitutional discourse forward (or at least evaluate it fully). In this case, we might learn that in addition to addressing the question of what "business firms do in politics" per se, empirical scholars might devise ways of validating or refuting less readily apparent—but more fundamentally determinative—metaphysical preconceptions about the nature of the individual-in-society.

There is also a complementarity of sorts between the interpretive study of the deep structure of constitutional discourse, and the social-scientific study of the core beliefs or motivations of individual judges (Segal and Cover 1989, 559; Tate 1981, 365). As Smith observes, "A wealth of . . . internal psychological factors and personal interests . . . influences all political conduct, including the judiciary's"; yet "the behavior of political actors . . . is influenced in part by the nature . . . of the ideas they possess, and the basic ideas of a given period and group often have a discernible structure, which may be articulated in revealing fashion by political writers of the day" (1985, 6–7). This statement is particularly true when the political actors in question are constitutional jurists and legal scholars; for the "very thing that makes constitutional discourse distinctive, its unusually self-conscious effort to connect decisions on immediate conflicts with . . . first principles of the political order, means that if the approach can be usefully applied anywhere, it should be of value here" (ibid.; see also Heymann 1990, 89; Mark Moore 1990, 79). Smith argues that "qualitative studies of the patterns of reasoning characteristic of various strains of legal discourse" can constitute independent "investigations into one dimension of political conduct—a dimension that needs to be assessed like any other if we are to build up a comprehensive empirical portrait of political life" (1988, 90).

Smith's comment suggests that interpretive studies of the deep structure of constitutional discourse and social-scientific studies of the "core belief systems" of individual judges can be viewed as mutually complementary empirical endeavors. In analyzing the deep structure of constitutional discourse over corporate electoral activity, for example, the empirical issue at hand is not whether, as a matter of "internal psychol-

ogy," individual judges and commentators actually hold to the conceptions of self that their arguments assume. (That is an important, but a separate, issue.) Instead, the empirical issue is whether, at the rhetorical level, judges and commentators publicly justify their arguments by resting them on such conceptions. If so, then to the extent that one or the other of these conceptions is vulnerable to challenge, so is the edifice of argumentation (whether for or against corporate electoral activity) that it supports.

In short, the extent to which constitutional ideas gain acceptability cannot always be explained either by their relative empirical or rational validity or by examining their appeal to judges' particular interests and psychological makeups. Often, their careers can be accounted for only by exploring how far they cohere with deep "ideological structures" (preconceptions and assumptions) concerning aspects of political life (Smith 1988, 104). On the other hand, an interpretive study of those preconceptions and assumptions can usefully complement the social-scientific study of judges' interests and motivations. It can also usefully expand the social-scientific study of the policy questions that come before the courts—in this case, to include the validation of different "empirical assumptions" concerning the individual-in-society.

THE AUTONOMOUS INDIVIDUAL AND THE ENCUMBERED INDIVIDUAL

There is, at bottom, a single thread—a single ideological underlay—connecting the four recurring judicial–scholarly attacks on corporate electoral activity (the electoral activity of large, incorporated, widely held, for-profit businesses), just as there is another such thread connecting the four basic defenses. At the most basic level, constitutional discourse over corporate electoral activity bespeaks something beyond a set of broad disagreements over the nature of the institutions in question (corporations, in particular, but also the political marketplace, government, political associations, and the economic marketplace). Instead, it takes the form of a very defined and persistent disagreement over the conceptual nature of the individual. More exactly, constitutional discourse over corporate electoral activity resolves itself into a disagreement over the conceptual nature of the individual as revealed in its interaction with these various social institutions.

To see this, consider first the conception of the individual underlying the arguments mounted by those who would defend corporate electoral activity. Time and again, judicial and scholarly defenders of corporate electoral activity have depicted an individual who develops political views largely—and admirably—independently of economic considerations. The individual's political opinions cannot be swayed by the economic throw-weight that others might bring to bear behind theirs. Rather, contrary to the fears of Archibald Cox, the individual determines personal politics on the merits. Nor are the individual's political positions hostage to personal economic interests. Instead, contrary to the fears of Justice White, the individual bases political views on conviction and acts accordingly. Large corporations, thus, cannot control the individual's political views with their wealth; nor, as a shareholder, will the individual who opposes its politics have difficulty disassociating from a profitable corporation. When acting politically, the individual is neither so mentally "foolish" as to be controlled by corporate economic power nor subject to "psychic trauma" due to personal shareholder economic interests but "autonomous" of both (*Austin* v. *Michigan* 1990, 687, 695; "Corporation and the Constitution" 1981, 1854). Merit and conviction thus hold sway for the individual; as a political actor, the individual is in no way encumbered by the economic power of others or by particular personal economic interests.

Additionally, those who favor corporate electoral activity have depicted an individual with a developed political identity, one separate and distinct from anything that might be implied about it by any institutional ties or associations. The government's economic support does not mean that the individual has therefore become a mere cipher through which the government speaks. The individual who supports a corporation economically does not thereby have political voice and identity co-opted by it; the individual cannot be construed as either identifying with, or espousing, its views politically.

Those in favor of corporate electoral activity thus posit that Americans, as conceptual individuals or selves, are autonomous and self-constituting political entities, capable of resisting others' economic clout and rising above their own personal economic imperatives. And they posit that Americans, as individuals, possess autonomous and self-contained political identities capable of being conceptually distinguished from their surrounding institutional ties and affiliations. There is an ideological commonality to these beliefs about the individual: together, they begin to approach the conception of the starkly autonomous or abstract self that (at least in some respects and until recently) had been associated with the liberal position in contemporary liberal–republican (liberal–communitarian) political theory debate. It is a self whose political life transcends and remains essentially distinct from its socioeconomic context. (By *socioeconomic context* I mean what would otherwise be a set of constitutive associations and roles and determinative powers and interests.) It is "an individual abstracted from context," a self "identified with a transcendent, autonomous capacity," "standing always at a certain distance from the interests it has," "literally unencumbered, the inventor of his own life" (Hinchman 1990, 759; Margaret Moore 1990, 461: Sandel 1982, 62; Walzer 1990, 9). Whatever its links to defenses of liberal political theory may be, the autonomous, abstract, or transcendent individual is, in its various

aspects, *the* central character in the defense of corporate electoral activity.

Those opposed to corporate electoral activity, by contrast, have rested their four main arguments on what, taken as a whole, amounts to a very different view of the individual's defining virtues and features. The individual's political views, they fear, may often be determined more by the firepower of wealth than by the merits; the individual's political actions, more by pecuniary interest than by conviction. And, the individual's political identity, they believe, is essentially implicated in, and remains indistinguishable from, institutional associations and roles—in particular, socioeconomic links and activities, whether those of government beneficiary or corporate shareholder. Such a self, in its embeddedness and encumberedness per se, is reminiscent of the more situated metaphysical conception of the individual that, in gross terms, had been associated rather more with the republican position in contemporary liberal–republican political theory debate. It is—perhaps more accurately—simply an "illiberal" or even an "antiindividualistic" conception as George Kateb uses the term (1989, 183–84; Smith 1985, 6).[9] In the words of the political theorists who have examined it, such an individual is variously conceived as "never completely abstracted from the institutions of . . . particular communities," "embedded in . . . social roles and communal relationships," not distinguishable from the interests it has, and "engulfed by social . . . determinants" (Gill 1986, 126; Kymlicka 1989, 9; Sandel 1982, 62; Thigpen and Downing 1987, 646).[10]

CONCLUSION

Recently, constitutional scholars and political theorists have shown an increasing interest in ascertaining the presence of various ideological structures within constitutional–legal texts, both cases and commentary. I have offered a qualitative narrative that shows that certain structures of thought concerning the conceptual nature of the individual have fundamentally shaped constitutional discourse in the area of corporate electoral activity; for apart from the corporation itself, the "individual" is the only other pervasive conceptual presence in constitutional discourse over corporate electoral activity. When cast at the level of the corporation, however, the four main issues raised in discourse on corporate electoral activity show little relationship one to another: Does corporate electoral activity distort the political marketplace? violate government neutrality? hijack shareholder political expression? economically coerce shareholders? By contrast, the more basic preconceptions of the individual as they animate each side of the four subdebates can be seen to form parts of a coherent whole: an autonomous or abstract individual on the part of those who defend corporate electoral activity, an encumbered or embedded individual on the part of those opposed.

Indeed, it is important to note in passing that not just the concept but also the word *individual* dominates the discourse I have discussed. Defenders of corporate electoral activity variously assert, as fundamental axioms, that "autonomous individuals can fend for themselves in the marketplace of ideas," that "the corporate voice is not the individual's voice," that "private individuals [who are given] special advantages" by the state do not thereby become its instruments, and so on. Opponents of corporate electoral activity, by contrast, rest their case on such assertions as that "membership in any organized group possessed of political interests commits an individual to indirect involvement in the political process," "individuals faced with the loss of a favorable investment will choose to sacrifice their political ideals," and the like. At its fundaments, the disagreement between constitutional proponents and opponents of corporate electoral activity is, quite explicitly, a debate over the conceptual nature of the individual-in-society.

Both sides in constitutional discourse over corporate electoral activity thus advance as root ideological assumptions and assertions starkly competing conceptions of the individual-in-society; in neither case does anything further lie beyond or beneath. The links between these two metaphysical conceptions of the individual and the reigning political theory debate between liberals and republicans are complex and obscure, if not entirely eroded. Nevertheless, they do govern at least one important strand of constitutional discourse. As such, they belong to the "patterns of thought" that political theorists and constitutional scholars should now be searching for in the deep structure of constitutional debate.

Of course, if we are to draw any general conclusions about judges' use of metaphysical conceptions of the individual, we must begin by analyzing Court decisions in a number of areas in terms of their reliance on various versions of the autonomous and encumbered selves. The fact that these preconceptions are so fundamentally determinant to a subject as seemingly unrelated to them as corporate electoral activity, coupled with their status as axial ideological constructs, suggests that there is no reason ab initio to delimit the range of constitutional questions where one might expect to find them playing an important role. I conclude that scholars might fruitfully begin exploring the extent to which metaphysical conceptions of the individual, not just various political theory conceptions of the good or just polity, govern judicial doctrine in other areas of constitutional discourse.

Notes

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1. Whatever distinctions may exist between the terms *republican* and *communitarian* (or, for that matter, between republicans of past eras and contemporary republicans) is not pertinent for my purposes. To the extent that the debate between liberals and Republicans is material to what follows, it is the *contemporary* debate only—in which republicans–communitarians generally seem content to identify themselves with either term.

2. Following recent usage in the liberal–republican debate, I will use the terms *individual* and *self* interchangeably, while recognizing that in other contexts the two terms may have different meanings (see, e.g., Rorty 1976, 315).

3. By "direct" corporate electoral expenditures and contributions, I mean expenditures and contributions funded by the corporate treasury, as distinct from those undertaken by the corporate political action committee, itself funded by contributions from employees and shareholders. While federal law prohibits direct corporate contributions and expenditures, it permits limited political action committee contributions and unlimited expenditures. The states, by contrast, are still largely divided in their approach to the legitimacy of direct corporate electoral activity. For example, 18 states currently prohibit direct corporate contributions in candidate elections, 14 place no limits on such contributions, and 18 fall somewhere in between (see Boggs and Boyce 1990, no. 3).

4. There is one distinction between candidate elections and referendum campaigns that allows for some coherence in the Court record. The Court has held that the state has a compelling interest in preventing political corruption and has observed that while candidates can return a favor, causes cannot. A concern for corruption, however, is both a relatively confined and a relatively closed issue in the constitutional–legal debate over corporate electoral activity. It is confined because (as just noted) it has been held to apply only to some types of corporate electoral activity (i.e., corporate involvement in candidate elections; see *First National* v. *Bellotti* 1978, 790) and not others. It is closed because both opponents *and* defenders of corporate electoral activity agree that it is a concern within that confined area and, instead, fight their battles on the other issues discussed in the text (see *Austin* v. *Michigan* 1990, 702). Being both a confined and a closed issue, the concern over corruption is not part of the ongoing mainstream of constitutional debate over corporate electoral activity. The remaining major issues are both universal (in the sense that they raise the same considerations about all types of corporate electoral activity) and open (i.e., no relative agreement has been reached on them within constitutional discourse). Consequently, unlike the issue of corruption, these issues divide participants cleanly into defenders and opponents of corporate electoral activity per se (see, the discussion in Sorauf 1986, 106, 116).

5. Until *Bellotti*, the broad question of "whether business corporations have a constitutionally protected liberty to engage in political activities" of any sort had "never been squarely addressed by [the] Court" (*First National* v. *Bellotti* 1978, 822). Until *Austin*, the Court had never passed on laws prohibiting direct corporate involvement in candidate elections, at either the federal or state level.

6. I do not mean to suggest that apart from the four disagreements I will be examining, there has been nothing else said on the subject of corporate electoral activity. I would suggest, however, that these four disagreements are the only ones that recur in constitutional discourse (often in more detailed and complex form than in my summaries) and that raise the most fundamental constitutional questions—questions concerning the nature of neutrality, coercion, unfettered political expression, and unclouded political judgment.

7. In *Austin*, the Court at points conflated this third argument against corporate electoral activity—that electorally active corporations co-opt and hence misrepresent the political support of shareholders—with the first argument noted in the text, namely, that corporate electoral activity distorts the political marketplace. To regulate corporate electoral activity, Justice Marshall opined (*Austin* v. *Michigan* 1990, 660), is to regulate the "corrosive and distorting effects of immense aggregations of wealth . . . that have little or no correlation to the public's support for the corporation's political ideas" (see also Ashdown 1991, 783; *Austin* v. *Michigan Chamber of Commerce*" 1991, 1071–72, 1077).

8. Epstein (1984, 476, 478) makes this argument with respect to corporate PAC electoral activity. With respect to the capacity of the corporate PAC to coerce employees, Handler and Mulkern report that "corporate executives who are solicited [by PACs] do not seem to behave like organization men, and they show resistance to the pressures to give that some insist must be overwhelming" (1982, 97).

9. In discussing the dichotomy between a radically transcendent and a radically situated subject, Kateb writes "We can continue to call our matter the debate between liberalism and communitarianism, but it could more precisely be called the debate between individualism and anti-individualism" (1989, 183–84).

10. Some republicans might argue that while they may well have advanced a self more or less encumbered and embedded, they had in mind its being encumbered by, and embedded in, a more communal and less institutional set of social arrangements than those assumed by opponents of corporate electoral activity. But as Nancy L. Rosenblum points out, the examples of community cited by republicans are "radically dissimilar. . . . [They] range from traditional positive structures like ethnic and kinship groups to economic, ideological, and cultural associations [including] corporate bodies" (1987, 156–57; see also the discussion in Sullivan 1988, 1714–21). The version of the encumbered self underlying constitutional argumentation against corporate electoral activity is, then (naturally enough, given the subject of the debate), one more situated in institutional ties (to corporations and government) than rooted in family or ethnic groups. Other republicans might argue that while they may have advanced a conception of the self encumbered by various institutional attachments, they have shied away from a version of the individual as determined or constituted by particular economic interests. Indeed, some republicans might insist that such an individual is a liberal-pluralistic, not a republican, conception. Here, again, it is important to bear in mind the wide-umbrella nature of the term *republican* (Sunstein 1988, 1547). To begin with, some republicans do, in fact, speak of a self encumbered by "the interests it has", or of an individual "engulfed in social determinants." More broadly, certain strands of republicanism clearly presuppose citizens who are not able to surmount their own private interests, just as prominent strands of liberalism (Kant, Rawls) posit individuals who *are* able to transcend their own private interests (Burtt 1990, 25–26, 29–32; Sunstein 1988, 1549, n. 146; ibid., 1558; ibid., 1567, n. 160). Of course, other strands of liberalism and republicanism do just the reverse (Burtt 1990, 27–29, 32–35; Sunstein 1988, 1548–50, 1573). In short, Sunstein, Burtt, and others paint rich pictures of the liberal and republican intellectual traditions. They show us that we can associate with various strands of liberalism both an individual who transcends, and an individual who is governed by, personal interests. Equally, they show us that we can associate with various strands of republicanism both an individual who pursues the common good and one encumbered by personal economic interests. All the more reason, then, why the self underlying opposition to corporate electoral activity is best described as starkly "encumbered" or "embedded," not "republican" or "communitarian." The terms *encumbered self* and *autonomous self* both carry a large number of specific connotations, and only one particular version of each animates the opposing sides in constitutional discourse over corporate electoral activity.

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WAR AND THE FATE OF REGIMES: A COMPARATIVE ANALYSIS

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Governments are likely to be held accountable for the success or failure of their foreign policies. Consequently, we claim that international wars can, under specified conditions, have domestically instigated consequences for violent regime change in the political systems of the participants. Drawing upon all international war participation between 1816 and 1975, we seek to answer the question, Do wars lead to violent changes of regime and if so, under what conditions? Three hypotheses set out the expected associations of a nation's initiator or target role in a war, the war outcome, and the costs of the war with domestically instigated violent changes of regime. Direct relationships are found for all three and hold even against possible threats to their validity and robustness. The results suggest that domestic politics play a larger role in national security policy than is generally believed by realist or neorealist theorists.

Today those of us living in democratic states take for granted the right to hold governments accountable that fail to advance and protect the well-being of their citizens. The acknowledgment of such a right is not universal, nor is it of such long standing. The right to punish a regime by removing it from power—with force if necessary—was hotly debated among social contract theorists throughout the seventeenth and eighteenth centuries. It remains a contentious issue in China, much of Africa and the Middle East, and elsewhere around the globe even today. Beyond the philosophical question of the "natural right" to punish regimes for failed policies is the empirical issue of accountability. Whatever position one may take on the philosophical issue, it is important for leaders and researchers alike to know whether regimes are held responsible for failed national security policies.

If, as a matter of fact, regimes (encompassing leaders and their policies) are held responsible for jeopardizing the well-being of citizens, then we should expect such accountability to dampen any enthusiasm for risky political adventures. In particular, if the fate of a regime turns on how its national security policies affect its citizens, then those policies must be shaped and constrained by expectations about the domestic political consequences if they fail. According to this perspective, every foreign policy maker must look over his or her shoulder and contemplate the prospective domestic political risks associated with each foreign policy action (Lamborn 1991).

We propose to investigate whether the fate of regimes has been dependent on domestic responses to war experiences over the past century-and-a-half. War-related actions are a good starting point, because war is the costliest policy in which a regime can engage. As such, it is the governmental action most likely to jeopardize citizen welfare. Because war has the potential to be extremely costly, experiences in war are particularly salient for an investigation of the political accountability of regimes. Our principal concern, then, is with the relationship between a government's war experience and the likelihood of a forceful removal of the regime by its domestic opponents.[1]

If a relationship between domestic responses to war performance and regime stability is validated empirically, then the realist or neorealist emphasis on external, structural features of the international system probably understates the importance of domestic affairs in shaping national security policy. Regime leaders, according to a perspective that takes accountability seriously, must consider how their actions will play at home even if an adventurous foreign policy ultimately is expected to prove successful militarily. The quest for enhanced international power and security would be expected also to accommodate the desire to satisfy domestic constituencies that can threaten a regime's survival at home (Bueno de Mesquita and Lalman 1992).

Does international war involvement increase the probability that a nation's political regime will be removed from power through violent means? Historians, sociologists, and political scientists have speculated that it does; but broad, systematic data on the relationship are lacking. No research directly addresses the possible linkage of war to revolutionary domestic upheaval in an explicit manner using a rigorous research design. We attempt to correct this lacuna and in doing so to ascertain how war experiences influence the stability of regimes.

The lack of research on this important possible consequence of war is puzzling. Social scientists seem generally agreed that wars are likely to produce an increase in the probability of a violent regime change, but they differ with regard to the factors believed to explain such changes. Some argue that the resource extraction necessary to fight a war will produce instability (Tilly 1975, 1990). Others argue that rebel-

lion will follow war participation when a regime is unable to extract the resources necessary to meet the demands placed upon it by the population. This leads to a consequent decrease in legitimacy and an increase in dissatisfaction with the product of the state (DeNardo 1985; Lamborn 1991; Organski and Kugler 1980). Still others argue that wars bring about a redistribution of power among interest groups and coalitions "and thus prime society for conflict over values and resources, including revolutionary conflict" (Starr 1991, 26; see also Goldstone 1980).

Despite these assertions, prior empirical research provides little evidence relating war experiences to domestic political upheaval and revolution. To be sure, there is research on the effects of international conflict on all levels of internal political conflict; but the results of this research are either mixed (Wilkenfeld and Zinnes 1973), based upon only casual inspection of the data (Sorokin 1957), or limited to a single society (Rasler and Thompson 1989; Stohl 1980).

In reviewing a number of these studies, Stohl begins with the observation that although it is widely thought that war leads to domestic political conflict, "only a few of the studies actually confront this problem with systematic data" (1980, 316). Stohl's further evaluation of these few studies points out their mixed findings and limitations of research design and interpretation. He concludes that a considerable amount of work needs to be done on the possible effects of international conflict on political instability.[2]

Since Stohl's review there has been relatively little new empirical research on war and violent regime change. Two studies published in the same year as Stohl's essay but probably not available to him offer examinations of war participation and regime change; but their analyses are brief and limited in range and do not distinguish among various forms of regime change (Siverson 1980; Stein and Russett 1980). Rasler and Thompson (1989) trace out the effects of war involvement on domestic political conflict; but their analysis, while theoretically rich and methodologically sophisticated, is limited to the United States in the twentieth century. As part of his analysis of the relationship between national economic decline and dispute escalation, Russett examines the effects of international conflict participation on rebellion, which he measures by a combination of the numbers of armed attacks on political authorities and the resulting deaths; but no direct relationship is found (1990, 136).

Still, the idea that there is a link between international war and internal political change persists—and perhaps for good reason. Tilly, for instance, asserts, "All of Europe's great revolutions, and many of its lesser ones, began with the strains imposed by war" (1990, 186). Tilly recounts the effects of wars in producing the English Revolution of 1688 and the French Revolution and concludes with the following description of the Russian Revolution: "Russian losses in World War I discredited tsarist rule, encouraged military defections, and made the state's vulnerability patent" (ibid.). Tilly's assertion is accurate; but it addresses only major revolutions, the largest, most visible part of violent regime change. Our interest is in probing the overall linkage between war participation and violent domestic political changes of all kinds—not just the relatively few highly visible cases. A few cases, no matter how widely recognized as historically important, are not the basis for a sustainable generalization (Riker 1957).

There is, consequently, much we do not know about the relationship that might exist between war participation and rebellion or revolution. For example, at what rate do the states that enter wars experience violent changes of regime? Does it make a difference in that rate if the state does well in its performance against its opponents (i.e., wins, rather than loses)? Does it make a difference whether the state is an initiator of the war or the target of some other state's policies? Do differences in the cost of war have a consequence for the likelihood of violent regime changes?

Answers to questions such as these are critically important if we are to understand not only the aftermath of war but *also the judgments and expectations of decision makers that promote its inception*. War is not only about gains in national power or security within the context of the international system. We believe that the causes and consequences of wars extend well beyond the systemic, structural impediments that are the focus of realist and neorealist theories of international affairs (Morgenthau 1973; Waltz 1979). War is also a powerful force shaped by, and giving shape to, domestic political affairs and for making *or breaking* the political fortune of national governments. As such, an understanding of the conditions under which war enhances or diminishes the fate of a regime is important both from the perspective of an interest in reducing the danger of violent conflicts and from the perspective of understanding linkages between domestic politics and foreign affairs.

INTERNATIONAL CONFLICT AND VIOLENT REGIME CHANGE

The fundamental question we wish to explore is whether states experience significant violent, internally motivated political changes following participation in international war and, if so, under what war-related conditions. Despite the dearth of broad-based empirical research, there are reasons to believe that a close connection exists between war and the fate of regimes. The ideas supporting this relationship (as mentioned) relate war involvement to regimes' accountability to the citizens they serve. Our consideration of this question takes place in several stages. First, we focus on the outcome of the war and the role of the participants in shaping this outcome. Then we move to an examination of the effects of war costs in influencing violent regime changes. We conclude with an investigation of the relationship be-

tween prior and current state experiences with violent regime change to ascertain whether the effects we observe are spurious or robust.

At the outset, we reason that a failed policy in the domain of international war will render a regime vulnerable to removal. A failed conflict policy can arise in several ways. We view all defeats in war as instances of failed policies. After all, war is extremely costly in lost life, property, and opportunities foregone. Consider that defeat in war almost always alters the loser's freedom of action, reducing the nation's autonomy over its own foreign policy or depriving the vanquished state of sovereignty over some portion of its citizens, territory, or national product (Morrow 1987). Evidence of such losses is likely to be more dramatic and more apparent to the domestic population than is evidence of the failure of economic policies, an area known to influence the survivability of regimes. Moreover, in nations in which an electoral system is not present, or is nonfunctional, war losses are abundantly obvious to members of the elite, who themselves may have both the opportunity and motive for replacing leaders.

Although fighting a war is sometimes warranted by beliefs about the prospects of success or even for purposes of building a reputation for toughness or steadfastness, still mounting *anticipated* costs heighten the impetus a regime faces to settle without fighting (Bueno de Mesquita and Lalman 1992; Wittman 1979). Fighting a losing war reflects upon a government's failure to anticipate adequately the costs being imposed on the nation. Such a failure is, we believe, likely to be punished; and this punishment is expected to increase with the costs of the war. This should be true whether the regime's leaders initiated the fighting or were the target of attack. A losing initiator regime is likely to be held accountable for its aggressiveness. A defeated victim who survives the wrath of the victor still confronts the prospect of punishment by its own citizenry for failing to cut losses and settle up earlier.

Victory in war is not as straightforward as defeat. An initiator who wins has generally gained some benefits for the state that apparently were not obtainable through other means, although the gains often come at a high price. Such an accomplishment is likely to prompt indifference or reward; it is unlikely to provoke punishment of the regime. But a victorious target faces a rather different circumstance. When the target of attack emerges as the winner, there is clear ex post evidence that the government's ex ante policy was a failure. Since an initiator is unlikely to attack unless it expects to do better by fighting than by not fighting, it is likely (though not inevitable) that the leaders of a defeated initiator state misjudged the nation's prospects. Such misjudgments are, to some extent, the responsibility of the loser—hence our hypothesis that defeat heightens the likelihood of punishment. But such misjudgments may also reflect a failure by the target to communicate adequately about its motivation and/or capabilities. Such failure to communicate must at least partially offset any domestic political benefits to be had as a result of ultimate success. Consequently, we hypothesize that victorious initiator governments gain the most benefits—victorious targets of aggression not doing as well as victorious initiators in terms of regime survival. Still, winning is better than losing; and this is expected to be true whether one is an initiator or the target.

We propose, then, the following hypotheses which, if supported, provide an empirical foundation for the claim that the political welfare of a regime is intimately tied to the foreign policy welfare of the state. Data supportive of these hypotheses provides evidence of an important linkage between domestic affairs and foreign policy choices, providing an important contrast to the classical, realist perspective:

HYPOTHESIS 1. *The chances of a domestically instigated, violent regime change increase with defeat in war.*

HYPOTHESIS 2. *Violent regime change is least likely for winning initiators; the likelihood of violent regime overthrow increases for winning targets, losing targets, and losing initiators in that order.*

HYPOTHESIS 3. *The chances of a violent regime change increase with the costs of war, irrespective of the nation's war outcome or initial conditions.*

We examine whether during a war or after its conclusion the leader *and* his or her *entire regime* is removed from office through violent means. By violent means we intend to cover revolutions, coups d'état, and the like but not (as noted) removal by the opponent state. Removal of an entire regime is a very drastic act that is not always easily accomplished and that when carried out successfully, often requires appreciable political mobilization. If regime change is a regular feature of major failures in national security policy, then the *expectation* of such a consequence is probably an important, domestically induced, pacifying element in international affairs, an element that helps account for the frequency with which states tolerate one another, rather than wage war (Keohane 1984).

For removal from office through violent means to be reasonably related to war experience, it should occur within a period from after the war's onset to a relatively short time after the end of the war. We define two alternative time frames as the relevant periods for consideration: (1) the period from the beginning of the war until three years after its end, and (2) the period from one year before the end of the war until one year after its end.

For our purposes, the first period, extending up to three years beyond the end of a war is taken as the germane, *extended* time frame for a war-related violent regime change to occur. We choose a three-year interval because it offers the opportunity for war costs and outcomes to be recognized by the population and for an opposition to form and act. Taking it further than three years, however, could possibly let nonwar phenomena enter the picture more strongly. If a leader's regime is removed during the war or within

three years of its termination, this seems to us as a reasonable case of war-related removal.

Two questions arise regarding this extended time frame. First, why did we use the entire period of the war? It is reasonable to suspect that policy failure will not necessarily be evident during the entire period of the war and that therefore we should look only at the period around the end of the war. For example, policy failure would not have been evident to Germans in 1940 but had become manifest by late July 1944. The problem with this is that we know of the end of the war post hoc. Those who forcefully overthrow a regime during wartime to change governmental policy have no way of knowing when the war will end. Second, why do we choose to extend our data into the third year after the end of a war? Do we not run the risk as the years pass beyond the end of the war of including violent regime changes having less and less to do with war experience and more to do with other possible variables? The answer to this is, of course, *yes*. However, any decision on a time frame will necessarily be arbitrary. We believe that an ample enough amount of time needs to pass so that the costs of a war can become apparent. After the end of the war but before fours years have passed, these costs are most likely to affect the victors. Thus, the time period will likely make the test of some of the hypotheses more stringent. The extended time frame has the virtue that it is unlikely to exclude relevant violent regime changes, but it has the limitation that it may include changes that were not related to the proximate war experience.

As a check on this extended time period we also evaluate our hypotheses in terms of a more *constrained*, second time interval encompassing violent regime changes that took place in the period from one year before the end of the war to one year after its end. In this way we can better evaluate the robustness of the evidence we uncover and assess whether it is consistent or inconsistent with our hypotheses. With this narrower time frame we mitigate the risk of including "irrelevant" regime changes; but, of course, we increase the chances of excluding violent regime changes that should have been included. Thus, each of our time frames offsets a limitation of the other, providing a more general basis for evaluating the reliability and robustness of the evidence we report.

THE DATA

Our data are relatively straightforward. The war data are taken from the well-known collection of the Correlates of War project reported in Small and Singer's (1982) *Resort to Arms*.[3] The data set not only reports on national involvement in all international wars between 1816 and 1975 with at least one thousand battle-related fatalities but also identifies the nations that initiated the wars and the eventual winners and losers.

We eliminate two categories of cases. First, we drop the cases in which the regime change is forced upon a state by its external enemies during the war or at its conclusion. This is consistent with our interest in domestically instigated violent regime change. For example, we do not include the new German governments that emerged in the years following the end of World War II or the removal of the Dutch government by the Germans during the war. Second, we exclude the cases in which there was no clear-cut winner (e.g., the Korean War). After removing these, our data set consists of 177 cases.

The data on violent changes of regime were derived from two basic sources. First, Banks's *Cross-Polity Time-Series Data* lists, by year, national attributes with respect to type of regime and number of successful forceful changes of government between 1815 and 1965 (1971, 3–53). These data were checked against the historical chronology given in Langer's (1952) *Encyclopedia of World History* and were also compared to information contained in Spuler's *Rulers and Governments of the World* (1977). Post-1965 data were gathered from *Facts on File* and Bienen and van de Walle's (1991) collection of data on political leaders.[4] The central question we asked was, Did there occur during a war or within the first three years after the end of a nation's involvement in an international war a forceful, irregular, domestically instigated change of its governing regime that resulted in the replacement of one elite by another? The identities of the cases of violent regime change we record are given in the Appendix.

In order to test Hypothesis 3, we need data on the costs of war. Measuring the costs of war directly is not a simple matter. The price of a war includes not only the immediate financial costs but needs to reflect opportunity costs as well. Although Organski and Kugler (1980) have been able to calculate some important dimensions of war costs for a few nations, we know of no data set that provides us a readily available measure for the number of nations with which we are dealing. However, an alternative measure is accessible in the war lethality data contained in the Small and Singer compendium (1982, 82–95). Small and Singer list for each nation's war participation the number of months it participated in the war, its number of battle deaths, its total population, its battle deaths per nation-month and its battle deaths per ten thousand population. This last measure is particularly attractive, since it is consistent across time and is insensitive to the size of a nation's population. Using battle deaths per ten thousand population permits us to make comparisons across nations and ask whether on average the winning targets who experienced violent regime changes suffered more deaths relative to their population than did the losing targets. Because these data are highly skewed at 11.98, we used a logarithmic transformation to reduce skewness considerably, to .95. In addition, it makes substantive sense to use the transformation because increasing battle deaths probably have a decreasing marginal impact that would otherwise be exaggerated (Jackman 1991).

DATA ANALYSIS

At the outset it is worthwhile to note that of the 177 war-participating nations in our data set, 32 (18.8%) experienced a violent change in their regime during the war or within three years of the war's end. It is difficult, however, to know how to evaluate this figure without some base against which to compare it. Is this 18.8% rate higher or lower than that experienced in nations that did not participate in war?

In order to estimate the effect of war on the rate of violent regime change, we drew a random sample of nation-years between 1815 and 1975, excluded any cases already in our war involvement data set and then determined whether the remaining nations experienced a violent regime change in the year drawn or in any of the three subsequent years.[5] We performed this procedure three times. The percentage rates of violent change in these samples were, from lowest to highest, 8.91%, 9.92%, and 10.21%.[6] The rate shown in the data of 18.8% indicates that war participation itself approximately doubles the chances of the regime's being violently overthrown by domestic opponents as a consequence of war participation.[7]

Our hypotheses, to which we now turn, predict significant variation across this base rate. With respect to hypothesis 1, what is the effect of winning or losing a war on the fate of the regime? A cross-tabulation of the data shows that there is a relatively strong relationship between losing a war and drastic changes in political leadership, with 29.5% (23/78) of the losers, and only 9.1% (9/99) of the winners, experiencing an violent regime change ($x^2 = 12.25$, $p < .01$). This is consistent with our first hypothesis and with the notion that failed national security policies provoke domestic political retribution.

Hypotheses 2 and 3 may be tested much more rigorously, since their predictions lead us to anticipate some relatively sharp differences in the data. Hypothesis 2 indicates that victorious initiators experience the lowest risk of violent regime change (i.e., in the probit analyses the relevant coefficient, b_1, is expected to be negative). The signs of the coefficients associated with the remaining variables from Hypothesis 2 (i.e., b_2, b_3, and b_4) can be positive or negative. In either case, b_2 and b_3 are expected to have magnitudes that fall between b_1 and b_4. This indicates that victorious initiators are less likely to suffer from violent changes in regime than are any other type of nation with a recent war experience, while victorious targets are less likely to suffer violent regime transfers than are defeated targets; and both are less likely to have such an experience than defeated initiators. Hypothesis 3 states that the likelihood of a violent change in regime increases as the costs of war increase, thereby leading to the prediction that b_5 will be positive.

In order to ascertain the overall relationship of these variables to violent regime change and to each other, we construct two probit models containing these ordered inequalities. The first model contains the effects postulated in Hypothesis 2 on the likelihood of a violent change in regime, and the second adds the costs of war to these variables. The expectation from the probit analysis is that violent regime change equals

$$-b_1\text{init. wins} \pm b_2\text{targ. wins} \pm b_3\text{targ. loses} + b_4\text{init. loses.}$$

Adding costs, it equals

$$-b_1\text{init. wins} \pm b_2\text{targ. wins} \pm b_3\text{targ. loses} \pm b_4\text{init. loses} + b_5\log(\text{battle deaths/population}),$$

where $b_1 < b_2 < b_3 < b_4$.

Table 1 displays the results of these probit models. The first column of the table shows the results without the inclusion of war costs. Here, everything is consistent with our expectations as to whether a regime endures more than three years beyond the end of a war or is violently removed in response to the nation's performance in war. The coefficients are all as predicted, with $b_1 < b_2 < b_3 < b_4$ in each probit analysis.[8]

How much of a difference do these factors make in the violent removal of a regime? Table 2 reports the cumulative normal distribution of the coefficients shown in Table 1, column 1; these are the translation of the probit coefficients into statement of probability of regime overthrow, given the contingent condition identified.[9] Thus, for losing initiators, the increment in the probability of violent regime change is .44. The other relevant incremental probabilities show losing targets half (.22) as likely to be violently removed as losing initiators and winning targets half again (.11). For winning initiators the probability is exceedingly small (.008).

As shown in Table 1, column 2 when the costs of war (in terms of the log of the battle deaths per ten thousand population) are added to the probit, the coefficients maintain the same order shown in column 1 and are fairly close to their original values. (However, note that the coefficients for the winners move closer together.)[10] It is tempting to look at interactions between battle deaths and the other variables, but there is a more revealing way of getting at the question.

Drawing upon the same method used to derive the results in Table 1, Figure 1 is a graph of the effect of levels of battle deaths on the probability of violent regime change given the four contingent conditions of interest. For the winners, as battle deaths increase, the chances of being violently overthrown nearly double. For losing targets, the chances of removal increase from slightly over .40 to just under .60; for losing initiators, the probability moves from just over .60 to slightly under .80. When compared to the effects of the contingent conditions themselves, it is evident that the effects of battle deaths are far from insignificant and increase the probability of removal by substantial amounts. For example (and most sig-

TABLE 1

Violent Changes of Regime: The Effects of Winning and Losing, Initiation, and Battle Deaths for the Extended and Constrained Models

INDEPENDENT VARIABLE	EXTENDED MODELS (N = 177) (1)	(2)	CONSTRAINED MODEL (N = 171) (3)
Initiator wins	−1.26**	−1.16**	−1.27**
	(.351)	(.364)	(.393)
Target wins	−1.09**	−1.13**	−1.15**
	(.345)	(.349)	(.395)
Target loses	−.64*	−.57*	−.43
	(.311)	(.320)	(.333)
Log(battle deaths/population)	—	.14*	.19**
		(.072)	(.078)
Constant[a]	−.13	−.51*	−.73*
	(.242)	(.306)	(.328)
x^2 ($p < .001$)	17.03	21.26	23.66

[a]The constant is the baseline established by the cases of defeat for the initiator.
*$p < .05$, one-tailed.
**$p < .01$, one-tailed.

nificantly), battle deaths increase the probability of winning initiators being deposed violently from .008 to just over .20. However, it would be a serious mistake to focus only on the differences between this figure and the data in Table 2, because there is another aspect of the data in Figure 1 that merits discussion. Note that even when battle deaths are included, the contingent conditions remain of overwhelming importance. Thus, although the regimes of losing targets experience an increase in the probability of violent removal from about .40 to just under .60, the latter figure is *still below the starting point of losing initiators*. The same pattern holds in comparing the losing targets to the winning initiators and winning targets. In short, increasing battle deaths raise the probability of violent regime change; but the conditions specified in Hypothesis 2 are even more important.

The results of this analysis provide strong support for the hypothesis we originally offered about the effects of war outcomes on the political fortunes of the regimes that are the initiators or targets of international war. Clearly, regimes that initiate wars and do not prevail are at the highest risk of being replaced, while those who initiate and win are at relatively little risk. Our results with respect to the targets of international war are also as expected. All of this is true whether the costs of the war are included or not.

TABLE 2

Cumulative Normal Probabilities of Violent Regime Change

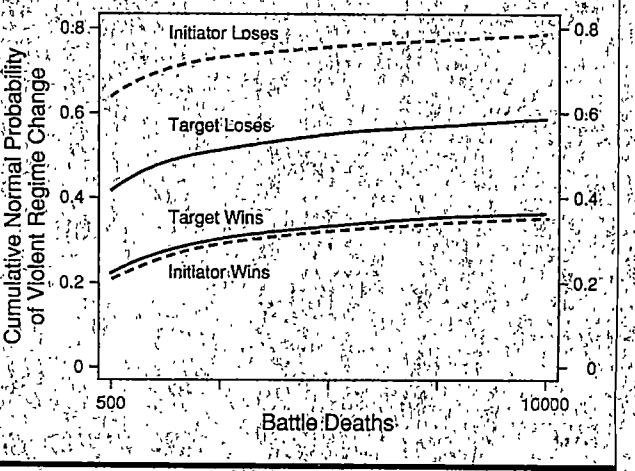
COEFFICIENT	CUMULATIVE NORMAL PROBABILITY
Initiator wins	.008
Target wins	.11
Target loses	.22
Initiator loses	.44

THREATS TO VALIDITY

Although our hypotheses are well supported, we pause to explore several threats to the robustness and validity of the results. Recall that the period over which we collected data on violent regime change extended from the onset of the war to three years beyond its end. As noted, it is reasonable to be

FIGURE 1

War Performance and the Probability of Regime Change



curious about the extent to which our findings would continue to obtain if this period were constrained to a much shorter one. For this purpose we remove from the data set all cases in which the violent change of regime took place more than a year before the end of the war and more than a year after the end of the war. This more limited time frame removes six cases from the data. The probit results from this more constrained data set are reported in Table 1, column 3. In terms of their sign, relative order, and statistical significance, the results shown here are extremely close to those obtained earlier, the main difference being that the effect on violent regime change of being a losing target is just beyond conventional standards of statistical significance ($p = .095$), although the odds are still better than 10 to 1 that the effect is not due to chance.

Another potential threat to the validity of our analysis derives from the fact that regimes and governments are always changing at some rate and that some nations are more prone to change than others. It is possible that to one degree or another, the changes we observe simply reflect a continuation of past governmental instability. After all, if a nation has experienced political instability in some period before its involvement in war, it is not unreasonable to think that irrespective of war involvement, some of that instability may be sufficiently embedded in the political system to account for the violent regime changes we observe better than does war involvement. To assess this possibility that the results are spurious, we constructed an index of prewar political stability for each of the 177 cases in our data. This was done by enumerating the number of successful coups d'état each case experienced in the 10 years prior to its war participation.[11] Interestingly, the results of this probit reveal that a relationship does exist between prewar political instability and violent regime change among the states participating in war (coefficient = .22, $p = .027$).

Given this, we need to assess the extent to which this estimated relationship is independent of the state's initial position in the war, its outcome, and its level of cost. It is not, after all, unreasonable to suspect that the regimes of unstable polities may be more sensitive to, for example, the costs of a war than regimes existing in generally stable environments. If so, then including our measure of prewar instability into our previous model could cause some of the already reported effects to be altered.

To investigate this prospective threat to the stability of the results, we include the prewar instability variable in the original full model of Table 1. Table 3 shows that the outcome and cost variables continue to have relatively the same estimated effects found in the earlier analyses even when the index of prewar instability is included. In other words, the political instability variable does *not* have a discernible impact on the weight of war outbreak, war outcome, or costs we estimated earlier. Our results continue to appear robust even when taking into account the impact of prior political instability.[12]

TABLE 3

The Effects of War Outcome, War Costs, and Prewar Instability on Violent Changes of Regime

INDEPENDENT VARIABLE	COEFFICIENT	STANDARD ERROR
Initiator wins	−1.26**	.380
Target wins	−1.16**	.353
Target loses	−.71*	.330
Log(battle deaths/population)	.15*	.074
Prewar coups	.27*	.125
Constant[a]	−.64*	.311

Note: N = 177, $\chi^2 = 25.91$, f = .000.
[a]The constant is the baseline established by the cases of defeat for the initiator.
*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.

CONCLUSION

Our results demonstrate quite clearly that the outcome of war has a dramatic effect on the fate of the regimes involved. Our hypotheses set out the expected associations; and all were supported by the evidence with regard to irregular, forceful changes in regime. Although this relationship has not been previously established, some may not find the results surprising. However, what we have shown is not merely a relationship, but (1) an estimate of the *size* of the effect of war on violent regime change against a baseline and (2) the *specification and testing of quite specific, detailed hypotheses* describing the relationship of war, war costs, and regime stability to violent regime change.

Although broadly based in time and ranging across all types of political systems, the results are fully consistent with the claim that the political welfare of governmental regimes is directly tied to the performance of the nation in war. This conclusion casts interesting light on a controversial part of international relations theory. The evidence presented here can be taken as an evaluation of the claim that the political welfare of foreign policy makers is intimately tied to national performance in conflicts that pose a potential threat to sovereignty and that the preferences of such leaders are constrained so as to give highest priority to maximizing the state's overall welfare (Bueno de Mesquita 1981). Having observed that regimes are ex post punished for failed foreign policies, we propose an ex ante inference. In the context of models of rational action, behavior is strongly influenced by expectations about consequences of actions "off the equilibrium path"; that is, the choice of actions is shaped by beliefs about what would happen if some other action were chosen. Leaders can anticipate that they will be held accountable for failed foreign policy adventures. Consequently, the choice of war-related behavior is likely to be dampened by the fear that the regime will be punished if things go awry. Probably, the wars we do

observe are cases where leaders generally thought the foreign policy action would turn out well from the perspective of their regime. They thought the potential benefits from fighting outweighed the associated risks to the regime. In that sense, the true effects of war on regime change are probably stronger than the observed effects reported here. The true effects include instances of wars that did not happen because of the anticipation of domestic political punishment. We believe, therefore, that the results lend considerable credence to models that treat war/peace decisions as if they are the product of rational choices by a unitary actor whose preferences are partially shaped by the existence of a threat to the state's sovereignty or autonomy and who takes into account the distribution of preferences and interests of the constituent elements that make up the domestic affairs of the state. The results show that governmental leaders must attend to the domestic political ramifications of their decisions in choosing foreign policy actions. The standard realist account, which largely ignores domestic political considerations, is difficult to sustain in light of the evidence adduced here.

APPENDIX

Our cases of violent regime change and year of war onset are derived from Banks 1971, *Facts on File* for 1966–75, and Small and Singer 1982.

Nation	Year of War Onset
Spain	1823
Mexico	1846
France	1849
Argentina	1851
Columbia	1863
Paraguay	1864
Spain	1865
Peru	1865
France	1870
Peru	1879
Guatemala	1885
El Salvador	1885
Honduras	1906
Honduras	1907
Nicaragua	1907
Turkey	1911
Turkey	1912
Germany	1914
Greece	1914
Bulgaria	1914
Turkey	1914
Austria–Hungary	1914
Russia	1914
Hungary	1919
Greece	1919
Turkey	1919
Paraguay	1932
Italy	1939
Syria	1947
United Arab Republic	1947
Pakistan	1965
Cyprus	1974

Notes

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1. We emphasize that the cases to be examined in the analysis are those in which the regime is removed by its domestic opponents. Thus, regimes that fall by virtue of foreign imposition are not included in the data.

2. Despite the time that has passed since Stohl's review was completed, it probably remains the best overall treatment of the relationship between internal and external conflict.

3. We include the wars listed in the Correlates of War data set (Small and Singer 1982). The wars do *not* include events that are interventions in which a government is removed forcefully by another without sufficient resistance being present to reach the "war" threshold of one thousand battle deaths. For that reason such events as the disappearance of the Baltic Republics into the Soviet Union in 1939 and the German invasion of Denmark in 1940 are not included in the analysis.

4. Also available to us were Gurr's Polity II data. These, however, were not fully suited to our needs. Although these data are quite extensive with respect to the measurement of certain kinds of political change, they focus not on the regime (i.e., the members of the political elite who make state decisions) but on the much broader, underlying patterns of political authority and constitutional structure. Our interest is in the fate of the group of political leaders who take a nation into war and not necessarily on the overall pattern of state authority, although several such changes are recorded in the data. Thus, for example, although there have been many irregular, usually forceful, changes of regime in Mexico (Bienen and van de Walle 1991, report 28), the Polity II data record only four changes of polity. The Polity II data, it may be noted, also contain Banks's (1971) data on violent political changes on which we partially rely.

5. We chose this time period because the average length of a war in our data set was just over one year. Combining that year with the three years we allowed after the war ended yields a time frame comparable to that used in our analyses of forceful regime changes among states involved in war.

6. Excluding the nations in our data set for the years they fought and the three years after the war produces a data set of slightly more than three thousand nation-years of data. We drew 5% samples from these. While our data go to 1975, 10 years beyond the end of the data from which the samples were drawn, it is not clear that there were any marked differences in these omitted years.

7. Almost two-thirds of the violent changes of regime in the three samples are to be found in the nations of Latin America. This high rate of forceful regime change in Latin America is consistent with the findings of Bienen and van de Walle (1991, 86). It might be argued that the samples should be stratified by region to reflect the rate of change more accurately. Such a procedure would undoubtedly show that the effect of war participation on relevant regime changes in the non–Latin American nations is much *higher* than we show here. However, given the state of the data set from which the samples were drawn (i.e., no geographic code is present), it is difficult to implement this method. Moreover, it is unnecessary to our purpose, which is only to show that war participation makes a meaningful difference in the rate of violent regime change. We will approach another aspect of this question further on.

8. Because of the nature of the independent variables, it was necessary to leave one of them out of the estimation of the model. The effect of the omitted variable is then contained in the constant. However, to make sure that this procedure was not favoring our hypotheses, we computed the zero-order product moment correlation between violent change of regime and all the variables in the probit reported in Table 1, column 2. The results are perfectly consistent with the esti-

mated model and our hypotheses. The Pearson product moment correlations between violent regime change and our variables are *initiator wins* −.22, *target wins* −.06, *target loses* .01, *initiator loses* .33, and *log(battle deaths/population)* .27.

9. For a very accessible explanation of this technique, see Wolfinger and Rosenstone 1980, app. C.

10. We provide the cross-tabulation of predicted and actual outcomes based on the probit analysis shown in column 2. The model of regime turnover that we propose is not a full equilibrium model. As such, it does not take into account all of the factors that contribute to forceful, domestically instigated regime change. Consequently, the model is not expected to account for all—or even most—of such regime changes but, rather, to isolate conditions that systematically *increase the likelihood* of such change. The model's significance is best evaluated, then, in terms of the extent to which the probit coefficients are consistent with the expectations expressed in our hypotheses, rather than in how many cases were fully determined by the factors we address in our model. The cross-tabulation follows (pseudo R^2 = .11). The results are consistently robust in terms of the ordinality of the coefficients; the differences across coefficients approach but do not achieve significance.

	Predicted Outcome	
Observed Outcome	No change of regime	Change of regime
No change of regime	143	2
Change of regime	26	6

11. Data on successful coups d'état were taken from Banks 1971, segment 1, field E and (for years after 1966) from *Facts on File* for 1966–75. We would have preferred to use data on attempted coups d'état, as well as successful ones, but we were unable to find any broad data reporting such attempts.

12. Our analysis has treated all regimes as equivalent; but we know that they differ, among other things, with respect to the ease with which leaders may be replaced. Elites in some states may be forced to use violence to remove the regime because other alternatives are not readily available to them, while other states (i.e., democracies), may have well established, even institutionalized, methods for removing the political leadership. Further, the number of winning democracies in our data may distort some of our findings. To investigate this possibility, we estimated the probit contained in Table 1, column 3 after removing democracies. Although there are some modest changes in the coefficients (none larger than .15), the order of the coefficient values remains the same.

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REVOLUTIONARY COLLECTIVE ACTION AND THE AGENT–STRUCTURE PROBLEM

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Unraveling the nexus between agents and structures is fundamental to an understanding of political and social change. The two most prominent methodological approaches to explain revolutionary collective action involve either individual reductionism or structural reductionism. Both approaches result in theoretical inconsistencies and/or explanatory anomalies. An alternative proposed here utilizes the concept of framing *developed in behavioral decision theory primarily by Quatrone and Tversky. It directly addresses the agent–structure problem by developing the proposition that individuals evoke alternative decision rules in different structural contexts. The result is greater theoretical coherence and resolution of anomalous cases. Additionally, this model begins to define a new role for ideology in explanations of revolutionary collective action.*

A little over a decade ago the study of revolution took two, distinctly separate, theoretic turns. In *States and Social Revolutions* Theda Skocpol (1979) argued for a structural and nonvoluntarist study of revolutions. At the same time, in *The Rational Peasant,* Samuel Popkin (1979) argued for a study of peasant revolutionary action based upon the axioms of rational choice. Both studies contribute to our understanding of revolution but are in an important sense methodologically—and thus theoretically—incompatible. Skocpol's exclusive focus upon social structures and the Popkin's equally exclusive emphasis upon individual action means that each implicitly denies the legitimacy of the other. Since their initial writing, these works and their attendant methodological approaches have remained distinct and apart.

This methodological bifurcation reflects a larger theoretical dilemma in the social sciences that Wendt (1987) refers to as the *agent–structure* problem. Political scientists, in the attempt to understand empirical reality, are everywhere confronted with two "truisms" about the social world. First, individuals are often agents whose intentional, self-conscious, actions both reproduce and transform social reality. Second, society consists largely of interconnecting social relationships (structures) that condition the interaction between agents, as well as the outcomes of agent action. Combined, these two dynamics suggest that a major theoretical task for political science is to unravel how agents and structures interact, thereby approaching more comprehensive explanations of social change.

The most common method employed to cope with the agent–structure problem has been to impart to either agents or structures a fundamentalist property. Such reductionism is evident in Skocpol's analysis, for she views individual revolutionary action to be a function of social-structural dynamics. Similarly, Popkin views social change (revolution) as a function of intentional maximizing behavior on the part of individuals. In neither approach is the agent–structure problem resolved: it is metatheoretically denied.[1] A second approach not often employed in political science but growing in popularity is to impart to both agents and structures equal ontological status and to view agents and structures as mutually constituted (Giddens 1984). However, without any meaningful analytical separation between agents and structures, it becomes impossible to unravel the causal nexus between the two and thus locate the cause or causes of social change (Taylor 1989).

In this vein, James Coleman (1986) argues that any theory of society that attempts to tackle the agent–structure dilemma must be grounded in an explicit model of individual purposive action. Harkening back to the writings of Hobbes, Smith, Locke, Rousseau, and Mill, Coleman notes that a model of purposive action was fundamental to classic political theorizing because

> it allowed connecting intentions of persons with macrosocial consequences. Thus the functioning of society as well as the engine of social change could be grounded in the purposive actions of individuals, taken in particular institutional and structural settings that shaped the incentives and thus action. Social theory with this kind of grounding made possible a conception of how social systems might be shaped by human will. (p. 1310)

The analysis that follows is so grounded. But in order to understand revolutionary movements, the nexus between individual action and social structure must be more completely developed than even Coleman's statement implies. I shall examine Skocpol's *States and Social Revolutions* as an example of methodological structuralism, then Popkin's *Rational Peasant* as an example of rational choice. It will be demonstrated that, upon close scrutiny, theoretical inconsistencies emerge in both approaches because of an inattention to the agent–structure problem.[2]

Against this, I will suggest one way in which these methodological (and as a result, theoretical) problems can be overcome.[3] I will lay the groundwork for a model of peasant revolt that makes explicit the connection between structural context and individual

decision rules by expanding the concept of "framing" developed in empirical behavioral decision theory. My thesis here is that revolutionary movements can only be understood by addressing the agent–structure dynamic and that this, in turn, requires an understanding that individuals invoke different decision rules in alternative structural contexts. These rules are related to agent understandings about current and past structural relationships. It will then follow that the role of both revolutionary organizations and their ideologies is communicative, intervening between structures and agents. The result is a reintegration of ideology into a theory of revolution. Finally, I will discuss theoretical implications and possible research programs.

METHODOLOGICAL STRUCTURALISM

Skocpol's study is in large part a forceful and explicit defense of methodological structuralism. At the outset Skocpol states that the study of revolutions requires that the social scientist "take an impersonal and nonsubjective viewpoint" (1979, 18). Methodological individualism, which makes revolution a consequence of intended action, is discounted because it (1) incorrectly implies that social order rests in consent and (2) is empirically incorrect. Historically, revolutionary actors have taken advantage of structural crises, but intentional action in no sense generated them. Hence, individuals should be viewed as "participants in complex unfolding of multiple [structural] conflicts," rather than purposive actors. (p. 17). Thus an understanding of revolutions requires one to rise above the viewpoints of potential agents to find important regularities across cases (p. 18). For Skocpol such regularities are to be found in the commonalities between social structures.

Nonetheless, there is in Skocpol's description an implicit and theoretically necessary model of intentional peasant action. Indeed, it is because this model is not made explicit at the outset that the nexus between individual action and social structure is never fully developed. As a result, the theory (and its internal logic) fails to provide an effective explanation of peasant rebellion.

Skocpol begins the analysis by making two assertions about the nature of peasant revolt. First, peasants are in a permanent revolutionary condition. The peasantry is perpetually subject to economic, political, and cultural marginality and socioeconomic immobility. By definition, then, peasants "always have grounds for rebellion against landlords, state agents, and merchants who exploit them" (1979, 115). Peasant grievances—for more land, greater autonomy, lowered taxes—are thus inherent to, and defined by, their position as a social class. "What is at issue," says Skocpol, "is not so much the objective potential for revolts on the grounds of justifiable grievances. It is rather the degree to which grievances that are always at least implicitly present can be collectively perceived and acted upon" (ibid).

The potential of the peasantry to act collectively is a function of structural positioning. To revolt, the peasantry must possess a degree of leverage against the landed elite that exists only when there is sufficient peasant solidarity and community autonomy, that is, *tactical freedom*. Such conditions are usually attendant to rentier agrarian systems where local peasants work small plots of land and are free from strict supervision by the landed elite (1979, 116). Here, the tactical freedom afforded to peasants is more amenable to local organizing, since individuals are not pitted against each other competing for seasonal employment as wage laborers. It is important to note that as a necessary component, tactical freedom need not be inherent to the existing structural status quo. It can (as Skocpol's description of the role of the Chinese Communist party in the revolution suggests), in effect, be "created" by a revolutionary organization that expertly secures areas of local control in which solidarity and organization can flourish (p. 148).

Additionally, tactical freedom must be accompanied by a relaxation of effective coercive state sanctions that normally crush peasant revolts (Skocpol 1979, 115). (Actually the source of sanctions is not theoretically important. In Skocpol's structural framework it becomes central only because, since the landed elite is dependent upon the state as the primary sanctioning agent, state impotence translates into a collapse of effective sanctioning.) It is the collapse of coercive restraints that affords peasants the room to utilize tactical freedom in concerted attacks against the landed elite. The implication throughout is that these structural conditions are necessary and sufficient conditions for widespread peasant revolt.[4] By combining the notion of a class in a permanent revolutionary condition with necessary structural conditions, one can state a unified hypothesis concerning peasant revolt, namely, that given sufficient solidarity and autonomy, a precipitous decline in sanctions will spur peasant revolt.[5]

Now, even the strictest structural interpretation would grant that revolt is an aggregation of individual decisions; for peasants must ultimately themselves choose to revolt. The assertion made by structural theories like Skocpol's is that the nature, scope, and timing of such a decision is structurally determined by the position of the peasantry as a social class. As was just noted, Skocpol holds that class grievances are "collectively perceived and acted upon." Across cases, then, the model requires that peasants act consistently when confronted with similar structures, that is, that peasants employ a single and mutual decision rule.

For Skocpol's theory, the model of action can only be that when confronted with a revolutionary choice, peasants employ a decision rule akin to a microeconomic rational maximizer and are thus driven by the desire to secure maximum benefit. Recall that given tactical freedom, the understanding that sanctions will be swift and sure prevents peasants from acting collectively on their structurally determined and

class-inherent desire for improvement. In short, peasants weigh the potential benefits of rebellion against the perceived costs (likelihood of retaliatory sanctions). When sanctions are understood as being swift and sure, the cost of rebellion is too high. However when sanctions are ineffective or impotent (as is the case in the administrative state crisis Skocpol describes), the decisional calculus changes, and rebellion becomes feasible.

The notion that a model of peasants as rational maximizers is central to Skocpol's theory is further supported by the explicit denial of alternative motivations. Peasants are not mass-mobilized by fundamentally new goals, values, or system-transcending ideologies (1979, 114). Peasant activities in revolutionary times and in mere rebellions are not fundamentally different (ibid). Both are directed at achieving immediate and local goals, such as greater access to land and relief from high taxes on their surpluses. Peasants participating in revolts are not swept up into a revolutionary fervor; rather, they are characterized as pursuing latent, class-based desires and demands in a more permissive structural environment. The point here is not only that the rational maximizer model of peasant action is implied throughout but that it is also a necessary assumption if the structural hypothesis is to work. It is only within the context of such an assumption that it can be argued that structural conditions "lead" to revolt.[6]

However, there are good theoretic reasons to expect that this nexus of rational egoism and permissive structures will not result in revolt. Here, an understanding of free-rider logic is essential. When confronted with the decision of whether or not to contribute to the provision of a public good (such as revolution), a rational individual will choose to free-ride rather than contribute a fair share. As Mancur Olson puts it: "The notion that groups of individuals will act to achieve their common or group interests, far from being a logical implication of the assumption that the individuals in a group will rationally further their individual interests, is in fact inconsistent with that assumption" (1965, 2). The assumption of rational maximization as a decision rule on the part of individual peasants does not logically lead to the conclusion Skocpol's analysis assumes, namely, that peasants will collectively act to address their class-defined goals and aspirations. Thus, while the structural hypothesis directs itself to explaining the timing and scope of peasant revolts, the individualist assumptions implicit throughout would seem to argue for a nonevent.

In fact, there are historical instances in which, given the necessary and sufficient structural conditions as described by Skocpol, peasants choose not to revolt. Ronald Waterbury (1986) in a detailed empirical analysis reconstructs and compares the economic and social conditions across the states of Oaxaca and Morelos in Mexico during the Zapatista Revolt. Waterbury notes that both states were predominantly populated by rural peasants. In Morelos the traditional clientilist economic system had undergone changes that were the result of modernization and centralization—a manifestation of the attempt to increase sugar production. In this system, dominated by the modern hacienda, the peasantry as a class had little or no autonomy in local decisions, owned very little land, and was forced to sell labor as a primary income source. This structural arrangement left peasants in direct competition with each other for work and land. They did not possess the tactical freedom with which to organize collective action. Conversely, Oaxaca did not undergo modernization; as a result, peasants enjoyed much local autonomy, owned a significant amount of land, and possessed a strong sense of community solidarity.

Skocpol's structural theory would predict that during the period of virtual state collapse in the early 1900s Oaxacan peasants, who were structurally positioned to take advantage of decreased state sanctioning, would openly revolt. However, Waterbury describes the Oaxaca response to the Zapata Rebellion as "reactionary," or at best neutral (1986, 411). Peasants opposed Zapatista leaders, who were ostensibly fighting to improve their condition. Interestingly, Morelos, with its system of modern haciendas and almost complete local control over local peasant life, was visited with active and violent rebellion.

The main thrust of this description is not to challenge the conclusions Skocpol draws about the importance of a structural analysis in determining the susceptibility of specific types of regimes to revolution and conditioning its eventual, often unintended, outcome. On this score, structural analyses are fundamental. Rather, the suggestion is that an exclusive focus upon structural dynamics had led to an incomplete development of a necessary nexus between structure and action and that as a result, the theory cannot account for the widespread peasant revolt against landlords, which Skocpol considers fundamental in explaining larger social revolutionary transformations.

While Skocpol's account displays the greatest theoretic rigor, other largely structural accounts have not completely ignored individual motivations. For example, Hooglund (1982), Tutino (1986) and Walton (1984) all focus primarily upon the deleterious effects of structural transformation on peasants (land reform, market penetration, subsistence autonomy, and security crises, etc.). These transformations, it is argued, lead to a change in peasant outlook that in turn, accounts for revolt. However, in agent–structure terms, the model of individual action is lacking. Walton emphasizes "noninevitability," Tutino focuses upon the shift from resignation to moral outrage, and Hooglund highlights the decay of regime legitimacy. These (new) peasant perceptions, while related to the structural dynamics described, can not adequately confront the free-rider critique.[7] Again, the desire for collective action is, by itself, inadequate.

METHODOLOGICAL INDIVIDUALISM

In an extensive analysis of peasant collective action in Vietnam, Popkin argues that "participation in revolution can be seen not as an anachronistic attempt to restore a fictional golden age, but rather as a calculated effort to improve one's future position" (Perry 1982, 890). The fundamental analytic concern is not the extent of the threat to a social class but, rather, the level of risk to the individual (Popkin 1979, 245). In contrast to Skocpol, Popkin begins at the individualist end of the methodological spectrum by asserting at the outset that peasants, in virtually all of their activities (economic, political, social, familial) employ a self-interested rational maximizing decision rule. Specifically, "individuals evaluate the possible outcomes associated with their choices in accordance with their preferences and values. In doing this they discount the evaluation of each outcome in accordance with their subjective estimates of the likelihood of the outcome. Finally, they make the choice which they believe will maximize their expected utility" (p. 31).

In focusing upon individual intentional action, Popkin is rejecting the claim that social structures should be the central analytic variable. For example, a structurally generated collapse of effective peasant sanctioning (central for Skocpol) is not an important variable, because "it must be stressed that village officials and landlords had to be undermined. Their power had not 'decayed' during the colonial period. . . . Each of these movements had to build a power base while opposed by rural and economic elites, who had been strengthened, not weakened by colonial policies" (1979, 187). Seemingly, social-structural conditions have no meaningful impact upon the potential of such groups to organize the peasantry effectively. The aggregation of intentional individual actions, rather than permissive structures, accounts for revolution. Concomitantly, the objective condition of the peasantry as a social class—its position in the social structure—is unimportant. For Popkin, peasants are not permanently revolutionary, since there is "no clear relationship between substance threat (or decline) and collective response" (p. 245). Rather, the decision to act is a function of the ability of revolutionary organizations and the political entrepreneurs who lead them to provide and create incentives that induce participation. Through the strategic manipulation of collective goods, political leaders entice rational egoistic peasants to provide support for revolutionary organizations. Here, Popkin begins where my earlier account of Skocpol ends, that is, with an explicit acknowledgement of the impediments to successful collective action (free-rider logic), which Skocpol does not address.

Under the heading "The Political Economy of Revolution" Popkin identifies four conditions under which a peasant would contribute to a revolutionary movement. Initially, Popkin is forced by his own findings to acknowledge the existence of moral compulsion as a motivation but then dismisses it by stating that "situations in which [such] contributions are widespread are infrequent in peasant society" (1979, 254). Next, Popkin argues that peasants participate when (1) it is rational in cost–benefit terms, (2) there are sufficient selective incentives, and (3) the contributions of others are contingent upon one's own (ibid). While these remaining three conditions account for contributions in a context of, say, interest groups (as in Olson's original formulation), they are problematic when applied to the large-scale social pressures of revolution. Each will be discussed in turn.

Cost–Benefit Calculus

Little is required of a revolutionary organization whenever, from an individual's perspective, it pays to contribute in simple cost–benefit terms. Conceptually, there are two instances in which this is the case. The first requires that the benefits of participation flow only to those who contribute; by definition, such a good is "excludable." Popkin demonstrates that in the Vietnamese case, the emergence of support for revolutionary organizations as such was largely a function of their ability to organize peasants into projects addressing their local and immediate concerns and where the benefits could be selectively allocated to those who contributed to the project (insurance schemes, the raiding of granaries, etc.). However, the notion of revolution implies systematic reorganization. In this regard, revolutions approach a "pure" public good that is by definition a good from which individuals cannot be excluded. Additionally, Popkin cannot account for individual revolutionary choices where the dominant incentive is to defect. The same dilemma that confronts Skocpol's analysis reemerges here. Class or group rationality that would imply revolt is not consistent with individual rationality and the incentive to free-ride.

By the second variant of simple cost–benefit calculations, rationality would dictate participation if the costs of defection outweighed those of contributing to the revolutionary effort. While Popkin himself only mentions this theoretical possibility in passing, Michael Taylor, expanding upon Popkin's work, argues that membership in a village community itself makes participation rational. Taylor emphasizes the notion that defection often has social costs:

> A number of studies have shown how important "social" incentives are in mobilizing people into collective action. An individual joins or contributes or participates because he is asked tacitly or overtly pressured by friends, colleagues workers or co-members of the association's local branch or cell. He cannot say "no" to them; he is afraid of losing their approval, respect or cooperation. (1988, 84)

When applied to revolution, however, such an insight is problematic in at least three ways: First, there is little data (none is provided by Popkin or Taylor) predicting that villagewide contempt would be sufficient threat to lead a peasant to incur the risks of engaging in open revolt—which is often associated

with increased probability of death. Second, and still more fundamental, Taylor's argument assumes a community already committed to revolution when the individual peasant is confronted with a revolutionary choice; that is, there must be some threshold (number of peasants) of community commitment below which ostracization becomes ineffective. Hence, while an individual in an already-committed village might be swayed by social pressure, this explanation cannot account for revolutionary choices up to $N - 1$, where N is the minimum level of commitment required for social pressure to be effective. Finally, a reliance upon the village as a key variable explicitly acknowledges the importance of accounting for social structure.[8] It depends, for example, upon the nature of relations within the local community, with other communities, and between the community, landlords, and state officials (Taylor 1988, 68).

Selective Incentives

The next condition in which revolutionary collective action is possible involves, in effect, decisions not related to revolution at all. Here, Popkin invokes the *by-product* theory of collective goods. When a collective good is the by-product of an organization's primary function and this primary function is the source of organizational support, it is rational for an individual to contribute if the benefits of this primary function flow exclusively to those who contribute.[9]

Here again, the nature of revolutionary collective action, which is often long and protracted, makes this description problematic. For the American Medical Association to lobby Congress, nothing more is required of doctors besides the payment of dues. The problem is that revolutionary struggles require increased popular action—and therefore contributions—above and beyond that which was initially sufficient for the organization to emerge and prosper. Examining village mobilization by the communists in Aman leads Popkin to note that organizing villagers into literacy campaigns, effecting improvements in water storage and irrigation facilities, and reforming the communal land system (including a more favorable tax system) were strategies directed at attracting peasant support away from rival organizations. However, successful as they were in garnering support and organizing the peasantry, these contributions per se can in no sense be directly translated into revolutionary action. Teachers' contributions of skill for the literacy campaign or farmers' contributions of labor to improve water delivery systems do not create protracted revolutionary war as a by-product. The question remains, Why revolt?

If, after a successful organizing campaign, peasants are asked to step up their contributions and actively revolt, they are in effect being asked to contribute to a new public good. This violates the conditions of by-product theory as stated by Olson (1965, 132–33); that is, the collective good of revolt is no longer a by-product of organizational support. Rather, individuals are now being asked to contribute *directly*, not indirectly, to revolution. Rationality here would dictate a separate calculation of the benefits and costs of revolution distinct from considerations or organizational support. As noted, in such a calculation the dominant incentive is to free-ride. The collective good of sustained revolutionary opposition as a by-product is unlike organizational lobbying by the American Medical Association, in that the former requires more of individuals than support for an organization.

Contingent Contributions

The final condition under which rational peasants would contribute to collective revolutionary action is that each individual must view a personal contribution as critical and necessary for the success of the overall collective project. This requires that the overall goal be broken down into "many small independent pieces, all of which are necessary," so that, since "each person has a monopoly over a necessary factor for the final goal, all contributions are essential" (Popkin 1979, 257). Here, peasants are acting as purchasers of private goods in the marketplace, with the very important stipulation that such a purchase will be effective only if "it is known for certain that all the others designated as contributors will contribute" (van de Kragt, Orbell, and Dawes 1983, 116). Such a stipulation, in turn, suggests that if individuals act "unreasonably" or if there is some reason to believe that others will not act rationally, the logic for an individual's contributing immediately collapses (p. 119). In revolutions, there are good reasons to believe that it will collapse.

As formally developed, investigators looking at how criticality of contribution impinges upon individual decisions to participate have assumed that the contributions required of each individual are essentially the same. Additionally, there is some preliminary evidence to suggest that individuals project their own expectations about cooperation to others (Dawes, Mctavish, and Shaklee 1977). Yet in the process of revolution, some tasks, by their very nature, are likely to demand more from some individuals than others. For example, helping in the construction of a dike entails less risk than engaging state forces in battle.

Therefore, Popkin is here burdened with the dilemma of demonstrating that the most difficult task (in terms of subjective estimation of costs) is seen by *every* individual as rational, so that noncooperation is not projected to others, and defection does not become the rational strategy. One might employ a class-based analysis and argue that the observed living conditions of a particular class or group are so bad that for all necessary tasks, cooperation is seen by all as rational. But this would directly counter Popkin's explicit denial of the need to look at social structure, group rationality, and the objective living conditions of the peasantry. Further, such a perspective would not allow the observer to account for individual decisions not to support a revolution-

ary organization by distinguishing between organizational failure and an a priori lack of necessary structural conditions. Thus, such a perspective is indistinguishable from a structurally deterministic explanation.

This argument implies that economic rationality alone inadequately accounts for sustained revolutionary movements. The model of action employed here was initially developed without consideration of the types of social-systemic dynamics central for an understanding of revolutions and then was applied to peasant revolt.[10] As a result, it is unable to provide viable predictions about revolutionary action, suggesting that this is a fundamentally different type of decision.

RISK-SEEKING CHOICES

If we are to understand peasant revolutionary action, then fidelity to Coleman's (1986) initial argument (that explanations of social phenomena need to be grounded in a model of individual action) dictates that we begin by reformulating the model of individual choice. Such a model must (1) identify what sort of decision rule is in effect (this, I shall argue, requires reference to social structure) and (2) demonstrate how this rule, when employed, accounts for revolutionary collective action.

Central to classic microeconomic rationality is the assumption of invariance. As Quatrone and Traversky note: "Perhaps the most fundamental principle of rational choice is the assumption of invariance. This assumption, which is rarely stated explicitly, requires that the preference order among prospects should not depend on how their outcomes and probabilities are described and thus that two alternative formulations of the same problem should yield the same choice" (1988, 727).

This assumption carries two important characteristics. First, decisions are outcome-oriented. They are based upon the utilities (weighted by their probability) that are the result of alternative choices. Second, the decision rule for a choice between two gains and a choice between two losses is essentially the same; both are evaluated in terms of movement along a linear and preestablished utility function.

As an alternative, *prospect theory* emerged from a branch of empirical investigation into the structure of individual choice that Simon (1985) has termed *bounded rationality*.[11] Results in this field suggest that individuals employ identifiable decisional heuristics that generate outcomes systematically deviating from the predictions of rational choice. Additionally, this research suggests that the choice of decision rule is contextually contingent.

Bernoulli (1954) initially altered the classic model and demonstrated that the relationship between money and utility is "concave"—consistent with the observation that a gain of $500 contributes less than twice the utility of a gain of $250. The concavity of the utility function predicts that individuals, in their choices, are *risk-averse*, defined as preferring a sure gain over a gamble offering a higher expected payoff. Subsequent research in the behavioral decision tradition added that the location of the status quo is important. Prospect theory makes a distinction between choices involving gains and choices involving losses and requires, therefore, the notion of a neutral status quo reference point (Figure 1). As Quatrone and Tversky put it, "According to prospect theory, an individual's attitude toward risk depends upon whether the outcomes are perceived as gains or losses, relative to the reference point" (1988, 722).

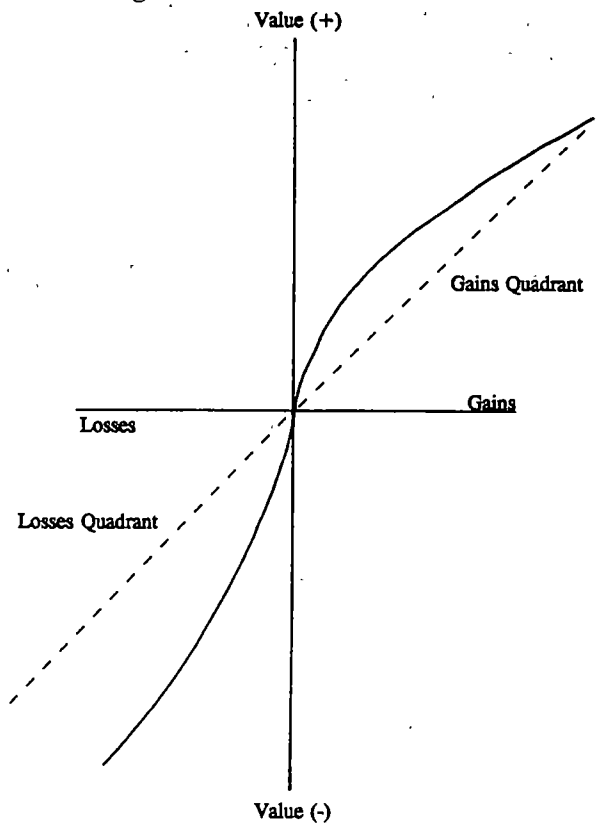
Here, the money-to-utility function in the gains quadrant (the upper right quadrant) is concave, consistent with Bernoulli's modification of the invariance model. The losses quadrant (lower left) is convex,[12] reflecting, for example, the observation that losing $500 does not seem twice as bad as losing $250. While individuals are risk-averse in the gains quadrant, the data show them to be risk-seeking in the losses quadrant.[13] The point here is that the decisional determinant is not the expected outcome, but how the choice is perceived relative to the status quo (reference point). Indeed, expected outcomes in one sense become irrelevant, since "the same decision can be framed in several different ways; different frames can lead to different decisions"—that is, "framing effects arise when the same objective alternatives are evaluated in relation to different points of reference" (Kahneman and Tversky 1982, 165). In this framework, explanations of individual choice are located in individual perceptions of gain or loss relative to the status quo.[14]

Both Popkin and (implicitly) Skocpol rely on the classic model of rational choice as a foundation for a model of individual action. If, however, the modified, status-quo-sensitive model is to move beyond a tool for criticism and contribute to a positive theory of revolutionary action, then a method is needed for determining when revolutionary decision becomes "framed" as a choice between gains or between losses. Accounting for revolutionary action requires conceptualizing a social-structural frame.

A *social frame* is defined here as the perception by individuals that participation in the existing social-structural arrangement means either gains or losses. However, in the standard explication of prospect theory, an individual who opts for the status quo garners no gains and incurs no losses. This initially appears problematic. However, there are at least two reasons why it is not. Adhering strictly to the framing effect as I have described it, the status quo will be placed in the losses frame if a deterioration of current conditions is expected to continue. Here, one who opts for the status quo can expect increased loss. Furthermore, there is no theoretical necessity that the reference point represent only current empirical conditions. Kahneman and Tversky have noted that "the reference point is the state to which one has become adapted" and that there are many cases in which "the reference point is determined by events that are only imagined" (1982, 171–172). Clearly, then, an individ-

FIGURE 1.

The Framing Effect



Note: The dotted line represents the original invariance model. The curve represents a modified, status quo sensitive, utility function suggested by the framing effect.

ual's reference point for any particular decision can be associated with some past state viewed as normal. Current conditions can be viewed as an "outcome" that is placed in the gains or losses quadrant relative to a subjective reference point that is itself reflective of past conditions.

In either scenario, this implies that peasants living through social-structural transformations that lead, for example, to an increased vulnerability to subsistence crises (market penetration into closed rural villages, regime change, famine, etc.) would view a choice in favor of status quo not as neutral but as a loss. For example, Eric Wolf argues that market penetration into rural villages damages the peasant class:

> The peasant economy was burdened by the pressure of taxation, by demands for redemption payments, and by the increased need for industrially produced commodities on the part of the peasantry itself. All together, these various pressures disrupted the precarious ecological balance of peasant society. Where the peasant had required a certain combination of resources to effect an adequate living, the pressures on these resources broke that ecological nexus. (1986, 175)[15]

The decision to revolt can now be (re)conceived as a choice providing a (usually small) likelihood of success and therefore (although this is often not the actual outcome) of improvement in the living conditions of the peasantry. It also involves some (probably larger) chance of failure, which would leave the individual less the costs of participation and might involve further costs in the form of state reprisals against a rebellious peasantry.

If an individual peasant's understanding of the existing social structural arrangement is placed in the losses frame, when confronted with a revolutionary choice, the decision to be made is between two losses: not to revolt entails the continued losses from the status quo, and to revolt entails an expected value of increased loss attendant to participation but also some smaller prospect for improvement. Here, framing predicts a greater predisposition toward risk (revolt) over the certainty of loss from the status quo in the attempt to return to the neutral reference point, that is, toward mitigating losses. Alternatively, if the current condition is not subjectively placed in the losses quadrant, then an individual will forgo the opportunity to improve his or her condition and opt for the status quo (i.e., reject revolutionary appeals).

FRAMING BY REVOLUTIONARY ORGANIZATIONS

If a losses frame is a first necessary condition for revolt, then the initial task for a revolutionary organization is to adopt—or construct—a worldview that effectively communicates to individual peasants the understanding that existing social-structural arrangements are worse than in some "normal" past.[16] It is interesting to note that this is nicely paralleled in a structural Marxian analysis. In classic Marxism economic terms, the argument is that technical improvements in the production process, given a labor theory of value, generate a series of crises leading to the monopolization of capital that in turn (and at the same time) increases the size of the proletariat and decreases the demand for labor. The result is depressed wages and the reality of not meeting subsistence needs (Loucks and Whitney 1983). Speaking more directly to peasants, Mao argued that "a man in China is usually subjected to three systems of authority" (political, clan, and religious) resulting in peasant oppression (Mao 1927, 44). Both these accounts describe the existing order as a process that operates systematically to the detriment of the individual. They also describe a process of systemic closure. There is no niche in the existing system where an individual can escape oppression.

However, there are also limits on the degree to which the ideology employed by revolutionary organizations can frame revolutionary choices; and these limits are the very reason why an analysis of social structure is necessary for an understanding of revolution. Ideological descriptions "assert the truth of relations between datable, localizable things in an objective world" (Ingram 1987, 23). In short, there is

a point at which a value system must ultimately access aspects of empirical reality for its legitimization. If it does not, the contradictions between ideological assertions about the world and an individual's subjective assessment of his or her social context prevents its adoption. The successful framing of a revolutionary choice, then, is not separate from the existing structural context but is, in an important sense, related to it. This is a central point and suggests the degree to which the nature of the link between ideology and the empirical conditions I have described can inform our understanding about revolutionary processes.

In addition to the framing of choices, a revolutionary organization must overcome what William Hinton has noted in peasant communities as an "all-pervading individualism engendered by the endless, personal struggle to acquire a little land to beat out the other fellow in the market place. Peasants individually driven to bankruptcy viewed economic disaster not as a social but as a personal matter, to be solved in isolation by whatever means came to hand" (quoted in Popkin 1979, 251). To overcome fatalism, a sense of agency is necessary. Aside from fostering a predisposition towards risk, structural framing is central as it shifts the locus of failure from the individual to the existing set of social-structural relationships. Individualistic fatalism is replaced with the notion that change can be effected by individual action through organizational means. Recall that when faced with losses, in the attempt to return to a normal condition, individuals often embark upon risky ventures implying only a small probability of success. In other words, an individual's subjective assessment about the likelihood of successfully negating the losses inherent in the status quo through revolution need not be large to initiate action. This suggests that the hurdles to effective organization, while significant, are not as high as an assumption of macroeconomic rationality would posit, since certainty of a positive payoff is no longer a necessary condition for participation.

Recall the case of the lack of revolt in Oaxaca, which contradicts Skocpol's structural theory. This could be explained by the fact that individual perceptions by peasants of the stable economic structure in Oaxaca did not place them in the losses frame. As prospect theory suggests, they were predisposed to forgo the opportunity to improve their condition (meager as it was) through the risky venture of revolutionary action (afforded them by a collapse in effective sanctioning), instead choosing the certainty of the status quo. Alternatively, in revolutionary Morelos, peasants had become marginalized. Loss of land, income, and stability were all the result of the modernization drive. Here, the same revolutionary appeals confronted peasants with two choices both having an expected value of increased losses. A predisposition toward risk led peasants to choose revolt in the attempt to mitigate losses.

Next, recall Popkin's application of by-product theory in a description of peasant mobilization by the communists in Aman. While the provision of selective incentives (literacy campaigns and changes in the communal land system) could account for the decision to give support to communist organizations, it could not account for revolutionary action, which required a separate, additional calculation. Indeed, in Popkin's own historical account, such tactics were effective in attracting peasant support away from rival organizations only after the seizure of government by the communists. As Popkin puts it, "Seizing power, in other words, was but the beginning of the process of gaining control of the villages and of bringing about distribution in the countryside" (1979, 223).

Prior to the August Revolution in 1945, Popkin describes the condition of Communist party as being "at its lowest point" (1979, 218). Later, events in the Tonkin mountain region put individual peasants at extreme risk and reinvigorated the party. Popkin describes the period of the Japanese occupation, in which

> large tracts of land were forcibly diverted from rice production to Jute, and as large quantities of peanuts and rice were commandeered for the war effort, famine entered Tonkin. As early as 1943, with American planes bombing the rail lines to Saigon and preventing the shipment of food, there was starvation in the north. By 1944, there was mass starvation that finally took between five hundred thousand and two million lives in Tonkin. (pp. 219–20)

Now, revolutionary and nationalist appeals by the communists confronted peasants with two choices. The first, to reject revolutionary appeals and do nothing, entailed the continued loss of the status quo. The second, to join the communists and revolt, offered the risky proposition of action, which would entail increased risks and costs but also offered some possibility of a return to normalcy. When combined with a redoubled propaganda effort emphasizing nationalism and the removal of foreigners, Tonkin was perceived as a losses frame, and the attendant predisposition toward risk allowed the Communist party to garner enough support to carry off the August Revolution. It was largely Tonkin peasants who "emerged from the backlands and mountains to mount the August Revolution" (1979, 221).

CONCLUSIONS

Current theories of revolution, whether rational choice or structural, make at least one of two incorrect assumptions that inhibit their ability to resolve the agent–structure problem: (1) the role of structures is narrowly understood as simply confining or delineating the boundaries within which intentional action takes place; and (2) only a single model of individual action is assumed, and this model transcends structural context.

Structural accounts are necessary tools for understanding the susceptibility of regimes to revolution and defining the universe of conceivable postrevolu-

tionary outcomes. Structural accounts of revolt speak best to potentials for action but cannot alone account for individual choices to revolt. More importantly, wedding a microeconomic decision rule to such structural accounts inadequately confronts the free-rider dilemma. As Coleman has recently stated, the structural benefits of successful revolutionary transformation are largely "independent of an individual's participation and provide him with no incentive to act. In fact, participation sometimes even leads to death. Yet in some revolts there is mass participation. How can this be? (1990, 495).[17]

Alternatively, sophisticated rational choice models, like Popkin's, have borrowed from the literature on collective action in an attempt to account directly for revolutionary choices. Here, the description of how and why peasant organizations gain support and prosper is enlightening. However, without addressing explicitly the social-structural context in which *revolutionary* action is nested, the logic of selective incentives breaks down, leading one to conclude that the choice to revolt is distinct from the decision to participate in a peasant organization as such.

Indeed, the weight of my discussion suggests that in certain circumstances, Coleman's rhetorical question needs to be reformulated. Rather than the aggregation of individualist rational calculations, collective rebellion should be conceived as an interaction between structure and intentional action—grounded in an empirically observed predisposition toward risk in the face of a choice between losses. Therefore, beyond a structuring of incentives, social structure can lead individuals to evoke decision rules *alternative* to microeconomic rationality. Collective action is then the aggregation of individualistic decisions to mitigate losses in certain structural contexts. Of course, in a comprehensive explanation of social revolution, an agent–structure theory of revolutionary choice is compatible with both methodological structuralism and rational choice models. Both structural potentials (regime susceptibility) and effective organizations are themselves necessary conditions for social revolution. Agent–structure theorizing is thus a necessary supplement to extant approaches attempting to explain social action.

The weakness of this conceptualization, however, is that it only partially resolves the agent–structure problem. In this description of revolt, risky social structures are assumed a priori—their genesis is not accounted for. The strength is in the attempt to address agent–structure questions (which of course extend beyond revolution) explicitly and thus avoid the pitfalls of rational or structural reductionism.

Finally, in this framework, the role of revolutionary ideology is central and needs to be reintegrated into a model of peasant revolt. Both Skocpol and Popkin explicitly deny its importance. Simon notes that in order to deduce the boundedly rational choice in a given context, "we must know the choosing organism's goals, *the information and conceptualization it has of the situation*, and its abilities to draw inferences from the information it processes" (1985, 294; emphasis mine). It is here than an examination of ideological premises as "framers" would be useful. Knowledge about the operation of ideas in local contexts, grounded in the local experience of individuals, is, then, a necessary component for an understanding of peasant revolt and one that calls for closer examination.

Notes

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1. Other examples here include Farhi's (1988) structural analysis (building explicitly on Skocpol's work) on Iran and Nicaragua. Alternatively, Muller and Seligson (1987) employ exclusively structural variables in a quantitative cross-national study of insurgency. The recent work on rational choice explanations of revolt includes Taylor's (1988) edited volume; see also Muller and Opp (1986). For compelling general arguments in favor of methodological individualism and structuralism see Coleman's *Foundations of Social Theory* (1990) and Tilly's *Big Structures, Large Processes, Huge Comparisons*, respectively (1984).

2. Because the argument here is largely theoretical, the empirical cases discussed are intended to highlight central theoretic points; they are not meant to be sufficient evidence for a "test" in themselves. The application of cases here is informed by Eckstien's (1975) discussion of the disciplined idiographic use of case studies.

3. Structural and individualistic theories of revolution have been discussed together in the literature reviews of specific revolutions, but these lack any integrative attempt. See, e.g., Akhavi 1989 on Iran; Chilcote 1989 on Portugal; and Duran 1982 on Mexico. Hawes's (1990) synthetic attempt, while it ignores the agent–structure problem, is a good summary of contending approaches.

4. Skocpol defends the ability of structural methodologies to generate discriminating tests (1979, chap. 1, esp. 33–40).

5. Note that Meadwell (1988) criticizes this structural hypothesis on its own terms by arguing that the relationship between autonomy and revolt is spurious and that solidarity is theoretically neither a necessary, nor a sufficient, condition for revolt.

6. For a more complete discussion of the implicit, but inescapable, relationship between structural determinism and microeconomic models of choice see Barry Hindess's *Political Choice and Social Structure* (1989).

7. Nor is it evident, e.g., that moral outrage or a decline in regime legitimacy necessarily leads to revolutionary choices. This is essentially the same criticism leveled at the earlier "relative deprivation" hypotheses of Davies (1962) and Gurr (1970). Specifically, there is no necessary reason to expect that the frustration one experiences, if translated into aggressive action, will be directed at overthrowing the status quo, rather than being vented elsewhere. Thus, moral outrage, or a decay of legitimacy, alone cannot account for rebellion against the state.

8. Taylor is not entirely antithetical to such an accounting despite his explicit defence of the analytic utility of microeconomic rationality.

9. Olson's description of the American Medical Association provides an exemplary case. A collective good provided by the AMA is influence (lobbying) in the formulation of tax policy and health legislation. Because any member of the medical community can receive such benefits, there is no incentive to contribute to the AMA. However, the AMA also provides to its members such noncollective, selective benefits as information on tax loopholes, new medicines, and support

in malpractice litigation, which only members who contribute can receive. Individual contributions are in order to receive these selective benefits, and any surplus resources collected by the AMA are then devoted in part to the lobbying effort. In this sense, lobbying is a by-product of rational choices by individuals who wish to garner other selective incentives.

10. Often, otherwise solid theoretical work on rational choice models of collective action fails to address the social-structural context in which such action is nested (see, e.g., Oliver 1980; Oliver and Marwell 1988; and Oliver, Marwell and Teixeira 1985).

11. For an extended discussion of "bounded rationality" and its role in political science see Simon 1985.

12. The only other difference is that this curve is steeper than in the gains quadrant, representing individuals' heightened sensitivity to losses.

13. In other words, experiments in the framing literature suggest that rather than choosing a certain smaller loss, individuals attempt to mitigate all loss and return to the status quo by choosing a risky option even though it has only a small likelihood of success. This is the case even if the gamble has a higher expected negative value than the certain loss.

14. As an example, Kahneman and Tversky (1982) have described the essential characteristics of framing through an illustration.

> *Gains Frame.* Imagine a choice between two options that imply personal gains: (1) a sure gain of $80; (2) a risky venture with an 85% chance of winning $100 and a 15% chance of winning nothing. When confronted with this choice, most prefer the sure gain over the risky venture even though the expected monetary outcome is $5 less.
>
> *Losses Frame.* Now imagine a second set of options that imply personal losses: (1) a sure loss of $80; (2) an 85% chance of loosing $100.00 and a 15% chance of loosing nothing. Here, individuals prefer the gamble over the sure thing even though the expected monetary loss is $5 greater.

Aside from the fact that in both scenarios individuals deviate from the predictions of simple microeconomic utility maximization, the central point here is, "Preferences between gains are risk averse and preferences between losses are risk seeking" (ibid., 160).

15. Here, in the attempt to infer a losses social frame, a structural or class-based analysis is quite useful. For analytic purposes, then, a losses frame can be inferred empirically. The intent is to follow Snidal's (1985) suggestion that incentive structure (here a losses social frame) be accessed *independently* of the observed choices of individuals, thus avoiding the tautology trap that captures some rational choice explanations.

16. This point should not be interpreted as an assertion that revolutionary movements are founded on the notion of "returning to the status quo." The notion of framing is not inconsistent with the understanding that revolutionary organizations most often aspire to move beyond normalcy, to an improved condition.

17. Coleman's answer is akin to Taylor's in that he relies upon community (or group) norms to provide effective incentives to participate (see Coleman 1990, 495–96).

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POLITICS, INSTITUTIONS, AND WELFARE SPENDING IN INDUSTRIALIZED DEMOCRACIES, 1960–82

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We examine the roles of democratic politics and political institutions in shaping social welfare spending in 18 contemporary capitalist democracies. We explore the social spending consequences of government partisanship, electoral competition and turnout, and the self-interested behaviors of politicians and bureaucrats, as well as such relatively durable facets of political institutions as neocorporatism, state centralization, and traditionalist policy legacies. Pooled time series analyses of welfare effort in 18 nations during the 1960–82 period show that electoral turnout, as well as left and center governments increase welfare effort; that the welfare efforts of governments led by particular types of parties show significant differences and vary notably with the strength of oppositional (and junior coalitional) parties; and that relatively neocorporatist, centralized, and traditionalistic polities are high on welfare effort. Overall, our findings suggest that contrary to many claims, both partisan and nonpartisan facets of democratic politics and political institutions shape contemporary social welfare effort.

To date, no consensus has been reached on the controversy launched by Cameron (1978), Castles and McKinlay (1979), and Stephens (1979) over how politics matters for welfare spending in industrialized democracies. Despite much quantitative empirical study, research has not determined to what extent, if any, welfare policies are shaped by government partisanship, the self-interests of government personnel, mass electoral preferences, or neocorporatist institutions.[1] Dissensus over these recurrent questions persists in large part because theoretical frameworks and empirical models of individual studies have been too narrow. Typically, quantitative studies have only focused on one or another subset of already controversial explanatory factors (e.g., left party strength), thereby courting biased findings by omitting variables that may be correlated with included variables (Johnston 1984, 259–65). Moreover, quantitative studies have frequently risked unstable findings by using coarse measures and minute samples—frequently with as few as 15 cases. In short, unreliability, as well as bias, has impaired the progressive cumulation of research findings.

Not only have individual studies been narrow. As we shall illustrate, the range of explanatory factors explored in the whole quantitative literature has been limited. This narrowness has exacerbated discensus over the political causes of welfare spending by excluding quantitative analyses of political forces often highlighted in historical and theoretical works—in particular, political institutions and democratic oppositions. For example, as regards the historical salience of political institutions, Germany's 1880s inaugurations of national social security programs; turn-of-the-century Scandinavian innovations in universalistic welfare legislation; and Britain's postwar consolidations of universalistic, flat-rate, uniform payment social security systems all arose within highly centralized states (Baldwin 1990).[2] Moreover, theoretical reasoning about the relevance of political institutions to welfare spending has expanded significantly since certain seminal works (Dahl 1982, chap. 4; Evans, Reuschemeyer, and Skocpol 1985; Lijphart 1984; and Powell 1982). Yet consideration of democratic political institutions has been scarce in the burgeoning quantitative–empirical literature on welfare spending (but see Cameron 1978). Similarly, while strategic interactions of partisan governments with their oppositions figure prominently in both political history and theory (Baldwin 1990; Downs 1957), influences of democratic oppositions upon government welfare legislation have been neglected in quantitative studies of welfare spending.

We attempt to remedy these problems by simultaneously assessing social welfare impacts of government partisanship, electoral competition and turnout, bureau voting of public sector interest groups, and reelection-oriented behavior of incumbent politicians. More novelly, we test for policy influences of democratic oppositions (and "junior partners") and greatly extend consideration of democratic political institutions. We use 23-year times series for 18 nations, significantly expanding the data base of conventional studies; and we assess social welfare impacts of democratic politics and political institutions in the context of models that account for impacts of welfare needs, economic capabilities, and economic structure.

To anticipate our major conclusions, high voter turnout, as well as center and left government leadership, augments welfare effort. Center-led governments do so even more than Left-led ones. The strength and partisanship of democratic oppositions matter for effects of partisan governments. Social welfare effort is enhanced by neocorporatist institu-

TABLE 1

Political Influences on Social Welfare Effort in Capitalist Democracies across Three Theoretical Perspectives

TYPE OF FACTOR	PERSPECTIVE: SOCIAL DEMOCRATIC CORPORATIST	POLITICAL DEMOCRATIC	STATIST
Political agency	left and center party strength	voter turnout; electoral competition	state-sector employment; electoral cycles
Political institutions	working class organization and political incorporation	majoritarian and consensus aspects of political systems	political/administrative capacity; conservative policy legacies

tions, state centralization, and traditionalistic policy legacies.

THEORY

Social Democratic corporatist, political democratic, and statist theoretical perspectives on the politics of welfare effort help structure our inquiry. Drawing on both neopluralist and neo-Marxist theories, the social democratic corporatist perspective emphasizes the importance of class-linked parties and interest organizations for welfare policy determination (e.g., Korpi 1983; Stephens 1979). The *political democratic* tradition of scholarship stresses the welfare policy impacts of such nonpartisan facets of electoral politics as electoral competition and turnout (Dye 1979; Pampel and Williamson, 1989). The *statist* perspective highlights the policymaking behavior of incumbent politicians and bureaucrats (Mueller 1979, chap. 6) while placing a special stress on state institutions (Skocpol 1985).

Upon close inspection, each of these perspectives can be said to have an explicitly *agency-centered* facet stressing the political action of voters, groups, parties, political executives, and administrators. Each perspective also has a *political-institutional* facet that highlights structural features of the state resurgent in recent "state-centered" accounts of politics, as well as many other aspects of political institutions (see also Alford and Friedland 1985). The individual political agency and political-institutional components of each perspective are summarized in Table 1.[3]

Political Agency

Social democratic corporatist theory stresses the mutually reinforcing roles of strong left (i.e., Socialist, Labor, and Social Democratic) parties and strong Labor movements. This is the case both for works of Marxian origins, in which government control by left parties and the existence of strong unions are effective, albeit reformist, manifestations of working-class mobilization (Korpi 1983; Stephens 1979) and for neopluralist studies, in which these parties and unions represent consequential political forces in democratic societies (Cameron 1978; Hibbs 1987). Moreover, some scholars working in this tradition have argued that governments led by centrist secular and Christian Democratic parties have advanced income-security policy (e.g., Castles 1982; Wilensky 1981).[4] From the social democratic corporatist perspective, the most central proposition to emerge has been that left and center (non-right) party leadership of government generates higher welfare effort than right and "indeterminate" party leadership. However, some have suggested important roles for oppositional, as well as governing, parties.

The growth of electorally consequential left parties, or "contagion from the Left," has sometimes led conservative governments to welfare-expanding reforms (Esping-Andersen 1990; Heclo 1974). Similarly, welfare policies of Left-led governments may be moderated by strong center and right oppositions (Heclo and Madsen 1987; Przeworski 1985; Stephens 1979). Center parties may vie with left parties for "median voters" and right parties may compete with them for "Tory" workers. Furthermore, the moderation of government welfare policies by democratic oppositions may be supplemented by the presence of junior governing partners. In particular, both Left- and Right-led governments often rely on the formal and informal support of minor centrist parties. When they do, these strategically positioned actors may moderate—indeed, redirect—the welfare orientations of dominant governing parties (Baldwin 1990, chaps. 3, 5).

A few scholars have qualitatively analyzed political oppositions (Baldwin 1990; Heclo 1974). A few more, using large-sample multivariate methods, have at least skirted the topic by examining the electoral support of parties, albeit without regard for their governing or nongoverning statuses (e.g., Korpi 1989). However, no systematic, multinational evidence exists on the consequences of democratic oppositions for governmental welfare policies. Here, we fill this vacuum by hypothesizing that Right-led governments are pressured in a prowelfare direction by electorally consequential left and center oppositions (and junior governing parties). In addition, we hypothesize that Left-led governments are pressured to moderate prowelfare policies by oppositions (and junior governing parties) of the Center and Right. Finally, extending the logic of our argument to all major party types, we conjecture that prowelfare policies of Center-led governments will be intensified by strong left oppositions and moderated by strong right oppositions.

Scholars within the social democratic tradition have downplayed effects of partisanship and stressed the social welfare impacts of such nonpartisan facets of electoral systems as electoral participation and com-

petition (Dye 1979; Pampel and Williamson 1989). Specifically, they suggest that parties in highly competitive electoral environments will make prowelfare appeals (e.g., promises of more income security) to "median" voters in hopes of gaining electoral majorities (see also Key 1949). They also argue that increases in voter turnout pressure governmental incumbents to spend more on welfare because rises in turnout reflect the entry into politics of new, lower-status voters. Recently, they have stressed the acts (or agency) of individual "median" voters and other voters underlying such "systemic" effects of elections. Overall, this perspective suggests that higher levels of electoral competition and of voter turnout lead to greater welfare effort.

State institutions aside, statists have stressed policy consequences of the self-interested behavior of bureaucrats and (incumbent) politicians. With respect to bureaucrats, Niskanen's (1971) assumption that budget-maximizing state bureaucrats act collectively to maximize public spending has been supported by studies of the American states and the industrialized democracies (Garand 1988, 840; Korpi 1989). These considerations suggest that the relative numbers of state-sector employees in a population promotes welfare effort. With regard to politicians, the assumption of vote maximization has grounded the proposition that incumbent politicians seek to expand social spending during the year or two preceding elections (e.g., Tufte 1978). Although more often challenged than supported by empirical evidence, this proposition nonetheless continues to receive frequent attention (e.g., Keech and Pak 1989). Thus, we explore the hypothesis that preelection welfare effort tends to exceed postelection effort.

Political Institutions

Consideration of the statist perspective naturally directs attention to the policy significance of political institutions. After reviewing the treatment of institutions in each perspective, we empirically identify some dimensions of democratic political institutions and pose hypotheses about their consequences for welfare policy. *Political institutions* is broadly contrued to connote a very wide range of elements that extends beyond the formal organizations of the state to encompass political parties and interest associations and, among such associations, even principally economic organizations (e.g., unions) that serve important interest group functions.

Traditions of union and left party strength ground social democratic corporatist politics. By *union strength* we mean a convergence of high levels of union density and organizational cohesiveness (see Alvarez, Garrett, and Lange 1991; Korpi 1983); by *left party strength* we mean a historical pattern of frequent left party governance (e.g., strong cabinet participation) and extensive left party voter mobilization (Korpi 1983; Powell 1982). *Social democratic corporatist politics* connotes a political context conducive to policies that concurrently pursue job and income security and aggregate economic growth (Alvarez, Garrett, and Lange 1991; Hicks 1988a, 1988b). Traditions of strong unions and left parties also connote high probabilities of working-class incorporation into institutions of tripartite (business–labor–state) social and economic policy formation; that is, they connote working-class interest representation, as well as working class political mobilization (Przeworski 1985).

Esping-Andersen's (1990) discussion of state institutions goes beyond traditional social democratic concerns to argue that the policy legacies of the absolutist state and of associated resistance to mass enfranchisement are the sources of a (traditional) conservative configuration of policy traits and goals present in some contemporary welfare states. This configuration, termed a *conservative policy regime*, is marked by vestiges of noblesse oblige, preemptions of progressive reforms, and welfare programs that reinforce the status prerogatives of public (e.g., civil servant) and private (e.g., ethnic) groups, thereby fostering clientelism. Furthermore, we suspect (following leads in Lipset 1983) that more general historical legacies of class and status rigidities may have similar consequences for contemporary welfare states. Explicit attention to such hierarchical or traditionalist legacies may be especially important for the German, Austrian, and Japanese cases, given the Allied decentralization of these nations' state structures following World War II (see Katzenstein 1985; Wolferen 1989). Postwar Allied institutional reforms may have obscured traditionalist legacies (e.g., democratic reforms may have obscured legacies of administrative paternalism) in these nations. As Esping-Andersen (1990) suggests, such conservative policy regimes, while distinct from social democratic corporatist ones, are also likely to lead to greater social welfare effort than liberal, market-oriented regimes.

Lijphart's *Democracies* (1984) impressively maps variations in formal political institutions across stable capitalist democracies. Lijphart organizes his survey of democratic institutions around majoritarian and consensus modes of democratic politics. His term *majoritarian* refers primarily to government by "minimal winning coalitions" and to institutions tending to support such government; that is, it refers to the rule of slim majorities and to the kinds of complementary institutions best illustrated in the United Kingdom and New Zealand: one-party and bare majority cabinets, fusion of governmental branches and jurisdictional centralization, unicameralism, two-party systems, single-cleavage patterns of conflict, and flexible (even unwritten) constitutions (chap. 1). Consensus government refers to rule by more diverse, contingently negotiated coalitions and to governments much more constrained by consensus than are majoritarian ones. More specifically, it refers to Swiss-like, antimajoritarian factors that require or encourage the sharing of power between the majority and the minority (grand coalitions), the dispersal of power (among executive, two legislative chambers, and minority parties), a fair distribution of power (proportional representation), the delegation of

power (to territorially or nonterritorially organized groups), and a formal limit on power by means of the minority vote (p. 30).

Operationally, Lijphart's effort culminates in a two-dimensional factor-analytical model of the majoritarian/consensus character of political institutions. The first factor connotes a majoritarian *minimal winning coalition* dimension that emphasizes the fewness of "effective parties" and issue cleavages, disproportional representation, and executive and minimal majority dominance of government (1984, 214). The second dimension—most closely associated with unicameralism, revenue centralization, and constitutional flexibility—is singled out by Lijphart as *federalism* (p. 215). However, care is needed here: as the scale scores federal nations low, a unitary characterization is more appropriate. We shall refer to this second dimension as Lijphart's majoritarian unitary dimension.

The welfare implications of the majoritarian/consensus dimension for welfare effort are indeterminate. On the one hand, majoritarian extremes may notably facilitate partisan enactment of distinctive policy goals (see Jackman 1986). On the other, their characteristically two-party forms promote "Downsian" platform convergence (Lijphart 1984, chaps. 1–2). Moreover, *consensus* systems may arise in political contexts that legitimize—indeed, foster—redistributive policies (Lijphart and Crepaz 1991; Masters and Robertson 1988; Powell 1982, chap. 9). Rather than offer specific hypotheses now, we shall return to this issue after our empirical assessment of dimensions of democratic political institutions.

For statist students of the welfare state, *institutions* connotes "democratic institutions," as well as policy structures, precedents, and experiences that enhance nations' policymaking capabilities—particularly those that foster effective policy ideas, cohesive legislation, and effective implementation (e.g., Skocpol 1985; Weir, Orloff, and Skocpol 1989). Reduction of all such capacities to a few measurable dimensions is infeasible, yet some dimensions stand out. One is state *centralization* in the sense of jurisdictional and administrative consolidation of policymaking and implementing authority (Orloff and Skocpol 1984). A second such dimension is *bureaucratization* itself, although the critical threshold of bureaucratization relevant to welfare effort may have been crossed in our 18 democracies by the 1960s (Weir, Orloff, and Skocpol 1989). An additional factor entails a nation's *policy precedents* and experiences. For example, a nation with a broader scope and a longer experience of major social programs will tend to have stronger grounds (precedents, advocates, etc.) for extending welfare policy (Wilensky 1975; Skocpol 1985).

Fortunately, state centralization, bureaucratization, and program precedents are all measurable (Pampel and Williamson 1989). We now analyze measures of these and other institutional variables in an effort to map some principal dimensions of cross-national variability in political institutions empirically. In order to identify some principal dimensions of political institutions, we factor-analyzed a number of temporally invariant institutional variables for the 18 continuously democratic industrialized democracies noted in Table 2. First, we sought to identify and index a summary dimension or two of state policymaking and administrative capabilities by tapping measures of state centralization, bureaucratization, and welfare program experience and Lijphart's majoritarian/consensus dimensions. We also attempted to identify a dimension of long-term working-class mobilization by means of measures of union density and cohesion, the historical incidence of left party government, and the extent of working-class electoral mobilization. Finally, we sought evidence about the clustering of traditionalist policy legacies with other facets of state institutions. (Our index of legacies combines scales of *legacies of absolutism* and *resistance to democracy* adapted from Esping-Andersen 1990, and a scale of *class rigidity* adapted from Lipset 1983).[5]

These measures of political institutions were analyzed by means of a principal components factor analysis of the 18 nations. After orthogonal rotation, this yielded the (structure) matrix of factor loadings displayed in Table 2. Of three sharply defined and distinguishable dimensions, one is a readily interpretable *left corporatism* dimension with strong loadings for the four social democratic corporatist items and loosely negligible loadings for all other items. A second *state centralization* dimension emerged with strong loadings for all administrative items except bureaucratization (.189) and, arguably, the *unitary/federal* dummy (.570). Interestingly, this latter item loads notably (.486) on the third, *bureaucratic traditionalism* dimension, which has strong loadings for traditionalist legacies (.802) and bureaucratization (.829). As Table 2 reveals, this coalescence of bureaucratization with traditionalism typifies non-Protestant nations (Austria, Belgium, France, Ireland, Italy, and Japan) known for their highly permeable, clientelistic administrations and is epitomized by the nationally centralized, but locally quasi-religious, quasi-private administrative apparatus of the Netherlands (Lijphart 1977). It reaches its nadir with the United Kingdom and its settler colonies. Only Denmark—a late monarchy, but Protestant and not renowned for its clientelism—mars the pattern.

Overall, we expect nations that rank relatively highly on each of these dimensions to have relatively high levels of welfare effort. We expect "left corporatism to affect welfare effort positively because it denotes the facets of working-class political economic mobilization and incorporation most likely to intensify pressures for more welfare effort. We expect state centralization to augment welfare spending because centralized/unitary government reduces veto points for the obstruction of prowelfare reforms and streamlines political instruments for the enactment of such reforms (see Cameron 1978). On the assumption that national political organizations can exert more prowelfare pressure on national policy than dispersed actors in subnational arenas, these aspects of centralization should augment welfare effort. Moreover, the

TABLE 2

Dimensions of Political Institutions: Orthogonally Rotated Principal Components circa. 1960

ITEMS	FACTOR 1: "LEFT CORPORATISM"	FACTOR 2: "STATE CENTRALIZATION"	FACTOR 3: "BUREAUCRATIC PATRIMONIALISM"
Variables			
Union strength	.897	.005	.200
Union centralizaton	.819	.041	.350
Class mobilization	.765	.318	−.003
Left rule, 1946–60	.906	.342	−.117
Revenue centralization	−.065	.880	.185
Unitary/federal	.299	.570	.486
State centralization	.329	.828	−.031
Early program consolidation	.144	.642	.590
Majoritarian—unitary	.224	.759	−.422
Majoritarian—minimal winning coalition	.270	.085	−.484
Welfare bureaucratization	.222	.189	.829
Traditionalist legacies	.141	−.018	.802
Nations			
Australia	−.335	.027	−1.259
Austria	.994	−.802	.855
Belgium	.581	.515	.582
Canada	−.731	−1.191	−1.326
Denmark	.885	.139	.955
Finland	1.088	−.274	−.234
France	−.293	.808	.414
West Germany	−.566	−1.176	.687
Ireland	−1.338	1.306	.599
Italy	−1.016	.801	1.142
Japan	−.846	−.608	.589
Netherlands	.030	.474	1.458
New Zealand	−.113	1.236	−2.361
Norway	1.803	−.147	−.136
Sweden	1.983	−.063	−.667
Switzerland	−.591	−1.626	−.038
United Kingdom	−.325	1.908	−.553
United States	−1.210	−1.328	−.706

Note: Three factors, specifically three orthogonal, varimax-rotated principal components, specify the dimensions. The factors are described by two matrices, one of variable loadings per nation, a second of factor scores per variable (or factor item). See Appendix on measurement of items in these factors.

relatively long-standing consolidation of major programs associated with centralization connotes statutory and administrative bases for the expansion of social welfare budgets. Finally, we expect bureaucratic traditionalism to affect welfare spending positively because it denotes tendencies toward noblesse oblige, patronage politics, and high-status bureaucrats (with specialized pension programs and the like). As noted, these tendencies should be more conducive to welfare spending than free market liberalism (imagine Italy versus Canada), yet they might congenially complement socialist politics (as in postwar Austria).

Welfare Needs, Economic Capacity, Economic Structure

With regard to environmental influences on welfare spending, each of our perspectives acknowledges the welfare impacts of economic needs, resources, and structures. Because the operation of these forces, although political, falls short of the manifestly political ones that we are highlighting in this paper, we treat them as supplementary explanatory factors and statistical controls.

First, rates of old age, unemployment, and other forms of need (or "dependence") that denote program eligibility are typically regarded as major sources of automatic (and occasionally discretionary) variations in social spending. In addition, studies indicate that inflation, by pressuring the legislation of upward adjustments of benefit levels, triggering in-place cost-of-living allowances, and augmenting revenue availability via bracket creep (all in excess of increases in real income), may augment welfare effort (e.g., Pampel and Williamson 1989; see also our earlier work, Hicks, Swank, and Ambuhl 1989). Moreover, numerous authors have hypothesized that economic development devolves new welfare functions on to the nation–state while concurrently gen-

erating the collective attitudes (altruism) and resources (tax bases) that facilitate "welfare expansion" (Wilensky 1975). Thus, we expect that the relative size of the aged population, the unemployment rate, price levels, and economic development will all increase social welfare spending.

Students of the welfare state often underscore the welfare impacts of international economic structure, particularly openness to the international economy. Such openness is a source of direct pressure on welfare effort, as well as of neocorporatist arrangements that promote it (Cameron 1978; Katzenstein 1985). The mechanisms grounding direct effects of openness consist of pressures for income security outlays (e.g., social wages and subsidies) that buffer individuals and firms from international volatility. Accordingly, we expect that the greater economic openness, the higher welfare spending.

Finally, the 1973 OPEC oil shock is widely regarded as a watershed between two macroeconomic eras (Hicks, Swank, and Ambuhl 1989; Swank 1988). The post-1973 unraveling of the Keynesian reconciliation of interventionist government and economic growth, as well as the post-1973 ascendance of antistatist movements and governments, arguably restrained welfare spending (Hicks, Swank, and Ambuhl 1989). However, stagflation, by extending the duration and experience of unemployment (and underemployment) increased the need and demand for welfare effort beyond what can be captured by our standard measure of the unemployment rate (Roubini and Sachs 1989). Hence, we control for a possible post-1973 shift in welfare effort without predicting its direction.

EMPIRICAL MEASURES, METHODS, AND ANALYSES

Measurement and Operationalization

The present work analyzes welfare effort in the 18 largest, most affluent and continuously democratic post–World War II nations (see Table 2). It does so for 1960–82, years for which complete and comparable data are available. Operationalized variables are detailed in the Appendix. However, a few immediate comments may help to clarify our measures.

The welfare effort ratio of welfare spending to gross domestic product is computed with the International Labor Organization (ILO) expenditure data and gross domestic product resource data. The gross domestic product's lesser vulnerability to distorting trade and exchange rate fluctuations makes it preferrable to gross national product data. Total expenditure data on social welfare spending from the ILO's *Cost of Social Security* improve on the comprehensiveness and comparability of analogous Organization for Economic Cooperation and Development (OECD) data by including health spending and some in-kind benefits. Welfare spending is defined here, (as by the ILO) as governmental spending relating to schemes, transfers, and services that (1) grant curative or preventive medical care, maintain income in case of involuntary diminuation of earnings, or grant supplementary income to persons with family responsibilities; (2) are legislatively sanctioned; and (3) are publicly or quasi-publicly administered (see ILO 1988, 1). A focus on total spending is chosen, first, so that we might maintain continuity with a large literature stressing such aggregates (see Castles 1982; Hicks, Swank, and Ambuhl 1989; Pampel and Williamson 1989; Swank 1988). A summary focus is used, second, because top-down theorists stress the causal priority of large aggregates over their components—which are decided on as shares of more-encompassing aggregates (Kamlet and Fischer 1984). Also, expenditures on subcategories of social welfare policy may function as alternate, functionally equivalent means to collective welfare. Finally, the analysis of aggregate spending is parsimonious, as well as important, while that of specific programs, although imperative, must lie beyond the present, already complex, effort.

To avoid simultaneity bias and curtail "specification searching" for optimal lags, explanatory variables were generally lagged one year except where theory or substance demanded otherwise. One occurred for age, which was measured so as to adjust empirically to cross-national differences in early retirement and pensioning practices and thus to minimize any underestimation of "automatic" age effects.[6] A second exception, the unemployment rate, is specified as a two-year moving average of unemployment at t and $t - 1$ because unemployment clearly has prompt "automatic" effects via unemployment compensation spending yet may elicit more discretionary policymaker responses after a year or so. Election periods, which may be targets for enhanced program benefits at t, as well as for economic stimulation by means of spending increases a year before, are for t and $t + 1$.[7]

The party strength measures reflect the potentially ample range in governments' lags in responding to environmental stimuli.[8] To take account of this, both governmental and oppositional party strength measures were cumulated over the range of years that proved consequential in exploratory distributed lag analyses of effects of partisan measures on welfare effort for years $t - 6$ through t.[9] Specifically, party strength measures were cumulated (i.e., averaged) for lags of one through four years ($t - 1$ through $t - 4$).

Turning to yearly operational definitions of party strength, parties were designated as left, center, or right using Castles and Mair's (1984) typology. More originally, strength itself was measured in terms of (1) party leadership of government and (2) party electoral and legislative strength in opposition (see Appendix). For each type of party, government leadership was measured as the proportion of years $t - 4$ through $t - 1$ during which a party led government in the sense of having a decisive majority of its cabinet positions. A type of party was judged to lead a government if it had at least 120% of the number of

cabinet portfolios of each other type; the present criterion yields a reasonable 26 (out of 414) nation-years of "indeterminate" government.[10]

With governmental leadership measured, we can turn to measuring governmental opposition and junior partnership. This combination of party statuses is measured as the four-year average (years $t - 1$ through $t - 4$) of two annual measures of the strength of parties *not* leading governments: the year's average of the party type's proportion of legislative seats and proportion of the popular vote in the last major election when the given party type did not govern.[11] Our measurement of opposition allows for effects of nongoverning parties (i.e., ones not leading governments) upon *effects* of governing ones to be gauged directly by means of the addition of a set of opposition–government product, or interaction, terms. For example, we operationalize classical "left contagion" as the interaction of left opposition and right governmental leadership by means of a term defined as the product of these two variables (i.e., left opposition × right governmental leadership, or *left–right*). The slope estimate for this term is interpretable as the effect of left party oppositional strength upon the (Left-contingent) effects of right party government upon welfare effort. Here, symmetry and thoroughness mandate three pairs of opposition–government interactions implying that left government effects on social welfare effort vary inversely with the strength of center and right oppositions, that right government effects vary directly with (i.e., are pressured in a positive direction by) the strength of center and left oppositions, and that center government effects vary directly with left opposition and inversely with right opposition.

Note that one of the four government leadership measures (left, center, right, or indeterminate) must be excluded from our regressions. More specifically, because government leadership measures are averaged from a mutually inclusive and exhaustive set of categories (i.e., left, center, right, indeterminate) whose scores sum to a constant 1.0 (across categories for each nation-year), estimates of partisan government effects are contrasts from a benchmark category. Moreover, in order to avert perfect multicollinearity among regressors, estimation of these contrasts requires that we omit one category of government leadership from our equations. We select the indeterminate category of government leadership as our benchmark and omitted category for two reasons. First, we expect left and center government effects on welfare effort similarly to exceed right and indeterminate ones, so that either the right or indeterminate category might serve. Second, the indeterminate category, unlike the right one, does not figure in our interaction hypotheses and, as a result, does not require specification among our regressors. Thus, omission of the indeterminate category is preferred to omission of the right one.[12]

Note that all three measures of opposition strength may be included together in our regressions. These measures are not constrained to sum to any particular total. Thus, values of one category are not fixed by values of the other two: degrees of freedom are sufficient for estimation of all three.

Specification

The general specification of our baseline model (or noninteractive) model, including signs for hypotheses, is as follows:

$$\begin{aligned}\text{Welfare effort} = a &+ B(1)\text{turnout} + B(2)\text{competition} \\ &+ B(3)\text{state employee} + B(4)\text{election period} \\ &+ B(5)\text{left gov't} + B(6)\text{center gov't} \\ &+/- B(7)\text{right gov't} + B(8)\text{ left opposition} \\ &+ B(9)\text{center opposition} \\ &+/- B(10)\text{ right opposition} + B(11)\text{left corporatism} \\ &+ B(12)\text{state centralization} \\ &+ B(13)\text{bureaucratic traditionalism} \\ &+ B(14)\text{Ln gross domestic product} \\ &+ B(15)\text{price levels} + B(16)\text{unemployment rate} \\ &+ B(17)\text{aged} + B(18)\text{openness} \\ &+/- B(19)\text{post-OPEC} \qquad (1)\end{aligned}$$

We have no favored hypotheses for right governments or oppositions.

Our opposition–government interaction model is more elaborate. To Equation 1 it adds product or interaction terms for pairs of interacting variables. Interpretations of estimates for them, as well as for variables multiplied to construct them, are complicated. In particular, estimates that come attached to government and government–opposition terms now serve to describe equations expressing *variable* welfare effects of partisan government strength as functions of oppositional strength. Most crucially, estimates for interaction terms are now slopes linking variable effects of governments led by particular parties in these functions to levels of oppositional strength for particular parties. Government estimates are now intercepts in these functions, *not* the so-called "main" effect that might be suggested by an analogy to interactions in analysis of variance tables.[13]

Estimation

The pooling of time series ($T = 23$ years) and cross-sections ($N = 18$ nations) permits large-sample analyses that draw on temporal and cross-national variation. However, equation errors will typically be heteroscedastic and autocorrelated within units, as well as autocorrelated across units, thereby degrading estimator precision (Kmenta 1988, chap. 12; Stimson 1985). To correct for these maladies, analyses are

performed using Park's generalized least squares model with corrections for (AR1) autoregressive, heteroscedastic, and contemporaneously/cross-sectionally correlated errors (see Kmenta 1988). Intercepts may also differ across units, yielding biased estimates. However, addition of the three political-institutional dimensions to this model should amply adjust for strictly cross-national differences in welfare effort, since these dimensions alone explain 60% of such variance. Finally, with regard to R-squareds, those in the tables are Buse R-squareds, which approximate the real R-squared conception of squared correlations between observed and predicted values of untransformed *Y*-values (White 1988). The Durbin–Watson *d*s are adjusted for gaps in our data—in this case for transitions between successive, "stacked" national times series.

Analytically, we first estimate a basic additive model and then estimate one that includes opposition–government interactions.[14] As a check on robustness, we next apply Tukey's (1958) "jackknife" to a key equation. The jackknife procedure tabulates destabilizations of estimates by excessively influential cases and provides alternative coefficient estimates that are robust in the face of such cases. To highlight robust findings, we reestimate the key equation after deletion of variables revealed unstable by jackknifing.

FINDINGS

Results support expectations, strongly rejecting most null hypotheses. Equations fit the data well, explaining over 90% of the variance in welfare effort; and they yield satisfactory Durbin–Watson *d*s (around 1.90).

In the additive equation of Table 3, column 1, nearly all hypotheses are supported at the .05 test level. Exceptions are provided by the unexpectedly negative (but insignificant) estimates for left opposition, state employees, and elections periods. Among variables for which we posed no hypotheses, the estimate for Right-led governments is close to zero and thus consistent with our discussion of government effects. The OPEC estimate is consistent with Roubini and Sachs' (1989) view that post-OPEC conditions exacerbated citizen wants. The right opposition estimate is positive.

With respect to the relative strength of effects, standardized slope estimates (betas) help clarify the relative magnitudes of effects of additive coefficients within an equation. This is especially the case in equations uncomplicated by interactions (Cohen and Cohen 1975). Thus, a ranking of the column 1 variables in terms of these betas may be instructive: aged population (.262), center government leadership (.237), state centralization and left corporatism (.238 and .221, respectively), price levels (.192), turnout (.131), state traditionalism and center party opposition (both .114), post-OPEC (.105), and left government (.104). The finding that the welfare impact of center governments exceeds that of left governments poses the one notable surprise. However, this ordering is consistent with findings and arguments in several studies (e.g., Wilensky 1981). Christian democratic and progressive liberal commitment to social amelioration (without a commensurate commitment to high-employment policies) goes far toward explaining this center government finding.

Turning to opposition–government interactions, we find five notable ones, four of them statistically significant interactions (see Table 3, col. 2). Specifically, we find negative *embourgeoisment* effects on left governments exerted by right, as well as center, oppositional strength. As center or right oppositional strength rises, left government expansion of welfare effort ebbs. In addition, we find positive "contagion from the Left" effects upon Center-, as well as Right-led governments, and marginally significant prowelfare pressures upon Right-led governments from center oppositions. With this strong evidence of the opposition–government interactions (which cause opposition estimates to denote effects of oppositions on indistinctly led, or "nonpartisan," governments) the left opposition estimate turns, somewhat anomalously, significantly *negative* while right, as well as center, opposition estimates remain significantly positive. However, the pattern is plausible: net of their impacts upon governments with distinct partisan leaderships and, in particular, governments of indeterminate leadership, left and right oppositions appear to make a "run for the middle" (Center): left parties retrench, while right ones reform (see Quinn and Shapiro 1991, on such "runs").[15]

Nonparty findings for interactive equations remain much like those for the additive equation (compare Table 3, cols. 1, 2). State employee and election period variables are insignificantly negative in both equations, calling into question the roles of reelectoral motivation and bureau voting heavily emphasized in some statist accounts of welfare state expansion. Otherwise, results are much as theory and the column 1 findings suggest. Voter turnout, electoral competition, and our dimensions of political institutions bolster welfare effort, as do affluence, price levels, unemployment, aged populations, and economic openess. Further discussion of findings, especially of the rather complex partisan ones, is in order. However, before proceeding, some diagnosis of estimate stability is wise.

First, we use the jackknife procedure to gauge stability across important subsets of cases (i.e., nations). A second procedure involves a reestimation of Table 3, column 2 after deletion of variables revealed unstable by jackknifing (Miller 1974; Tukey 1958). Consideration of estimate robustness, although tedious, will help us boil findings down to a relatively robust set.

Diagnoses

The jackknife reestimations entail (1) new estimates of slopes and t-statistics for each regressor in each of the 18 possible 17-nation subsets of the original

TABLE 3

Multiple Regression (GLS) of Welfare Effort on Hypothesized Determinants

VARIABLE (NAME)	WELFARE EFFORT WITHOUT PARTY INTERACTIONS (1) EST. COEF.	(1) STAND. COEF.[a]	WEFARE EFFORT WITH PARTY INTERACTIONS EST. COEF. (2)	JACKKNIFE (3)	POSTJACKKNIFE WITH PARTY INTERACTIONS ("SHORT EQUATION") (4)
Turnout	.0765* (.008)	.131	.0717* (.008)	0 6.977	.0567* (.007)
Electoral competition	1.4101* (.825)	.014	1.4590* (.833)	1 −5.841	—
State employees	−.0256 (−.018)	−.010	−.0192 (−.018)	7 −4.090	—
Election period	−.0001 (−.000)	−.001	−.0003 (−.000)	11 2.472	—
Left government	.0154* (.006)	.104	.0301* (.008)	1 4.400	.0038 (.007)
Center government	.0324* (.004)	.237	.0080 (.007)	9 .022	.0300* (.007)
Right government	.0264 (.119)	.063	−.0055 (−.008)	11 −3.919	.0135* (.006)
Left opposition	−.0016 (−.012)	−.004	−.0591* (−.018)	0 −1.585	−.0657* (−.016)
Center opposition	.0475* (.009)	.114	.0523* (.012)	0 8.723	.0706* (.012)
Right opposition	.0335* (.013)	.082	.0356* (.014)	2 3.380	−.0303* (−.012)
Left corporatism	.0132* (.003)	.221	.0138* (.003)	0 .243	.0176* (.003)
State centralization	.0142* (.002)	.238	.0133* (.002)	0 3.630	.0182* (.002)
Bureaucratic traditionalism	.0068* (.002)	.114	.0076* (.002)	0 −3.815	.0095* (.002)
Center–Left	—	—	−.0900* (−.020)	1 −5.855	−.0537* (−.012)
Right–Left	—	—	−.0585* (−.018)	3 −8.836	—
Left–Center	—	—	.0634* (.015)	1 8.044	.0294* (.009)
Left–Right	—	—	.0934* (.017)	0 79.616	.0492* (.009)
Center–Right	—	—	.0151 (.011)	4 2.191	—
lnGDP	.0118* (.003)	.062	.0130* (.003)	0 −2.401	.0103* (.003)
Price level	.0004* (.000)	.192	.0004* (.000)	0 1.001	.0005* (.000)
Unemployment rate	.1091* (.027)	.046	.1183* (.029)	0 6.234	—
Aged share of population	.6643* (.058)	.262	.6907* (.058)	0 10.611	.7159* (.061)

TABLE 3

Multiple Regression (GLS) of Welfare Effort on Hypothesized Determinants *continued*

VARIABLE (NAME)	WELFARE EFFORT WITHOUT PARTY INTERACTIONS (1) EST. COEF.	STAND. COEF.[a]	WEFARE EFFORT WITH PARTY INTERACTIONS EST. COEF. (2)	JACKKNIFE (3)	POSTJACKKNIFE WITH PARTY INTERACTIONS ("SHORT EQUATION") (4)
Openness	.0206* (.004)	.090	.0205* (.004)	0 8.790	—
Post-OPEC	.0127* (.001)	.105	.0137* (.001)	0 6.551	.0153* (.000)
Constant	−.0690* (−.014)	.000	−.0049* (−.001)	0 −1.517	−.0232* (−.011)
Degrees of freedom	394		389		394
R^{2c}	.908		.923		.912
Adj. R^2	.904		.921		.908
DW d	1.884		1.890		1.921

[a]Standardized coefficients, which vary largely between −1.0 and +1.0.
[b]Jackknife information consists, first, of the number of times that slope estimates for a variable fail to yield absolute t-values of 1.0 or more. It secondly includes (below these counts in parentheses) *signed* t-values computed by means of jackknife procedure (Tukey 1958, 135–48). All jackknifing is done here with nations as observations.
[c]R-squareds are Buse estimates of "real" R-squared (see Kmenta 1986, 616–85); Adjusted R-squareds are computed from these; and Durbin–Watson d is computed on corrected residuals and adjusted for "gaps" (or international variation) in stacked time series.
*$p \leq .05$, one-tailed test.

sample and (2) unique new jackknifed estimates of slopes and t-statistics for each variable for the complete population of cases (computed from the new estimates described in point 1).[16] We display information of type 1 in terms of counts of the number of times that t-statistics for given variables fall below 1.0 in absolute value. We display information of type 2 in terms of signed t-statistics for regressors, for these illuminate both the direction and statistical significance (and robustness) of coefficients. (See Table 3, col. 3 for jackknife results.)

The jackknife procedure suggests that several variables may have cross-nationally unstable estimates. First, electoral competition has an anomalously negative t-value of −5.841, while state employee and election period variables have numerous t-values less than 1.0 in absolute value. In addition, bureaucratic traditionalism and economic product both have anomalously negative jackknifed t-statistics (despite 18 correctly signed ones in each of the 18 subpopulations).[17] Left and center opposition terms and the Left–Right product terms show slight signs of instability (one or two "small" t-values each). The center and right government, right opposition and the Right–Left and Center–Right interactions show more troublesome signs of instability; that is, they display several "small" t-values and/or anomalously signed jackknifed statistics.[18]

Table 3, column 2 probably represents the most informative of our equations.[19] Nonetheless, examination of an equation from which cross-nationally unstable regressors have been removed has the possible advantage of deleting arguably extraneous variables and the certain advantage of highlighting ones on which evidence is robust. To identify such an equation, jackknife computations (some reported in col. 3) were used to specify the cross-nationally robust equation of column 4.[20]

This equation retains four of the previously six "environmental" variables: affluence, prices, aged population, and post-OPEC (but not unemployment or openness). It bears 13 of our 18 manifestly political variables: voter turnout, the three political-institutional dimensions, all six governmental and oppositional measures of party strength and three opposition–government interactions [Center–Left, Left–Center and Left–Right]). By omitting variables that are conventionally "relevant" to the prediction of welfare effort (despite their reliance on particular cases for relevance), the "short" equation does court specification bias. Yet, the short equation has heuristic value for highlighting the variables that withstood jackknife diagnoses even though it does not supplant the column 2 equation as our best-specified, least-biased, best-fitting, and, indeed, most informative equation. Although we must discount the estimates that do not make it past the jackknifing procedure into the column 4 equation, the column 2 equation has the best estimates.

Final patterns

Two forms of "contagion from the Left" are evident across all equations. In one, the strength of left parties excluded from governmental leadership pressures Right-led governments to augment welfare

effort (see Figure 1, upper left-hand quadrant). Right government effects on welfare effort are negative (−.0055) in the absence of all left opposition (e.g., in the United States) in the preferred equation of column 2. However, they become positive with increases in left opposition (see Figure 1, upper left). Principal examples of strong prowelfare effects of right governments facing strong left oppositions occur for Australia and New Zealand in the 1960s (when left oppositional strength figures at times approached 49%) and Japan during late 1960s and 1970s when left oppositional strength peaked. In the second form of "contagion from the Left," left opposition effects on center governments' welfare effort roughly parallel those on right government welfare effort (see Figure 1, upper right). Principal examples of upward boosts in center welfare effort under the influence of strong left oppositions are Finland in the 1960s and, more fleetingly, Norway in 1970–72 and Sweden in 1981–82. (The most salient *lapses* of center party welfare effort where left oppositions are absent occur in the United States during the Carter and (late) Johnson administrations.)

Two forms of left *embourgeoisment* effects are present. Moderations of the prowelfare effects of Left-led governments by centrist oppositions and junior partners are present in all equations. In the equation of Table 3, column 2, left government effects, strongly positive (.0301) when center oppositions are null, decline quickly with increases in the strength of center oppositions, approaching zero as center "oppositions" reach high shares of seats and votes (i.e., 45%; see Figure 1, lower left-hand quadrant). The greatest moderations (and possible reversals) of left welfare effort under centrist pressures occur in Austria and Sweden. The second form of left *embourgeoisment* effects reinforces this picture of substantial, but readily compromised, left government support for welfare effort. As the lower right quadrant of Figure 1 indicates, our findings include a Right–Left interaction indicating that prowelfare effects of left governments are strong at the outset but decline toward zero (and beyond) as right "opposition" shares of votes and seats approach 45%. The greatest such attenuations of left welfare effort arise for the German Social Democratic party governments of the 1970s and Danish left governments of the early 1960s.

Overall, strong evidence emerges for strategic interactions among parties. In these interactions, dominant governing parties accommodate their policymaking orientations to those of parties in oppositional—and subordinate governmental—roles. Some interesting surprises emerge from these interactions. Left parties appear to submit to strong antiwelfare pressures from *all* oppositions. Despite typically clear left rule effects on welfare effort and favorable centrist orientations toward welfare effort when in office, center oppositions appear to dampen the welfare effort of left parties. Perhaps this occurs because the strong, longer-run welfare-spending legacies captured by our left corporatism variables can be effectively evoked by the center to claim middle-of-the-road swing voters. On the other hand, centrist welfare orientations are reinforced, rather than undercut, by left oppositional pressures.

Returning to nonpartisan variables, evidence for positive voter turnout effects is pervasive and robust. Evidence for prowelfare effects of political institutions is also strong. Significant positive estimates for social democratic corporatism, state administrative/political capabilities, and traditional policy legacies is everywhere strong and robust. Finally, evidence for the roles of socioeconomic forces is strong: affluence, the sizes of aged and unemployed populations, inflation, openness, and the increased material needs of the first post-OPEC decade all exert upward pressure on welfare effort.

CONCLUSION

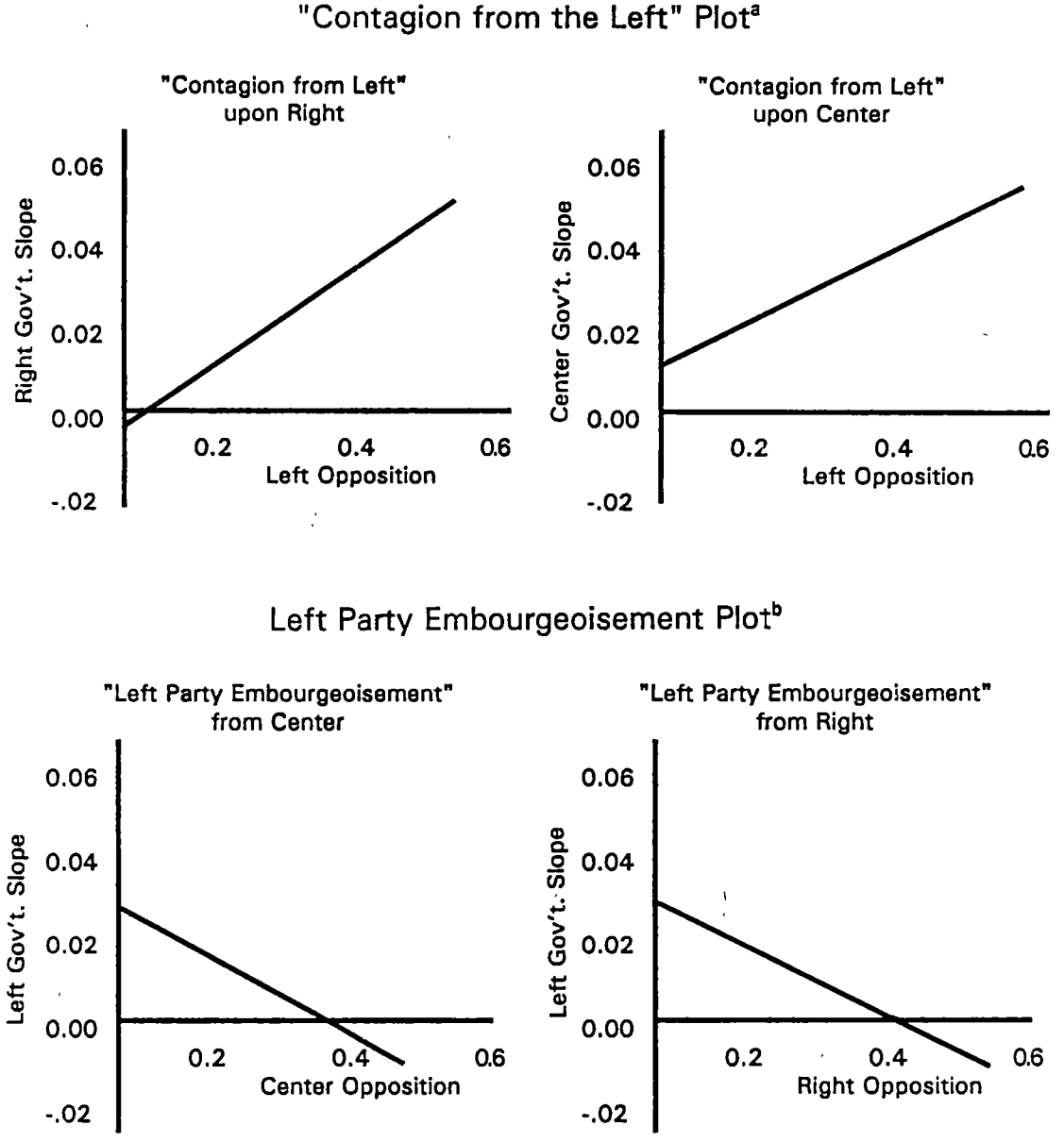
Both electoral turnout and differences in the strengths and ideologies of parties matter for the share of national incomes spent by democracies on social welfare programs. Moreover, not only does the strength of governing parties matter, but the strength of oppositional parties and parties at the margins of ruling coalitions plays an important role in determining welfare effort. Dimensions of political institutions that include traditions of left party electoral success and working-class mobilization and interest representation, state administrative and political capacities for cohesive policymaking, and heavily bureaucratic and traditionally hierarchical state institutions also matter, along with a host of socioeconomic environmental conditions. However, neither the preelectoral budgetary machinations of incumbent politicians nor the bureau voting of public sector interest groups (each a focal activity in public choice theory) seems to matter for welfare effort. Similarly, party electoral competition, a centerpiece of much "democratic pluralist" theorizing, does not seem to matter for welfare effort.[21] These conclusions appear to hold, at least for the 18 largest affluent democracies between 1960 and 1982.

Consistent with recent claims by Pampel and Williamson (1989), voter turnout appears to have exerted an upward pressure on welfare effort. Even net of ample controls for political and environmental factors, the political democratic stress on turnout as a source of demands for social ammelioration is vindicated.

Consistent with partisan theory stressing center party delivery of income security outlays, our simple additive models indicate that Center-led governments on average generate higher levels of welfare effort than do other governments, left ones included (Wilensky 1981). However, consistent with the social democratic authors who acknowledge centrist as well as leftist advocacy of income security spending, our additive findings encompass clear left party effects on welfare effort (see Castles 1982; Stephens 1979; see also our earlier work, Hicks and Swank

FIGURE 1

Selected Conditional Slope Estimates for Types of Governments Plotted Against Contingencies



Note: [a]Estimates of opposition-contingent effects of partisan governments when oppositional conditions for them equal .0 (e.g., of right government effects when left opposition = 0) are the slope estimates attached to the relevant partisan government term (e.g., right government) in the estimation equation. Estimates for slopes of lines in our plots (e.g., for effects of right government *by* left opposition) are the slope estimates for the product terms (like Left–Right) for the pair. Further elaboration of interactions are available on request; see also Friedrich 1982; Lange and Garrett 1985.
[b]Partial plots of left governments' effects conditional on center opposition controlling for right opposition (on left of panel) and of left effects conditional on right opposition controlling for center opposition (on right).

1984). The finding that center effects not only complement left ones but actually exceed them should be no great surprise. Lack of consistent center (as opposed to left) full-employment policies has made centrist governments dependent upon income transfer programs for their pursuit of income security (Alvarez, Garrett, and Lange 1991; Janoski 1990). Furthermore, centrist welfare effort appears to be intensified by left oppositions and impervious to right ones, while left welfare effort appears to be dampened by center, as well as right, oppositions. In short, oppositional dynamics appear to buoy center party welfare effort but to depress left party welfare effort. Moreover, inclusion of left corporatism, a broad-ranging measure of left political strength, into our models slightly moderates estimates for short-run effects of left governments. Whether this moderation should be regarded as suppressing or sharpening the short-term effect of left government is unclear. What is clear is that our present introduction of left

corporatism extends—and thus augments, rather than limits—overall evidence for the policy relevance of social democracy.[22]

In interactive models, Left-led governments appear to affect welfare effort in inverse proportion to the strength of center and, perhaps, right oppositions, augmenting it when and where these oppositions are relatively weak and reducing it insofar as they are strong. Patterns of left government accommodation to oppositional strength pervade these analyses. (Whether left governments compensate victims of welfare retrenchment with alternative benefits we cannot say.) Complementing the patterns of left accommodation to center and right oppositions are patterns of "contagion from the Left." To the extent that left oppositions are electorally and legislatively strong, they pressure parties to their *right* to enhance welfare effort. As left opposition strengthens, otherwise weak or negative social spending impacts of right governments are lifted to substantially positive levels. Notable center party contributions to welfare effort are substantially multiplied when the Left is strong. Interestingly, governments without clear left, conservative, or centrist leadership appear to be as supportive of welfare effort as conservative governments. In addition, left and right oppositions appear to affect welfare policy in additional ways. Net of our evidence for oppositional pressures, we find indications that left and right parties also may sometimes woo voters and, when governments lack distinct partisan leadership, court inclusion in ruling coalitions by reversing characteristic welfare stands. Consistent with Quinn and Shapiro's (1991) "run for the middle," left oppositions sometimes facilitate welfare retrenchments, while right parties sometimes foster welfare expansion.

In short, the partisan character of governments, oppositions, and interactions between the two matter for welfare effort. Overall, the importance of left and center governments for the extent of welfare effort revealed by our simpler, additive findings reaffirms earlier findings (Castles 1982; Hicks and Swank 1984; Stephens 1979).[23] With regard to the interaction of governments and oppositions, our findings suggest a mix of party distinctiveness and interparty convergence in the policy enactments of party governments that resembles claims by recent Downsian theorists (e.g., Austen-Smith and Banks 1990). However, interparty convergences in our models of welfare effort are functions of specific differences of ideology and strength between governments and oppositions, such as are not highlighted in Downsian theory. True, our discussion of convergence is formulated in terms of a set of loosely coupled propositions (e.g., "contagion from the Left") that might benefit from, as well as inform, formal theorizing within the Downsian tradition. However, our treatment of these questions implies that extension of the Downsian tradition to the comparative study of policy determination may require some reformulation of conventional Downsian models.

Finally, our analyses suggest that political institutions matter a great deal for welfare effort. Cross-national differences in the three dimensions of political institutions identified here substantially differentiate states' social welfare effort provision. Social democratic corporatism, conceived in terms of the degree to which left parties and unions mobilize and incorporate industrial workers into national policy-making networks, appears, net of short-run partisan politics, to be a strong source of national differences in welfare effort. In addition, structurally centralized states with histories of early consolidation of major social security programs do, indeed, appear to capacitate national welfare effort, as many have suggested (Orloff and Skocpol 1984; Wilensky 1975). Lastly, bureaucratic traditionalism—a combination of a highly bureaucratized welfare administrations plus hierarchical, antidemocratic legacies, that apparently undergirds welfare state clientelism (Esping-Andersen 1990; Rokkan 1970)—enriches our conception of political institutions and further differentiates the welfare efforts of contemporary industrialized democracies.

Appendix:

Y *Welfare Effort.* Total social welfare expenditures as proportions of gross domestic product, from the ILO's *Cost of Social Security.*

X_1 *Turnout.* Valid voters as proportions of persons eligible for voting, from Mackie and Rose (1974); *European Journal of Political Research,* (selected years). Measurement at $t - 1$ in analyses.

X_2 *Party Competition.* Measures of competition among parties computed from data on mass voting for parties, as in Pampel and Williamson 1989, chap. 3 and Swank 1983. Sources are the same as those for X_1 and X_5 through X_{10}. Measured at $t - 1$ in analyses with the formula

$$1 - \sum_{i=1}^{N} (P_i - 1/N),^2$$

where N is number of parties and P_i is proportional vote for party i.

X_3 *State Employees.* All government civilian employees as proportions of population with data from OECD 1982, OECD's *Labor Force Statistics,* and various national statistical yearbooks. Measured at $t - 1$ in analyses.

X_4 *Election Period.* Equal to 1 if a year is an election year (in which presidential or general parliamentary elections occur) *or* a preelection year; otherwise 0. Election times from *Keesings Contemporary Archives* (selected years), and Mackie and Rose 1974; *European Journal of Political Research* (selected years).

X_5 *Left Party Government Leadership.* Four-year average ($t - 4$ through $t - 1$) of left party leadership of government, coded 1 for each year in which left party cabinet participation (portfolios as percentage of total cabinet portfolios) is more than 120% of center party participation and more than 120% of right party participation, otherwise 0.[24] Party classifications generally correspond to Castles and Mair 1984, although some minor divergences exist. On these differences see Swank 1988, 1992. Cabinet participation data come from Browne and Dreijmanis 1982 and *Keesings Contemporary Archives.*

X_6 *Center Party Government Leadership.* Strictly analogous to left party government leadership.

X_7 *Right Party Government Leadership.* Strictly analogous to left party government leadership.

X_8 *Left Opposition.* Electoral and legislative strength of left

parties (as categorized by Castles and Mair 1984) when not leading government and measured as an average of left party proportion of vote in last election and proportion of seats in lower house of legislature as ascertained from Mackie and Rose (1974), *European Journal of Political Research* (selected years). Averaged $t - 4$ through $t - 1$. Equal to 0 when left government leadership equal to 1.

X_9 *Center Opposition.* Strictly analogous to left opposition.

X_{10} *Right Opposition.* Strictly analogous to left opposition.[25]

X_{11} *Left Corporatism (Factor 1).* Factor-score scale measuring strength of long-term working-class mobilization and political economic integration (see Table 2). Union strength is measured using a modification of Cameron's index of union strength (1984, 165). This consists of the product of union density (in 1960) times the average of *three* scales of union centralization: organizational unity of labor, confederation power in central bargaining, and scope of collective bargaining. In order to stress the importance of union centralization and compensate for possible exaggeration of union centralization by Cameron in Denmark and the low countries, union centralization is measured a second time by means of a three-point (one-, two-, or three-level) scale devised by Wallerstein (1989). Left party strength is measured following Stephens's (1979) index of cumulative rule, as the number of years, 1946–60, marked by left party–led government. Left voter mobilization is measured as an index of left voting weighted by voter turnout, specifically as [(LPV + LCV)/2] × TO, where LPV is *left party votes* as a proportion of all votes (1957–60), LCV is Korpi's (1983) index of *class voting* around 1970 (assumed adequately to approximate cross-national variations in 1960 class voting), and TO is the average, proportional voter *turnout* for 1957–60.

X_{12} *State Centralization (Factor 2).* Factor-score scale measuring state centralization, central state prominence, and legacies of welfare program consolidation (see Table 2). In brief, state centralization is measured in three ways: (1) as a unitary/federal (UF) dichotomy (unitary = 1.00, otherwise .00) (see Lijphart 1984, Table 10.2 for categorization); (2) in terms of revenue centralization (RC) as the ratio of central governmental to all governmental revenues extracted in 1960 (see Lijphart 1984 and Pampel and Stryker 1988 for precedents and OECD's *National Accounts of OECD Nations* for data); (3) as a composite index of state centralization weighted by the size of the state centralized. Specifically, it is measured as [(UF + RC)/2] × GE, where GE is the *government employee* share in total employment, UF and RC are as defined, and all three elements of the index vary between .0 and 1.0 (see Table 2 on state centralization). Following Pampel and Stryker 1988, state bureaucratization is measured by stressing administrative costs of welfare spending as proportions of all welfare spending. Welfare program consolidation measures the legacy of a diversified welfare state by the number of years up to 1960 that a nation has had *all* of the following four major welfare programs: (1) social security retirement pensions; (2) unemployment compensation, (3) workman's compensation and (4) sickness and maternity care. Family allowances were dropped because they had still not been adopted by all affluent democracies by 1960. Of course, health programs were not universally adopted at the minimal level recorded by Social Security Administration (1981) data sources until the mid 1960s; but the number of post-1960 "years-to-adoption" is small and makes for minor measurement error. The pluralist concern with formal political institutions and their majoritarian/consensus interpretation is operationalized using the factor scores for Lijphart's majoritarian minimal winning coalition and majoritarian unitary factor-analytical dimensions (1984, chap. 13).

X_{13} *Bureaucratic Traditionalism (Factor 3).* Factor-score scale measuring extent of state traditionalism, bureaucratization, and majoritarian unitary government. State traditionalist legacy is measured as an additive index of three (standardized) items: (1) a five-point scale of the *extent of state absolutism* in the eighteenth and nineteenth centuries (Rokkan 1970, chap. 3, Table 1); (2) a five-point scale of *resistance to mass enfranchisement* (ibid., Table 2); and (3) a three-point scale of the extend of class rigidity (class systems as systems of status division; Lipset 1983). Although a straightforward, unweighted additive index was used, a principal components analysis indicated unidimensionality by yielding the following pattern of loadings: absolutism (.81); obstructed enfranchisement (.84), class rigidity (.80). Bureaucratization is measured as for X_{12}, and majoritarian unitary government as in Table 2 and Lijphart 1984.

X_{14} *Natural Logarithm of Real Gross Domestic Product/Population (lnGDP).* Through 1980, from Summers and Heston 1982. Thereafter computed as extrapolation of 1980 figures from growth rates from OECD 1986. Measured at $t - 1$.

X_{15} *Price Level.* Consumer price index from International Monetary Fund. Measured at $t - 1$ in analyses.

X_{16} *Unemployment Rate.* Unemployed as proportion of economically active population at t and $t - 1$, from OECD's *Main Economic Indicators.*

X_{17} *Aged Share of Population.* Proportion of population over 65 years of age, from United Nations' *Demographic Yearbook.* Measured at year ($t - 5$ through $t - 1$) with highest correlation with welfare effort in "aged" regression model.[26]

X_{18} *Openness.* Imports plus exports as proportions of gross domestic product, from OECD's *Main Economic Indicators.* Measured at $t - 1$ in the analyses.

X_{19} *Post-OPEC.* Equal to 1 for 1975 and later, otherwise 0.

Notes

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1. See Hicks, Swank, and Ambuhl 1989 and Pampel and Williamson 1989 for conflicting views on partisanship; see Garand 1988 and Korpi 1989 for conflicting views on state administrators; see Dye 1979 and Hicks and Swank 1984 for conflicting views on electoral factors.

2. As for Bismarckian "federalism," Taylor debunks it best: "Bismarck's constitution was supposed to be federal, but its federalism was fraudulent, window dressing to make the dictatorship of Prussia more respectable" (1946, 117).

3. Each of these perspectives acknowledges the welfare relevance of social welfare needs, economic capacities for welfare provision, and economic structure. Note that we attempt in our models to specify only *direct* (or "structural") influences upon policy. For example, party-channeled effects of openness suggested by Cameron (1978) are ignored.

4. There is a strong theoretical and empirical basis for ascribing center parties prowelfare policy goals. Baldwin's (1990) study of Scandinavian and British "welfare" politics highlights the middle-class appeal and secular center advocacy of social insurance policies as anodynes for middle-class risk while Stephens (1979) and Wilensky (1981) analyze and document similar consequences of the Christian (democratic) center.

5. For a detailed description of the items used to measure left corporatism, state capabilities, and traditionalist policy legacies, see X_{11}–X_{13} in the Appendix. Although these institutional measures are temporally invariant, they are not exempt from causally legible associations with welfare effort. We confine measurement to circa 1960, where strong temporal invariance can be assumed; and we extend it back earlier to wherever temporal variability or historical depth mandates

(e.g., to 1946 for left rule; to the eighteenth century for absolutism) and wherever data permit.

6. Age was measured for each nation at the year $t - 5$ through $t - 1$ for which the population share over 65 years of age most strongly affected welfare effort. Almon distributed-lag regression of effort upon population share at $t - 5$ through $t - 1$ was used to identify key years (Johnston 1984). Census use of interpolated values for annual population may, alas, offset our effort at precision. Also, economic product was measured as the natural logarithm of per capita gross domestic product because this defines slopes per unit of percentage change in "product," rather than per raw dollar changes in it and such changes are more likely to be linearly related to welfare effort (a proportion) than are changes in raw dollars: a change from $4,000 to $5,000 should matter more than one from $14,000 to $15,000.

7. For OPEC, an arguable fourth exception, years were coded "post-OPEC" starting in 1976 because simultaneous deterioration of income growth, inflation, and unemployment did not occur until 1975, and one-year response lags seemed apt.

8. Our measurement of party strength diverges from common single-year measures (e.g., Swank 1992). It particularly diverges from the "1946-to-present" cumulative measures in Pampel and Williamson (1989), which overreact to the limitations of "immediate" party effects with 35-plus year measures and *equate* the policy influences of recent, with those of decade-old, governments.

9. A regression was run with the regressors of the "basic" model and the Almon distributed lags for $t - 6$ through t for government and opposition items.

10. This seemingly permissive criterion was adopted after a more stringent one (e.g., greater than 50% of all cabinet portfolios) yielded more periods of indeterminate governmental leadership than was plausible. See also n. 24.

11. This measure combines strictly oppositional parties entirely excluded from governmental participation with junior partners in coalition governments, distinguishing for the first time the strengths of clearly governing parties and other parties. Note that for the typical two-house legislature, the lower house was used to focus measures for both votes and seats.

12. We have a further reason for excluding indeterminate governments: it allows for simultaneous tests of interactions for all three major types of parties. Also, interpretive focus, statistical tractability and numerical relevance ground our grouping of oppositional and junior partner parties in the measure of oppositional strength. Separate specifications of oppositional, junior partner, and leadership variables across left, center, and right parties (for nine terms) would exacerbate multicollinearity and complicate interpretation very seriously—prohibitively once interactions (as many as 12) were introduced. Grouping of governmental junior partners with governmental leaders would blur our focus on dominant partisan directions of governments. Finally, the legislative and electoral strength of junior partners relative to strictly oppositional ones is small (on average .02 as opposed to .17 per party type); and the variance explained by partner strength as opposed to strictly oppositional strength in the current oppositional measure is small as well (on average, 10%, as opposed to 90%). Note that ommission of (any) one of the three ostensibly redundant oppositional variables does not notably alter findings.

13. Detailed elaboration of the interaction model is available on request. See also Friedrich 1982.

14. We do so by excluding the measure for the benchmark category from the equations. We enter all six of the possible opposition–government interactions, then drop the one (with the *lowest* t-statistic) that does not have an (absolute) t-value greater than 1.0 (see Johnston 1984, 504–6). Then we repeat this procedure until only interaction terms with (absolute) t-values greater than 1.0 are retained. As a check on estimate stability, we also proceed with a forward, stepwise treatment on the interactions for both equations. This yields identical results. Yet we must caution that multicollinearity levels for these variables measured in terms of R-squared deletes (or "1-tolerance") are never less than .90. Hence, we cannot confidently assert that t-tests have the power for exclusions of opposition–government interactions to be compelling, only that retentions override serious loss of power due to multicollinearity and evince indications that multicollinearity has really taken a bite. As for eventually excluded variables, we cannot be certain that they do not result from multicollinearity.

15. Opposition findings are unaltered by deletion of any single opposition term, despite resulting reductions in multicollinearity (see n. 12, 14).

16. This procedure is executed as follows for the equation of Table 3, column 2. The equation is reestimated with each of its 18 nations removed in one of the 18 reestimations. Reestimations are examined qualitatively for departures from the estimates obtained in the original estimations with all 18 cases. Reestimations are also used to compute new jackknifed values of coefficient estimates (BJ) plus new test statistics for each new jackknifed coefficient. A given jackknifed coefficient estimate BJ equals the mean of the "pseudovalues" B_i*. $B_i* = 18(B) - 17(B_i')$, where B is the 18-case estimate of the coefficient in question and the B_i' are the 18 separate 17-case estimates (and $i = 1, 2, \ldots, 18$). The standard error for each of the BJ estimates is just the standard error of the B_i* that are averaged to define BJ. From the values of the BJ and their standard errors, t-statistics are computed.

17. As pseudovalues equal $B_i* = 18(B) - 17(B_i')$, where B is the 18-case estimate of the coefficient in question and the B_i, are the 18 separate 17-case estimates (and $i = 1, 2, \ldots, 18$), they may be distinctly signed from both the initial sample-wide and subsample estimates where the absolute values of $17(B_i')$ exceed those of $18(B)$. Since the jackknifed slopes (standard errors and t-values) are computed from the pseudovalues, jackknifed estimates may be marked by reversals of signs from full-sample and subsample estimates.

18. However, it merits noting that some of these variables were already insignificant in Table 3, column 2, and that no new instability is revealed for these. Moreover, although statistically insignificant estimates may be viewed skeptically, *significant* interactions are dubiously interpreted in the absence of additive estimates for their component parts (Friedrich 1982). These estimates anchor conditional effects of partisan governments to intercepts, as well as to levels of oppositions. For example, where left government and right oppositions interact, slope estimates for the left government term anchor the variation of left government effects (across levels of right opposition) by specifying these effects when right opposition is .0.

19. A variant of the column 2 equation that substituted the OECD measure of social welfare transfer expenditures for the ILO measure of social security spending (including in-kind benefits and services) yielded few new indications of instability. A dummy variable–generalized least squares variant was also run. This controls for 0–1 dummy variable regressors for 17 of our 18 nations, leaving strictly longitudinal, within-nation variance for other regressors to explain. It provides some immunization against any exclusions of causally relevant, cross-national differences from the matrix of regressors; but it also downwardly biases (in absolute value) all regressors with cross-national components to their variation, because it monopolizes all cross-national variation (Judge et al. 1985). This variant reinforces evidence of estimator stability.

20. To jackknife, all nonparty variables with t-statistics below 1.0 in absolute value in the jackknifed result of Table 3, column 3, were deleted; and jackknife computations were performed on the resulting specification. Then, using results of these jackknives, nonparty and party product (or interaction) term variables with absolute t-values below 1.0 were deleted, and jackknife computations performed again. As above, we refrained from removing additive "party" specifications with t-values below the 1.0 criterion level because of their integral part in the interpretation of interactions (see nn. 13, 18). Pertinent left government, right government, and

right opposition variables have significant jackknifed slopes and may well be "stable."

21. An anonymous reviewer suggested that the median voters for whom parties compete sometimes favor welfare retrenchment *and* that center parties represent broad middle-class constituencies (cf. "risk-class?". Baldwin 1990). This pair of insights offers a good puzzle for future study.

22. For a few years, findings from Pampel and Williamson (1989) have clouded "partisan–welfare" relations. They coded their party variables so that first center, and later nonright, governments serve as benchmarks. Their analyses reveal that left welfare effort is less than center effort (chap. 3, Table 3) and center less than nonright (chap. 3, Table 6); but their analyses do not reveal whether or not left welfare effort exceeds right effort (as in, e.g., Hicks and Swank 1984; Stephens 1979). Moreover, that study suffers from an unnecessarily small and arbitrary sample of 126 observation (18 every five years, 1950–80) and a rather thin specification of equations. Certainly, ours are the first *clear* findings of distinct yet "less-than-center" effects. As for left corporatism's impact on year-to-year left rule effects, optimal precision may call for a less governmentally centered specification of left corporatism. On the other hand, precision might warrant a yet fuller specification of nonleft traditions of political economic strength. Present specifications of policy-relevant institutions should not be regarded as definitive.

23. See note 21.

24. *Party Leadership. Australia* conservative 1960–72 and 1976–82, left 1973–75; *Austria* center 1966–69, left 1970–82, none 1960–65; *Belgium* center 1960–71, 1974–80, none 1972 and 1981–82, left 1973; *Canada* right 1960–63 and 1979–82, center 1963–78; *Denmark* left 1960–67 and 1972–82, right 1968–71; *Finland* left 1967–72, 1973–75, none 1964 and 1971, center 1960–63, 1965–66, and 1976–77; *France* right 1960–80, left 1981–82; *West Germany* right 1960–66 and 1982, none 1967–69, left 1970–81; *Ireland* right 1960–82; *Italy*: center 1960–82; *Japan*: right 1960–82; *Netherlands* center 1965–66 and 1978–82, left 1974–77, none 1961–64, 1967–72, and 1973; *New Zealand* right 1960–72, 1976–82, left 1973–75; *Norway* center 1966–82, right 1982, left 1960–65, 1971–72, and 1974–81; *Sweden* left 1960–75, center 1977–82; *Switzerland* center 1960–82; *United Kingdom* left 1965–69 and 1974–78, none 1970, right 1960–64, 1971–73, and 1979–82; *United States* center 1960–68 and 1977–80, right 1969–76 and 1981–82. For rationales and summary of leadership periods, see n. 11.

25. The product terms for opposition–government interactions are center opposition × left party government leadership (Center–Left), right opposition × left party government leadership (Right–Left), right opposition × center party government leadership (Right–Center), left opposition × center party government leadership (Left–Center), left opposition × right party government leadership (Left–Right), and center opposition × right party government leadership (Center–Right).

26. See n. 6 for operational details.

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RESTRICTIVE LEGISLATIVE PROCEDURES IN FRANCE AND THE UNITED STATES

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I develop a framework for applying existing formal models of restrictive amendment procedures in Congress to the study of the French government's use of two restrictive legislative procedures, the package vote and the "guillotine". I test six hypotheses derived from existing formal models and from existing research on the French National Assembly. The analysis shows that the French government invokes the two procedures on the same types of distributive and jurisdictionally complex bills that frequently receive closed rules in Congress. The analysis also shows that the decision to use the restrictive procedures is strongly linked to the majority status of the government, suggesting they are used to preserve agreements between parties in the same way that restrictive amendment procedures are used to preserve agreements between individual members of Congress. Thus, existing formal models of legislative institutions can help us study how procedural structures shape strategic bargaining between political parties in parliamentary systems, especially during coalition and minority government.

Scholars widely recognize the fundamental role of formal institutional arrangements in democratic processes (Dahl 1989, pt. 4; Mayhew 1974; Riker 1980, 1986); and formal models of legislative choice have carefully explored how specific institutional structures shape legislative strategies and policy outcomes in the U.S. Congress. These models provide important insights into a wide variety of topics, including how institutional arrangements affect the distribution of benefits to congressional districts (Baron and Ferejohn 1989a, 1989b; Ferejohn, Fiorina, and McKelvey 1987; Shepsle and Weingast 1981; Weingast 1979); how institutional arrangements affect opportunities for congressional committees to exercise power against Congress (Denzau and Mackay 1983; Shepsle and Weingast 1987; Weingast and Marshall 1989); how institutional arrangements affect the efficient allocation of public resources (Baron 1990; Shepsle and Weingast 1984a; Weingast, Shepsle, and Johnson 1981); and how institutional arrangements encourage members of Congress to become policy experts (Gilligan and Krehbiel 1987, 1989). But although these formal studies make key contributions to our understanding of congressional politics, they have not been used to study how institutional arrangements shape legislative processes in parliamentary systems.[1]

The parochial application of existing formal models to Congress is unfortunate. On the one hand, comparative studies of parliamentary democracy suffer because they neglect the insights that emerge from the formal literature. On the other hand, formal studies of legislative institutions suffer because the failure to test existing models outside of Congress makes it difficult to determine if existing formal models in fact produce *general propositions* about institutional arrangements and choice processes. Consequently, claims about the comparative properties of institutional structures are difficult to defend; and theories about the choice between competing institutional arrangements are difficult to develop.

This research extends the application of formal models of legislative choice to the study of parliamentary politics. It focuses specifically on two procedures—the package vote and the "guillotine"—that sharply restrict the legislative role of the French National Assembly. I argue that existing models of congressional committees and closed amendment rules can be used to make predictions about the conditions that lead the French government (i.e., the prime minister and his cabinet) to invoke these two restrictive procedures. I then demonstrate that the French government's use of the package vote and the guillotine to resolve "distributive" problems is similar to the Congress members' use of closed rules to preserve distributive agreements. Since the logic underlying existing formal models of Congress can be used to explain the French government's use of restrictive procedures, the analysis suggests that future studies of parliamentary politics should look beyond the traditional focus on how institutional arrangements structure the hierarchical relationship between the executive and the parliament to a more general focus on how institutional arrangements constrain strategic interactions *between* parties.

THE PACKAGE VOTE AND THE GUILLOTINE IN FRENCH POLITICS

As part of an effort to redress the problems of cabinet instability and legislative paralysis that were endemic to the French Fourth Republic, the authors of the Fifth Republic Constitution created a variety of institutions intended to enable the government to restrict the legislative role played by the National Assembly. The constitutional assault on the National Assembly

focused to a large extent on the right of deputies to amend legislation. Under the rules of the Fifth Republic, deputy amendments cannot have the net effect of increasing spending or decreasing revenue (Article 40); deputy amendments can be declared inadmissible if they have not been submitted to the relevant standing committee for consideration (Article 44); and deputy amendments must be in the "domain of law" rather than the "domain of regulation" (Articles 34 and 41). In addition, the Fifth Republic Constitution established two constitutional procedures, the package vote and the "guillotine," that permit the government to exclude otherwise admissible amendments. The package vote, found in Article 44.3 of the Constitution, permits the government to group articles and amendments selectively, excluding the amendments to which it is opposed. Under this procedure, the parliament must vote either to accept or to reject the government's policy. The *guillotine*, found in Article 49.3 of the Constitution, permits the government to "engage its responsibility,"[2] that is, to attach the fate of a bill to a censure vote in the National Assembly. Under this procedure, there is no vote on the bill itself. Instead, all debate ceases immediately; and if a motion of censure is not introduced and voted within a specified time limit, the bill is considered as adopted in the form designated by the government.[3] The package vote and guillotine, then, are both institutions that force the National Assembly to make an "up or down" choice on a policy package determined by the government.

Existing general studies of French politics treat both the package vote and the guillotine as part of the government's large procedural arsenal that renders the French parliament impotent (Andrews 1982; Ehrmann 1983, chap. 10; Williams 1968; Wright 1989, chap. 7). The only studies that focus *specifically* on the package vote and the guillotine are in French, and the most common theme in these studies relates to the "antidemocratic" effects of both institutions. The French studies criticize the institutions because they permit the government to protect its bills from changes by deputies in the National Assembly. Hence, the procedures move final policy outcomes away from the preferences of the directly elected deputies toward the preferences of the government. This usage incites complaints typified by that of Senator Guille, protesting in Senate debate the use of the package vote in 1962: "[The government] leaves us with a frustrating obligation, either to consent to what we judge bad and dangerous for France, or to reject what we estimate necessary and urgent for the French" (quoted in Avril 1965, 441).

The guillotine is generally perceived to be more antidemocratic than the package vote because, in addition to limiting parliamentary amendment activity, the guillotine permits laws to be adopted without the parliament's voting on them. Deputy Paul Reynaud's 11 January 1961 letter to Prime Minister Michel Debré sums up this criticism: "The promulgation of [Article 49.3] violates two principles which are at the core of all democratic regimes. . . . The first is formulated . . . by Article 3 of the Constitution: 'The national sovereignty is held by the people, who exercise it by their representatives or by way of referendum'. The second . . . is found in Article 34: 'The law is voted by Parliament'. These two principles were violated thanks to Article 49 of the Constitution."[4]

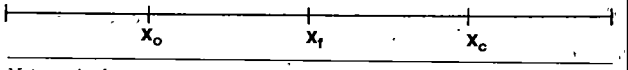
Reynaud was protesting the government's use of Article 49.3 to pass a bill relating to the *force de frappe*, which established France's nuclear weapons capability. Many deputies were hostile to the bill, and the government invoked the guillotine during the debate on a motion to send the bill back to committee before the bill was publicly debated in first reading. The government later invoked Article 49.3 during the second and third readings, so the *force de frappe* became the first bill ever to be adopted without a vote in the National Assembly (Lascombe 1981). In general, the procedure has been used on a wide variety of important bills, including bills related to the budget, social security, changes in French electoral law, the nationalization and denationalization of French industry, and private education.

Although the antidemocratic aspects of both procedures are widely discussed, scholars also recognize that the package vote and the guillotine can benefit deputies in parliament. This occurs when the government uses the procedures to protect its majority from potentially embarrassing votes on amendments posed by the opposition. Such usage has important stabilizing effects when the government's majority is divided or numerically weak (see Duverger 1987, 98–99; Parodi 1972, 26, 190–93; see also Avril 1965, 447). The procedures can also be useful in limiting or reversing changes made to a bill by the Senate or by a conference committee (*commission mixte paritaire*; see Avril 1971; Duverger 1987, chap. 4; Lascombe 1981).

In short, existing research argues that the guillotine and the package vote are used to protect the government's draft of bills from changes in parliament, to protect deputies in the majority from undesirable votes or debate, and to stabilize fragile government coalitions. These studies fail, however, to explain the conditions that actually lead the government to use the two restrictive procedures. For instance, although the studies suggest that the government uses the procedures to curtail parliamentary changes to government bills, they do not explain what types of bills are most likely to be subjected to the procedures. Similarly, although existing studies state that the procedures can be used to stabilize fragile coalitions, they do not predict how the government will use the procedures when it lacks majority support. We might expect, for example, that use of the guillotine declines during minority government because the government fears censure. We might also expect, however, that use of the guillotine increases during minority government because it may provide the only means for the government to enact bills opposed by parlia-

FIGURE 1

Strategic Committee Proposals under a Closed Rule



Note: x_o is the status quo, x_f is the ideal point of the floor, and x_c is the ideal point of the committee. If $|x - x_o| \leq |x_c - x_f|$, then a strategic closed-rule proposal by the committee is $x_f + |x_f - x_o|$.

ment. To fill such gaps in our understanding of the two procedures, we can turn to existing formal models of choice processes.

FORMAL MODELS OF RESTRICTIVE AMENDMENT PROCEDURES AND THE CASE OF FRANCE

Formal models have carefully examined the effects of restrictive amendment procedures on choice processes in the U.S. Congress. Much of this research follows Shepsle's pathbreaking (1979) analysis of structure-induced equilibrium, which demonstrates how policy choices can be understood as a function of the organization of committee systems and of the rules constraining the amendment process. Subsequent models have focused in particular on the importance of committee "gate keeping" power (the power of committees to refuse to report a bill to the floor), "open rules" (where the floor can offer amendments to a committee proposal), and "closed rules" (where the floor must vote either to accept or reject a committee's proposal without the opportunity to amend it).[5]

These models demonstrate that substantial insights into the role of restrictive procedures can be gained from considering strategic behavior by legislative actors.[6] Denzau and Mackay (1981), for instance, show that structure-induced equilibria may not exist when a legislator's preferences on one dimension are linked to preferences on other dimensions. Other models show that legislative rules placing restrictions on the parent body enable strategic committees to bring about outcomes that are not the most preferred policy of the floor (Baron and Ferejohn 1989a; Denzau and Mackay 1983). Under an open rule, a strategic committee will "close the gates" whenever the status quo is more desirable to the committee than is the floor median. Under a closed rule, if the floor's ideal point is between the committee's ideal point and the status quo, a strategic committee can exercise more influence over final policy than would be possible under an open rule. In Figure 1, for example, if preferences are symmetric and single-peaked, then a strategic committee can exercise influence under a closed rule that would be impossible under an open rule whenever the status quo lies in the interval stretching from x_o to x_f (where $|x_o - x_f| \leq |x_f - x_c|$). When the status quo is in this interval, the committee can make a proposal in the interval between x_f and x_c that is the same distance from the floor as was the status quo (so that the proposal by the committee is $x_f + |x_f - x_o|$), and this proposal will be adopted (see Denzau and Mackay 1983).

Existing models, then, demonstrate that in the pursuit of optimal policy outcomes, there are incentives for legislators to cut strategic deals, for the composition of majorities to change, and for actors to attempt to alter the dimensions on which tough legislative decisions are made. It is therefore important to understand not only the costs and benefits of strategic behavior, but also how legislators use legislative procedures to limit these costs and to enforce agreements.

Existing formal models of restrictive amendment procedures also shed light on the effects of uncertainty on procedural choice. We are particularly concerned in this study with uncertainty by the legislators about the preferences of the floor. Denzau and Mackay (1983) show that in a unidimensional world where committee members seek to maximize utility (given the committee's probabilistic beliefs about the floor's preferences), regardless of whether the amendment rule is open or closed, sincere behavior is optimal only if the committee believes with certainty that its ideal point coincides with the ideal point of the floor. Therefore, uncertainty introduces inertia into the system because it induces strategic committees to "close the gates" under an open rule. Similarly, uncertainty increases the likelihood that strategic committees will exploit the benefits of a closed rule.

The Case of France

Three differences between congressional and French politics must be considered before existing models of legislative choice can make sense in the French context. The first difference is the "unit of analysis." In Congress, the political actor is the individual legislator, whereas in parliamentary systems the most interesting and important unit of analysis is the political party (see, e.g., Powell 1989; Strom 1990a). In the French case, Converse and Pierce (1986) demonstrate the central role played by political parties in the representation process. The unified nature of party activity is evident in roll call voting analysis, where defections from party discipline are very rare (see Converse and Pierce 1986, 552–62; Wilson and Wiste 1976). Of course, political parties cannot be the only unit of analysis, because the discussion suggests that differences between the government and its parliamentary party (or parties) are sometimes resolved by the government's utilization of the package vote or the guillotine. Hence, the units of analysis in France are parties and the government.

The second difference is the dependence in parliamentary systems of the government on the confidence of parliament. The link between government survival and parliamentary confidence produces two dimensions of preference for each political party: (1) support or opposition for policy and (2) support

or opposition for the government.[7] For parties of the opposition, a proposed bill or amendment may represent an improvement over the status quo but nonetheless be rejected to keep clear for voters the distinction between government and opposition. Conversely, government parties in parliament may find a proposal worse than the status quo but nonetheless accept it to remain loyal to the government. The first dimension of choice clearly is tied to the (often Euclidean) spatial assumptions of most existing formal models. The second dimension of choice, however, implies an assumption of dichotomous preference—a party is either for or against the government.

The two dimensions of party utility in parliamentary systems may lead us to different expectations about the use of the guillotine, as opposed to the package vote. When the guillotine is invoked, there is no vote on the actual bill before parliament. Hence, the procedure does not require deputies to take an explicit policy position but, rather, a position regarding support for, or opposition to, the government. The package vote, on the other hand, requires that the deputies take an explicit policy position, because a vote on the bill itself occurs. There is not, however, a need to take a stand for or against the government. The two procedures therefore require choices on different dimensions of party preferences, one dimension related to policy and the other related to support for, or opposition to, the government. If different factors affect the two dimensions of choice, then the conditions leading the government to use the two procedures should not be the same.

The third difference between congressional and parliamentary politics relates to committees and amendment rules. The powers of standing committees in France are not at all similar to those of committees modeled in the existing formal literature. French committees cannot exercise gatekeeping power and are never granted a closed rule, because the government controls the parliamentary agenda (Article 42 of the Constitution). But existing formal models of committee power are applicable to France if we assume that the government is the "committee." Since the government controls the parliamentary agenda, it is endowed with both proposal and gatekeeping powers. In addition, when a government puts its own bill on the agenda, it is the text of the government's bill that serves as the basis for discussion (Article 48 of the Constitution), not the text of the committee's report of the bill.

If we assume that the government fills the role of committees modeled in the existing formal literature, then the "closed rule" in the French context is imposed when the government invokes either of the two "take it or leave it" procedures: the package vote or the guillotine. Of course, it is important to note an inconsistency between the closed rule modeled in the existing literature and the "closed rules" examined in France. The guillotine, unlike a closed rule, requires the National Assembly to choose not between accepting and rejecting a policy, but, rather, between accepting and rejecting the government. And unlike committees in the existing formal literature, the French government selects its own amendment rule.

In short, although there are differences between French legislative institutions and the institutions examined in existing models of legislative choice, the case of France provides an important opportunity to test existing models outside the U.S. Congress.

HYPOTHESES ABOUT THE USE OF RESTRICTIVE PROCEDURES IN FRANCE

I draw on this discussion to develop six specific hypotheses about the conditions that lead the government to invoke the two restrictive procedures. The first three derive from existing formal research on restrictive amendment procedures; the fourth derives from both the formal literature and existing French politics research; the last two derive from existing research on French political institutions.

HYPOTHESIS 1. JURISDICTIONAL COMPLEXITY. *The probability that the government will use restrictive procedures on a bill increases as the number of issue dimensions pertaining to the bill increases.*

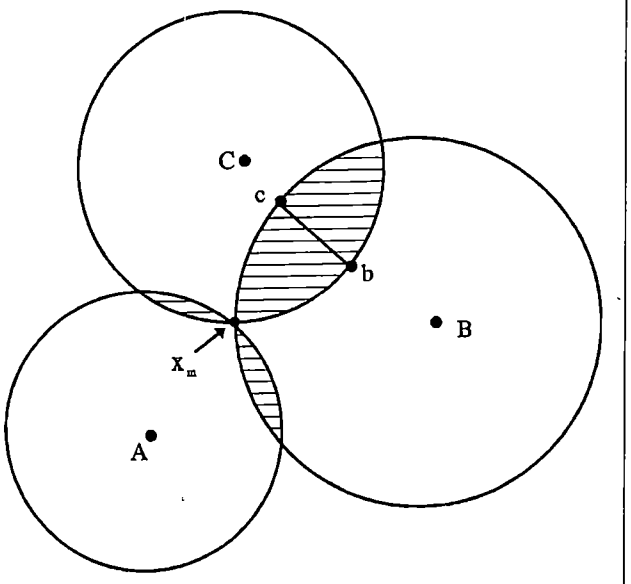
HYPOTHESIS 2. DISTRIBUTIVE BILLS. *The probability that the government will use restrictive procedures on a bill increases when the bill is distributive in nature.*

Hypotheses 1 and 2, discussed in detail by Krehbiel (1991; see also Denzau, Riker, and Shepsle 1985; Shepsle and Weingast 1984b), recognize that closed rules preserve "gains from trade" on dimensionally complex issues. Together, these two hypotheses represent a central result from existing formal models. An example of the logic underlying these hypotheses is given in Figure 2. Here, parties A, B, and C must choose a policy on two dimensions. If the two dimensions are voted one at a time under an open rule, the outcome will be x_m, the issue-by-issue median. However, any proposal in the shaded area (the win set of x_m) will defeat x_m. Hence, any two parties could benefit from gains engineered under a closed rule. For example, parties B and C could make a closed-rule proposal on the BC contract curve between the points b and c. Any point on the line between b and is c not only preferred by both B and C to x_m but is also Pareto-optimal in the sense that neither B nor C could be made better off without making the other party worse off. Both the package vote and the guillotine have the institutional structure necessary to preserve gains from trade in the manner suggested by this example.

Although the logic behind Hypotheses 1 and 2 is the same, jurisdictionally complex bills and distributive bills need to be kept conceptually distinct. A jurisdictionally complex bill touches on more than

FIGURE 2

Jurisdictional Complexity and Closed Rules



Note: x_m is the issue-by-issue median. The shaded area is the win + set of x_m.

one dimension of individual (or party) preference. For example, a bill concerning both the amount of money the state transfers to local communities for education and the total number of public education jobs paid by the state might touch on one dimension of choice related to the decentralization of political decision making and another dimension of choice regarding the amount of state money spent on education. A distributive bill, on the other hand, is one where the benefits are concentrated and the costs are spread out. Following the previous example, a distributive bill might be one where parliament must decide how geographically to distribute a fixed amount of state funds earmarked for education.

Students of parliamentary politics may be skeptical of Hypotheses 1 and 2 since parliaments clearly do not practice distributive politics like that in Congress. Although I do not want to suggest that French parliamentary politics is "distributive" in the U.S. congressional sense, it is important to recognize that parliaments must vote publicly on the distribution of limited public resources. Hence, although individual deputies may not frequently fight successfully on the floor of parliament for particularistic benefits, intuitions underlying Hypotheses 1 and 2 are relevant to parliamentary systems because they can help us to understand how governments use restrictive procedures to address problems between factions within a government party, between parties who are partners in a government coalition, and between the government and opposition parties during minority government.

HYPOTHESIS 3: GOVERNMENT STATUS. *As the government's status becomes weaker or more heterogeneous, the probability that restrictive procedures will be invoked on a bill increases.*

Hypothesis 3 is the only hypothesis strictly related to systemic rather than bill-specific factors. As I have noted, research on French institutions suggests that use of restrictive procedures is related to the government's status but does not provide a satisfying theoretical explanation of the relationship between systemic factors and the use of restrictive procedures. Existing formal research can help fill this theoretical void. In particular, formal research on the effects of uncertainty, strategic behavior, and dimensional complexity all lead to the development of Hypothesis 3.

Existing models show that as uncertainty about the preferences of the floor increases, the benefits to the proposer under a closed rule also increase. Hence, if we assume that a government is more uncertain about the preferences of the majority when the government coalition becomes more heterogeneous or loses its majority status, then closed rules should be used in such situations to combat uncertainty. Existing models also show that strategic amendment activity permits political actors to achieve more desirable policy outcomes than does sincere behavior. In parliamentary democracies, strategic behavior should increase during minority government because a wider variety of strategic coalitions are possible than is the case during majority government, when the roles of government and opposition parties are clearly delineated. Hence, restrictive procedures should be used by minority governments to combat strategic behavior by the opposition.

We can be more confident that the logic underlying formal models leads to Hypothesis 3 if there exists evidence of an increase in strategic voting behavior during minority government. Measuring strategic behavior is problematic, since it requires information about party preferences; but one possible approach in France is to assume that it is rarely sincere for the preferences of the Communists, a left-wing party, at once to differ from the Socialists and be the same as those of the conservative parties on the Right. This assumption, which is consistent with studies of the ideology of French parties and French party supporters (Converse and Pierce 1986), enables us to make inferences about strategic behavior from the voting records of the parties. Evidence of strategic behavior by the opposition parties exists if the Communists and the parties on the Right vote together to defeat the Socialists on motions, articles, or amendments during minority government (i.e., on votes that precede the final vote on a bill) but fail to defeat the Socialists in final votes on bills. Table 1 shows that the Socialists were defeated on motions, articles, and amendments 6% of the time in the first two years of minority government. As a point of reference, the first row of the table shows that this is substantially more often than was true during the Socialist major-

TABLE 1

Strategic Voting during Minority Government

TYPE OF GOVERNMENT	PERCENTAGE OF VOTES WON BY SOCIALISTS: AMENDMENTS, MOTIONS, ARTICLES	FINAL BILLS
Majority	99.2	100
Minority	94.0	99.5

Note: Data in the table are from over 500 votes randomly selected by me. The first row of the table covers the 1981–86 Socialist majority government. The second row of the table covers the 1988–June 1990 period of Socialist minority government.

ity government of 1981–86, which lost similar votes less than 1% of the time. However, in final votes on bills, the Communists and the parties on the Right joined together to defeat only one Socialist bill in over two hundred. Hence, the Communists and the conservative parties voted together only in votes that preceded the final decision on bills. The data in Table 1, then, suggest that minority government encourages strategic behavior by opposition parties; and we might expect the government to use restrictive procedures to combat this strategic behavior.

A final link between formal models and Hypothesis 3 derives from Budge and Fairlie's argument that parties in parliamentary systems are often linked to particular issues; that is, parties frequently "stress particular issues because they almost always work in their favor—they do, in a real sense 'own' them" (1983, 271; see also Budge, Robertson, and Hearl 1987). Parties on the Right, for example, can never hope to appear more committed to social reforms than parties on the Left; and parties on the Left can never hope to appear more committed to military defense than parties on the Right. If individual parties are primarily concerned with different dimensions of policy, then jurisdictional complexity increases with the number of parties in a government coalition. Hence, following Hypothesis 1, restrictive procedures can be used to preserve agreements between coalition parties during multiparty government.

HYPOTHESIS 4: URGENCY. *Restrictive procedures are used to hasten action on the floor of the National Assembly.*

This hypothesis derives from Baron and Ferejohn's (1989a, also 1989b) "divide the dollar" model, which shows that the benefits to all legislators of a closed rule increase as the costs of delay increase. In addition, French politics scholars argue that the procedures can be used to expedite the legislative process (Avril 1965; Duverger 1987, chap. 4). The package vote enables the National Assembly to avoid voting all of the amendments that are introduced by the opposition, while the guillotine immediately stops all debate on a bill.

HYPOTHESIS 5: AVOID POLITICAL EMBARRASSMENT. *The government will invoke restrictive procedures on bills to protect its majority from embarrassing votes and debates.*

This hypothesis cannot be derived from existing formal models because such models assume that voters or parties care only about final policy outcomes. Instead, the hypothesis derives strictly from the research on French politics I have discussed. The arguments presented above dovetail nicely with those of Baumgartner (1987, 1989), who argues that opposition deputies can use the French National Assembly as a tool for turning public opinion against government policies. The hypothesis predicts that as the controversy of an issue increases, pressures by parliament (and specifically the opposition) will increase in forms permitting the maximum focus of public attention on the issue. The government can react to limit the pressure by invoking the restrictive procedures. We should therefore expect the government to use the procedures on the bills that are most vigorously attacked by the opposition and during time periods when public debate on bills is most costly.

Since the logic behind Hypothesis 5 relates to protecting the government majority from undesirable votes or debate, it is difficult to generate different predictions for the two restrictive procedures. The package vote does not protect the majority from opposition attacks because under the procedure, authors can defend their amendments. It does, however, protect the majority from voting these amendments. The guillotine protects the majority not only from voting amendments but also from voting a bill itself. In addition, it protects the majority from protracted debate since all debate ceases immediately when the procedure is invoked. Under the guillotine, however, the opposition has wide latitude to criticize the majority during the debate on the censure motion that generally follows the use of the procedure.

HYPOTHESIS 6: POLICY DISTANCE. *The government will use restrictive procedures to prevent changes by the National Assembly to the government's draft of a bill; or, the probability that the government will invoke restrictive procedures on a bill increases as the preferences of the government and the National Assembly diverge.*

Hypothesis 6 emerges directly from the literature on French legislative politics, which argues that both types of restrictive procedures are used to protect the government's own bill from changes by a hostile National Assembly. This hypothesis is related to the frequent claim that both procedures (especially the guillotine) are antidemocratic.

THE RESULTS

Tables 2 and 3 give the results of multivariate analyses of Hypotheses 1–6 using data that I collected at the archives of the National Assembly in Paris. The

TABLE 2

Logit Analysis of the Package Vote

EXPLANATORY VARIABLES	(1)	(2)	(3)	(4)
Constant	−5.18 (1.12)	−4.38 (.98)	−5.31 (1.12)	−4.45 (.98)
Multiple referrals (*jurisdiction*)	.56 (.78) *.046*	2.38 (.60)	—	1.33 (.72)
Finance Committee (*distributive*)	2.73 (.84) *.46*	—	3.10 (.67)	2.20 (.76)
Coalition government	1.92 (.82) *.27*	2.07 (.79)	1.95 (.82)	1.65 (.78)
Minority government	5.75 (1.48) *.89*	5.39 (1.39)	5.80 (1.47)	5.42 (1.48)
Days until end of session	.002 (.006) *.00013*	.004 (.006)	.002 (.006)	.004 (.006)
Opposition activity (*controversy*)	1.25 (.29) *.14*	1.01 (.26)	1.29 (.28)	1.30 (.28)
Days until next election	−.003 (.001) *−.00023*	−.003 (.001)	−.003 (.001)	−.003 (.001)
Number of adopted amendments	.017 (.007) *.0011*	.011 (.006)	.018 (.006)	—
Report lag	−.01 (.01) *−.00073*	−.01 (.01)	−.01 (.01)	−.01 (.01)
Log likelihood	−43.61	−49.53	−43.87	−46.60
Number of cases	356	356	356	356

Note: The dependent variable takes the value 1 if the package vote is used, 0 otherwise. Standard errors are given in parentheses. The italicized number in column 1 gives the change in the probability of observing the package vote given a one-unit increase in the independent variable when the prior probability of observing a package vote is .07 (7% of the bills in the sample are subjected to the package vote).

TABLE 3

Logit Analysis of the Guillotine

EXPLANATORY VARIABLES	(1)	(2)	(3)	(4)
Constant	−4.00 (1.05)	−3.66 (1.00)	−3.88 (1.03)	−4.14 (1.05)
Multiple referrals (*jurisdiction*)	−.64 (.77) *−.023*	.65 (.61)	—	−.77 (.73)
Finance Committee (*distributive*)	1.97 (.68) *.22*	—	1.63 (.55)	2.02 (.67)
Coalition government	.56 (.76) *.034*	.77 (.73)	.54 (.75)	.64 (.76)
Minority government	1.93 (.86) *.22*	1.67 (.84)	1.39 (.86)	2.01 (.84)
Days until end of session	−.017 (.010) *−.00078*	−.015 (.010)	−.017 (.010)	−.017 (.010)
Opposition activity (*controversy*)	.76 (.29) *.051*	.62 (.25)	.69 (.25)	.74 (.27)
Days until next election	.0001 (.0008) *.0000033*	.0003 (.0008)	.00008 (.0008)	.0001 (.0008)
Number of adopted amendments	−.005 (.008) *−.00023*	−.008 (.008)	−.007 (.008)	—
Report lag	−.007 (1.06) *−.00032*	−.005 (.006)	−.006 (.006)	−.007 (.006)
Log likelihood	−53.17	−57.26	−53.52	−53.36
Number of cases	356	356	356	356

Note: The dependent variable takes the value 1 if the guillotine is used, 0 otherwise. Standard errors are given in parentheses. The italicized number in column 1 gives the change in the probability of observing the guillotine given a one-unit increase in the independent variable when the prior probability of observing a guillotine is .05 (5% of the bills in the sample are subjected to the guillotine).

data set includes information about amendment and procedural activity for 356 *projets de loi* (government bills) that were voted in first reading between the 21 March 1978 elections and the end of the legislative session in December 1989.[8] The bill is the unit of analysis, and the dependent variable takes the value one if the restrictive procedure is used and zero if it is not. The dichotomous nature of the dependent variable makes logit an appropriate model. Of the 356 bills, 37 (just over 10%) were subjected to one or both of the restrictive procedures. The frequency of French restrictive procedures is therefore greater in the restricted sample than the frequency of closed rules in Congress (1975–86), although a larger percentage of bills in Congress are subjected to modified restrictive rules (Bach and Smith 1988, 57).

I test Hypothesis 1 (that use of the guillotine should increase with a bill's jurisdictional complexity) by assuming that bills considered by more than one committee are jurisdictionally complex. Hence, *jurisdiction* is a dummy variable that takes the value one if the bill is referred to more than one committee, zero otherwise. I test Hypothesis 2 by assuming that Finance Committee bills are distributive, because they involve the levying of taxes and the distribution of money. *Distributive*, then, is a dummy variable that takes the value one if the bill's original jurisdiction is the Finance Committee, zero otherwise. The Finance Committee had original jurisdiction for 79 of the 356

bills in the sample, of which 12 were annual budget bills. Both *jurisdiction* and *distributive* should have positive coefficients if Hypotheses 1 and 2 are true.

The effects of government status (Hypothesis 3) are measured using two dummy variables. The first, *coalition*, takes the value one if the government is a coalition majority, zero otherwise. The second, *minority*, takes the value one if the government is a minority government, zero otherwise. The coefficients for each of these two variables are used to estimate the difference between the probability that a coalition or minority government will invoke a restrictive procedure and the probability that a single-party majority government will invoke a restrictive procedure.

I examine whether there is an increase in the utilization of restrictive procedures as legislative sessions approach their end to test Hypothesis 4 (that restrictive procedures are used to hasten decision making). *End session* therefore simply measures the number of days until the end of the parliamentary session and should have a negative coefficient if Hypothesis 4 is true.

Hypothesis 5 is difficult to test because of the problems inherent in determining the types of issues potentially subjecting the government to embarrassing debate or amendments. I consider two plausible approaches. The first measures the level of controversy raised by a bill as a function of the types of efforts made by the opposition to oppose the bill's adoption. *Controversy* measures four activities that the opposition can undertake to oppose a bill after the bill has come to the floor for discussion. The variable is constructed as follows: (1) If the number of amendments posed by the opposition is more than one standard deviation above the mean number of opposition amendments in the data set, the value one is added to the index; (2) if the number of suspensions of debate demanded by the opposition is more that one standard deviation above the mean number of suspensions demanded by the opposition in the data set, then the value one is added to the index;[9] (3) if the opposition demands a verification of the quorum, the value one is added to the index; and (4) if the opposition submits one (or more) of three preliminary motions (moving the previous question, moving the bill inadmissible, moving it be returned to committee for reconsideration) the value one is added to the index.[10] *Controversy* can take values of zero to four, with a higher value indicating a larger degree of controversy. If potential government embarrassment increases with the controversy of the measure, then *controversy* should have a positive coefficient.

Another measure of the potential embarrassment that a bill might cause the government relates to the timing of the bill with respect to the next election. If the government invokes restrictive procedures to avoid electorally difficult votes or debate, then the payoff of invoking the restrictive procedures should increase as elections approach. *Next election* measures the number of days from the time a bill is voted to the next parliamentary or presidential election and hence should have a negative coefficient to support Hypothesis 5.

I test Hypothesis 6 (that restrictive procedures are used by the government to protect its bill from change) by assuming that successful amendment activity is a proxy for differences between government and floor preferences. As the changes to the government's text of a bill increase, policy divergence increases. *Amendments* is simply the sum of all accepted amendments on a bill.

The final variable in the model, *report*, is a control variable not directly related to any of the six hypotheses. This variable measures the number of days between the submission of the bill by the government and the report by the relevant committee. It is a proxy for the importance of the bill to the government, because the government determines when bills are reported and debated in the National Assembly (and hence can call for its most important bills quickly while leaving less important ones buried in committee). *Report* should have a negative coefficient if the probability of restrictive procedures increases with the importance of the bill to the government.

Tables 2 and 3 give four specifications of the statistical model analyzing the government's decision to invoke the package vote and the guillotine, respectively. Column 1 in both tables includes all the independent variables, and the italicized number in column 1 gives the expected increase or decrease in the probability of observing the package vote or the guillotine, given a one-unit increase in each independent variable. The italicized number is calculated based on the assumption that the prior probability of observing the package vote is .07 (since 7% of the bills in the sample are subjected to the package vote) and that the prior probability of observing the guillotine is .05 (since 5% of the bills in the sample are subjected to the guillotine).

First consider Hypotheses 1 and 2. For the package vote, all specifications in Table 2 support Hypothesis 2. Column 1 shows that if a bill is submitted to the Finance Committee, the probability that the bill will be subjected to the package vote increases by 46%. Hypothesis 1 also receives some support in the model, with *jurisdiction* having the expected sign, albeit with a coefficient substantially smaller than that of *distributive* and with a standard error that leaves doubt about its statistical significance. However, an examination of columns 2 and 3 shows that the poor performance of *jurisdiction* in column 1 is probably due to multicollinearity between *jurisdiction* and *distributive*. In column 2, *distributive* is suppressed and *jurisdiction* is highly statistically significant with a coefficient substantially larger than is the case when *distributive* is included. In column 3, *jurisdiction* is suppressed and the coefficient of *distributive* is even larger than is the case in column 1. Although the data do not permit us to sort out the independent effects of jurisdictional complexity and distributive consequences, the specifications in columns 2 and 3 do give us reason to believe that both variables are important to the government's package vote decision. This

interpretation is supported by simply noting that 24% of jurisdictionally complex bills in the sample are subjected to the package vote, whereas only 3% of bills that are not jurisdictionally complex are subjected to the package vote.

The guillotine data do not support Hypotheses 1 and 2 to the extent that is true in the case of the package vote. Table 3 shows that *distributive* is statistically significant with the expected sign, supporting Hypothesis 2 but that the effect is much smaller in magnitude than is the case for the package vote. The data for the guillotine, on the other hand, do not support Hypothesis 1. The parameter estimate for *jurisdiction* is not statistically significant (and has the wrong sign) in all specifications of the model, including columns 2 and 3, which attempt to examine the effect of multicollinearity between *jurisdiction* and *distributive* on the parameter estimates for these two variables. In sum, two key hypotheses from existing formal models of closed rules (Hypothesis 1 regarding jurisdictional complexity and Hypothesis 2 regarding the distributive characteristics of bills) are supported by the tests for the package vote. The same is not true for the guillotine.

Next consider Hypothesis 3. The parameter estimates for the variables related to the status of the government support the hypothesis in the case of the package vote. The effect of minority government on the probability of the package vote is very large: if the prior probability of observing a package vote is .07 under a single-party majority government, the probability is .96 under a minority government. The existence of a coalition government also increases the probability of the package vote, but to a lesser extent than is the case with minority government. The existence of minority government also has a large and statistically significant effect on the government's decision to use the guillotine; but Table 3 shows that the existence of coalition government does not have a significant impact on the government's decision calculus.

The results pertaining to minority government deserve special comment because they are significant to our understanding of French politics in two respects. First, Wright (1989), Andrews (1982), and Frears (1981) each argue that parliamentary impotence in the Fifth Republic is due largely to the existence of a progovernmental majority; and they predict that this impotence may disappear in the absence of such a majority. The data presented indicate that this might not be true, because during minority government, the government's widespread use of restrictive procedures can limit the legislative role of parliamentary parties. Second, the government's frequent resort to the guillotine during minority government may have positive implications for cabinet stability. The guillotine enables opposition parties to oppose bills and then abstain on Article 49.3 censure motions. Hence, opposition parties can avoid both bringing down the government and paralyzing the legislative process without supporting either the government or its policies. The fact that French institutional arrangements create this strategy for opposition parties should make minority government in France more stable than might otherwise be possible.

The hypothesis that restrictive procedures are used to expedite the legislative process (Hypothesis 4) is tested by *end session*. Columns 1–4 indicate that the government's propensity to utilize the package vote is not affected by the government's desire to expedite the legislative process. Similarly, Table 3 shows that the guillotine is not used with increasing frequency as the legislative session draws to an end. These results are perhaps not surprising, given uncertainty in existing research about the extent to which the procedures can actually be used to hasten the process. The package vote does not limit the right of authors either to introduce or to defend amendments (Carcassonne 1988, 336), and the guillotine entails the delays of a censure motion.

With respect to Hypothesis 5, the coefficients for *controversy* are somewhat difficult to interpret given the nature of the variables; but their effects are clearly significant for both the package vote and the guillotine. In the case of the package vote, when the value of *controversy* is four (its maximum value), the probability of the package vote is .92 (.07 + .85). Hence, the effect of full-fledged opposition efforts to draw attention to a bill on the probability of the package vote are very important. The same is true for the guillotine, although the magnitude of the effect is smaller.

The number of days until the next election is also used to test Hypothesis 5. The variable is statistically significant in the expected direction for the package vote, although the magnitude of the effect is small. In the case of the guillotine, the coefficient is not statistically significant and has the wrong sign. The direction of the effect may suggest that the government does not like to use the guillotine as elections approach since its usage may send a sign that the government is without a majority for its policies. In fact, in the sample data, if there are less than two hundred days until the next election, less than 2.5% of all bills are subjected to the guillotine, while if there are more than five hundred days until the next election, almost 7% of all bills are subjected to the guillotine.

The number of adopted amendments is used to test Hypothesis 6, which states that the use of restrictive procedures should increase as the policy preferences of the government and the National Assembly diverge. Table 2 shows that the amendment variable is statistically significant with the expected sign. To interpret the magnitude of the effect, it is useful to note that if the number of successful amendments on a bill is equal to the mean number of successful amendments in the sample (i.e., 27 amendments adopted), then the probability of the package vote increases by only .03 over the case where no amendments are adopted. However, if the number of successful amendments is exactly one standard deviation above the average number of adopted amendments (i.e., 68 amendments adopted), then the probability

of the package vote increases by .13 over the case where there are no amendments.

Hypothesis 6 does not receive support from the data in the case of the guillotine. In Table 3, the parameter estimates for *amendments* are never statistically significant and have the wrong sign. Tables 2 and 3 therefore suggest that the government's use of the package vote is linked to the policy preferences of the legislative actors, but the government's use of the guillotine is not. This interpretation is further supported by the fact that in the sample 44 amendments are reserved, on average, when the government invokes the package vote, but not the guillotine, on a bill, whereas only two amendments are reserved, on average, when the government invokes the guillotine, but not the package vote. The statistical analysis therefore highlights the fact that use of the guillotine touches dimensions of preference related both to government support and policy outcomes, while use of the package vote touches policy preferences almost exclusively.

As a measure of policy divergence between the government and the National Assembly, *amendments* may raise certain objections. Successful amendments by the National Assembly to the government text may not, for example, be a good proxy for the differences between government and National Assembly preferences, because if a bill is multidimensional, a large number of amendments may not entail a large divergence between government and floor preferences. Similarly, large bills may be subjected to more amendments of all types than are small bills. Since the effects of amendment activity may be difficult to interpret, it is desirable to estimate the model without *amendments* to determine whether the inclusion or exclusion of this variable significantly effects the estimation of the other variables. This is done in column 4 of Tables 2 and 3. It is reassuring to note that the coefficients and standard errors of all the variables remain relatively stable for both the package vote and the guillotine when the amendment variable is suppressed.

Finally, the control variable in the model, *report* is not statistically significant for either the package vote or the guillotine. Therefore, if *report* is a good proxy for the importance of the bill to the government, then the importance of the bill does not affect the government's procedural choice when we control for other factors.

CONCLUSION

The analysis presented suggests that the government's use of the package vote *and* the guillotine is associated with the distributive consequences of bills, the level of controversy raised by bills, and the existence of minority government. The analysis also demonstrates that the package vote and the guillotine arise under different circumstances and are not procedurally interchangeable. The government's use of the guillotine (but not of the package vote) seems to decline as elections approach. More importantly, the government's use of the package vote (but not of the guillotine) increases with the jurisdictional complexity of bills, divergences between the policy preferences of the government and the National Assembly, and the existence of coalition government. The package vote, then, appears to be more "antidemocratic" than the guillotine in the sense that as the policy preferences of the government and the National Assembly diverge, the government uses the package vote with increasing frequency to reserve majority and opposition amendments. We might therefore conclude that the package vote is an institution used by the government *against* its own majority in the National Assembly, whereas the guillotine is an institution used by the government to *protect* its majority in the National Assembly from undesirable votes or debate.

The examination of the conditions that lead the government to invoke the two restrictive procedures reveals a similarity between congressional politics and parliamentary politics that is previously ignored by both Congress and comparative scholars. This similarity emerges from considering two results in the empirical analysis. First, the probability of both the package vote and the guillotine increases on controversial bills and on bills sent to the Finance Committee; and the probability of the package vote increases on bills sent to more than one committee. In Congress, controversial bills, bills referred to multiple committees, and bills referred to the "prestige" committees (e.g., Budget, Ways and Means, and Appropriations) are most likely to be subjected to closed or restrictive rules (see Bach and Smith 1988). French restrictive procedures, then, are used on the same sorts of bills that are subjected to restrictive amendments rules in Congress. Second, when we control for the characteristics of bills, the majority status of the government strongly affects the probability with which the government will resort to restrictive procedures. When the government has more than one party, use of the package vote increases; and when the government has only minority support, use of both the package vote and the guillotine increases. Hence, it seems that restrictive procedures in parliamentary systems may be used to preserve agreements *between parties* in the same way that restrictive voting procedures are used to preserve agreements between individual members of Congress.

Together, these two findings suggest a new focus in future studies of parliamentary politics. In the past, studies of legislative politics in parliamentary democracies generally examine the extent to which legislative power is concentrated in the hands of either the parliament or the executive;[11] and studies focusing specifically on parliamentary institutional arrangements often focus on how legislative rules affect the vertical relationship between executives and parliaments.[12] This study suggests that institutional arrangements play important roles in structuring not only legislative–executive relationships but also strategic interactions between parties that are

partners in coalitions and between government and opposition parties during minority government. Consequently, it is important to explore more generally how institutions shape legislative strategies and final policy outcomes when issues divide the government and parliament, parties within a government coalition, and (during minority government) the government and the opposition. This analysis suggests that such further research might clearly reveal claims about the antidemocratic nature of institutions like the package vote and the guillotine to be misguided. Instead, restrictive amendment procedures in parliamentary systems, like restrictive amendment procedures in Congress, might very well be found to enhance the efficiency of legislative bargaining processes and the stability of legislative outcomes. Further comparative research on this question is clearly feasible, since procedures like the French guillotine also exist in Germany (Dalton 1989, 59–60), Belgium, Finland, Italy, and the Netherlands (Inter-Parliamentary Union 1986, Table 40).

The analysis presented also has implications for the formal study of legislative institutions. The formal hypotheses related to jurisdictional complexity, distributive consequences, and the nature of the government coalition are all supported by the package vote data, providing new evidence that efforts to model legislative institutions are in fact producing general propositions about choice processes. At the same time, the guillotine data fail to support hypotheses related to jurisdictional complexity and coalition government, raising important questions about the circumstances under which we can apply existing models to parliamentary arenas.

It is not surprising that existing models explain the use of the package vote better than the use of the guillotine. First, the package vote procedure is directly tied to the policy content of bills, whereas the utilization of the guillotine is not; that is, use of the package vote requires the National Assembly to vote either for or against the government's package, whereas use of the guillotine does not require a vote on the bill but, rather (potentially), a vote on a censure motion. Since most existing formal models assume that party utility is strictly a function of short-term outcomes in a well-defined policy space, it seems logical that empirical results for institutions that touch directly on policy preferences (i.e., the package vote) are better explained by existing formal models than are empirical results for institutions such as the guillotine, which touch less clearly on policy preferences.

Second, use of the guillotine carries with it much higher political costs than does use of the package vote. Both procedures can give a public impression that the government lacks a majority for its policy, is internally divided, or is authoritarian against the directly elected representatives of the voters. But the costs of using Article 49.3 are higher because it invites the most "political" act in parliament—a motion of censure. Consequently, use of the procedure receives much greater attention and criticism from the media. Evidence of the higher political costs of the guillotine comes from simply noting that the guillotine is used much less frequently. From 1959 to 1990, the package vote was used 236 times while the guillotine was used 64 times (Maus 1988, 160, 181–82; Secrétariat Général de l'Assemblée Nationale 1988, 1989, 1990). Since existing formal models do not associate costs with the actual utilization of restrictive procedures, it is not surprising that the use of an institution heavy with such costs (e.g., the guillotine) is less empirically consistent with predictions of existing formal models than is the use of an institution for which the costs of utilization are low (e.g., the package vote). The differences between the package vote results and the guillotine results therefore suggest that it might be fruitful for future formal models of parliamentary politics to explore the inclusion of officeholding preferences into the objective functions of parties, as well as to examine the potential political costs associated with using particular types of procedures.

To conclude, my effort to use existing formal models of congressional politics to study the French government's use of restrictive procedures suggests that the emphasis in previous comparative research on the differences between congressional and parliamentary politics has perhaps led us to overlook potentially important similarities in the two types of systems. Specifically, the data suggest that restrictive legislative procedures are used on the same types of issues for the same types of reasons in the French National Assembly and in Congress. Future studies of parliamentary systems could therefore reap benefits by using the intuitions from formal models to study the types of issues that divide parties, and to explore how institutional arrangements are designed to permit competing parties to make and to preserve policy agreements. At the same time, future efforts to develop formal theories about how procedural structures shape the nature and stability of policy outcomes would be enhanced if attempts were made to develop models of noncongressional institutions and conduct additional tests of existing models in noncongressional settings.

Notes

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1. Formal models of coalition formation are an exception (see, esp. Austen-Smith and Banks 1988, 1990; Baron 1991; and Laver and Shepsle 1990). These studies do not, however, examine legislative politics after government formation is complete.

2. This is a literal translation from the French, for which I cannot suggest an appropriate English term.

3. The name *guillotine* does not come from cynical French political observers who see a parallel between the political procedure found in Article 49.3 and another political procedure commonly employed during the eighteenth-century "Reign of Terror." Instead, the name is taken from a close cousin that exists in Great Britain. The British guillotine, however, simply stops all debate on a bill. There is no formal procedure to attach the vote on the bill to a confidence vote (although the British government can state that it will resign if the bill is not adopted).

4. The full text of the letter is reprinted in Maus 1988, p. 183.

5. See Krehbiel 1989 for an excellent review of the formal literature on restrictive legislative institutions. Dion (1992) analyzes the robustness of Shepsle's structure-induced equilibrium.

6. A *sincere* legislative actor proposes, or votes for, his or her most preferred outcome at every stage in the legislative process, whereas a *strategic* legislative actor proposes or votes for alternatives (or amendments) that may not be most preferred at the intermediate stage in order to maximize utility of the final outcome.

7. See Strom 1990b for an excellent discussion of the office-seeking, vote-seeking, and policy-seeking motivations of political parties in parliamentary systems.

8. Several criteria were used to select the bills. First, since the National Assembly seldom plays a large role in foreign affairs, bills originally assigned to either the Defense Committee or to the Foreign Affairs Committee were not included. Second, to avoid the possibility that National Assembly activity reflects efforts to reverse changes made by the Senate, only bills originally submitted to the National Assembly were included. A random sample of the remaining bills were selected; but all finance bills, which dominate the legislative agenda each autumn, were included.

9. Article 58.3 of the Rules of the National Assembly permits the president of a parliamentary group (or his or her designee) to demand at any time a suspension of parliamentary debate for the purpose of a parliamentary group meeting.

10. If a party moves the previous question or moves that a bill is inadmissible (see Rules of the National Assembly, Article 91), a vote on the motion must occur before the individual articles are debated and voted (Article 104.2). The adoption of either type of motion defeats the bill. Parties can also move that bills be returned to committee for further consideration (Article 91.6–8). All three types of motions provide an opportunity for the opposition to criticize a measure publicly before any changes to the bill are made.

11. See, e.g., Lijphart, 1984 and Powell 1982, chap. 4.

12. See, e.g., Loewenberg and Patterson, 1979 and Mezey 1979.

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PROTESTANT THOUGHT AND REPUBLICAN SPIRIT: HOW LUTHER ENCHANTED THE WORLD

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Arguments about the emergence of modern political theory often claim that Protestantism's significance was that it evacuated the political world, that a more properly political ethic took its place, a "disenchanted" one. I shall consider Luther's understanding of biblical history, thoughts on the Christian prince, and view of the "bonds of union" between Christians in order to understand the relationship between the political and spiritual realms. I suggest that even though Luther argues for the separation of the two realms, his political realm is by no means disenchanted. His politics can only be understood in light of his claims about the purview of God the Father and God the Son. "Political vacuum theories," I suggest, misconstrue the relationship between politics and religion in Protestant thought.

It was Rousseau's claim that Christianity produces no "bonds of unions" between human beings (1984, 183). Bonds of union, he thought, must come from a source other than Christian religion if they are to be politically vital and without virulent excess. Christian religion, he thought,

> having no specific connection with the body politic, leaves the law with only the force the law itself possesses, adding nothing to it; and hence one of the chief bonds necessary for holding any particular society together is lacking. Nor is this all: for far from attaching the hearts of citizens to the state, this religion detaches them from it as from all other things of this world; and I know of nothing more contrary to the social spirit. (1984, 182)

Machiavelli, steering only a slightly different course, thought Christianity untenable because it was unable to guide the prince or would-be prince. That the necessities, the verities, of political life might be known required the exposition he would provide. Christianity invariably fails the political actor. Machiavelli would convey a new knowledge:

> Since my intent is to write something useful to whoever understands it, it appeared to me more fitting to go directly to the effectual truth of the thing than to the imagination of it. And many have imagined republics and principalities that have never been seen or known to exist in truth; for it is so far from how one lives to how one should live that he who lets go of what is done for what should be done learns his ruin rather than his preservation. For a man who wants to make a profession of good in all respects must come to ruin among many who are not good. (1985, 61)

Notwithstanding their many differences, Rousseau and Machiavelli accord in their view that a properly republican ethic must replace a politics *entranced* by principles now anathema to it, principles that can only cripple and diminish political life. Christianity is without bonds of union; Christianity cannot guide the prince. Effective political action demands that the verities of politics not be conflated or confused with those of religion—if the latter can be said to exist at all.

While this general argument about properly political action is one of long-standing pedigree, it has a peculiar resonation with the Protestant *theological* claim that the two realms, spiritual and carnal, must be separated that each may do its own work. Indeed, in Rousseau's thought there is a strange mixture of Protestant sensibility and republican spirit, a mixture lacking in Machiavelli's thought—perhaps because the Christianity of which he conceived belonged to the Roman Church. Whether he intended it or not, Rousseau sounds Protestant when he speaks of the true religion (which would complement his vigorous polity) as being, "without temples, altars or rituals, and limited to the inward devotion to the supreme God and the eternal obligations of morality [as it is found in the] pure and simple religion of the Gospel" (1984, 181).

This peculiar alliance of Protestant thought and Republican spirit is worth considering, not merely because it raises questions about Rousseau's ambivalence toward Christianity but also because the supposition has long been in force that Protestant thought *did* evacuate the political world and that a more properly political ethic was established as a consequence of that evacuation. Protestantism disenchanted the world, it removed the binding injunctions of God from the sphere of worldly activity, and left humankind on its own, without divine support. What Rousseau only hoped for—an inward devotion (Protestantism purified, perhaps) supplementing a purely political ethic—is often taken to be a description of the actual relationship between Protestant thought and Republican spirit. And this has given rise to one or another version of what may appropriately be called *political vacuum theories.*

POLITICAL VACUUM THEORIES

Nature abhors a vacuum, and so do political theorists. Political vacuum theories suggest that the political consequences (unintended, to be sure) of Protes-

tantism's insistence upon separating the spiritual and carnal realms was a vacuum of worldly authority in which a vital political ethic could reemerge. The recurring supposition in these readings is that Aristotle was right: to be human is to be a *zoon politikon* (1958, 5). Augustine's (later, the Reformation's) effort to orient the soul toward the Civitas Dei and away from the things of this world which "sway in their temporal instability" (1984, 5) could, for a time, supplant but not ultimately supersede this human verity. Fifteen centuries of bad (political) faith are brought to an end, ironically, by the very success of Luther and the other reformers' anxious struggle to rediscover and proclaim anew, as Augustine had once (1961, 212), the distinction between the carnal and spiritual realms, a distinction which the Roman Church had all but effaced.

As I have said, the supposition of political vacuum theories is that the political reemerges after an overlong hiatus. Wolin, for example, argues that Luther depoliticized theology while Machiavelli detheologized politics (1961, 142–44). Both assaults served to remove the religious obstacles to the reemergence of the political. Where the scholar sympathetic to Weber's (1958) view of history might announce—and perhaps decry—the onset of the world's *disenchantment* in the face of the onslaught, Wolin and other political vacuum theorists are untroubled by this development. One theorist's loss is another's gain. For the political vacuum theorists, disenchantment heralds not loss but rather recovery.

Sabine, while perhaps not as trenchant as Wolin, sees the significance of Protestantism in much the same way: Luther's withdrawal into the religious sphere left the world to its own devices. "Luther's stress upon the pure inwardness of religious experience inculcated an attitude of quietude and quiescence toward worldly power. Religion perhaps gained in spirituality, but the state certainly gained in power" (1937, 363).

Skinner not unlike Sabine, claims that "the main influence of Lutheran political theory in early modern Europe lay in the direction of encouraging and legitimating the emergence of united and absolutist monarchies" (1980, vol. 2, 113). The autonomy of politics, the infelicity of deriving political obligation from God's Word, marks the onset of modern political thought. "Any attempt to excavate the foundations of modern political thought needs to begin with the recovery and translation of Aristotle's *Politics*, and the consequent re-emergence of the idea that political philosophy constitutes an independent discipline worthy of study in its own right" (p. 349).

Pocock's arguments are consistent with the political vacuum theory, as well (1973, 148–201; 1975). In his view, Augustine initiates an epoch in which history gives way to eschatology (1975, 33). The events in the City of Man acquire whatever significance they may have in the light of their bearing upon the City of God, in which each individual soul may participate. Public history—the secular events that have shaped a people over time—is subordinated to the private search for redemption. Here is the "separation of redemption and history, salvation from society, soul and body" (ibid., 35).[1] Pocock's story is "of the revival in the early modern West of the ancient ideal of *homo politicus* (the *zoon politikon* of Aristotle), who affirms his being and his virtue by the medium of political action" (ibid., 550). While he does not trace the reemergence of a properly political ethic to Luther and the Reformation, he, too, supposes that Christianity (in its Augustinian mode, upon which Luther clearly relied) divested politics of its import to human life.

This *pneumatic* view of Christianity—whether one dates it from Augustine or Luther and the Reformation—has become an axiom on the basis of which theories of the emergence of modern political thought are developed. There is a prima facie case for this view, of course. At first blush it is uncontroversial; even a cursory glance toward Luther reveals his opposition to the Catholic analogical understanding and his insistence (qualified in important ways) that Law and Gospel—the carnal and the spiritual realm, the Old and the New Testaments—are, or pertain to, different orders that must not be conflated.

Luther's insistence upon separating the two realms, however, only captures a portion of what needs to be grasped to understand the subtle relationship between politics and religion in his thought and, by extension, in Protestant thought. The great paradox in Luther (his distinctions notwithstanding) is that the world is *not* disenchanted. A purely political ethic (and here, of course, Aristotle, whom Luther detested [1961a, 470–71] is the counterpoint) could not be countenanced. This is the theoretical perplexity that any interpretation of Luther must address. Luther's pronouncements of the stark disjunction between the two worlds must be juxtaposed with his view that *this* world is not emancipated from the other. Luther does, it is true, adduce a theological argument for separating the two worlds that each may be granted what it is due. This world, however, does not become disenchanted as a consequence of separating religious and worldly affairs. Needless to say, the theoretical significance of the underlying unity of these seemingly opposed notions bears on the question of the disenchantment of the world that Protestantism purportedly occasions. It is my contention here that Protestant thought does not disenchant the Roman Church–dominated world but, rather, enchants the world in a different manner than does the Roman Church. How this assertion bears on political vacuum theories is apparent and will be considered more fully in the Conclusion.

LUTHER'S UNDERSTANDING OF BIBLICAL HISTORY

It is perhaps not immediately clear why speculation about biblical history is pertinent to political theory; yet Protestant thought—theological and philosophi-

cal—has been preoccupied with it right from the beginning. Roughly, analogy is to Aquinas and the Roman Church what the dialectic of history is to Luther and Protestant theology, namely, a way to comprehend the relationship between the orders of reality. The dialectic Luther adduces—the theory of history that comprehends the meaning of Christ's *preservation and annulment* of the Old Law (Matt. 5:17)—is one that political theorists have sadly neglected. Yet *the* Reformation question was about the meaning of Christ's preservation and annulment. Were political theorists to recognize this, the extensive passages in, for example, Hobbes and Locke about the Old and New Testaments could be understood in their proper context, rather than dismissed as archaic or veiled discourse. Hobbes and Locke, like Hegel later on (1948), write of biblical history and the meaning of Christ's fulfillment of the Law not because it is a polite convention that must be observed but because massive political implications follow from the answer one provides (for a fuller discussion, see Mitchell 1991).

Luther's view of this dialectic is complicated; yet it is essential to understand its features if we are to establish the context within which he situates his politics.

For Luther, there are three moments of history, each of which has its corresponding "light" or truth: the Old Testament epoch, before Christ; the New Testament epoch, after the Incarnation; and the moment of the final revelation. The first light is the light of *nature*. By this light the meaning of suffering and injustice in the carnal realm, in the world, remains incomprehensible. Within the moment of history in which this first light shines, suffering and injustice are unsuccessfully treated as matters of offense against God. Here are the Jews.

Right conduct, works, and obedience to the Law, however, fail to recompense. It is by the light of *grace*—the promise of the New Testament—that suffering and injustice become comprehensible for the first time. What is more clearly revealed by this light is that God deplores not the death *of* his people but the death *in* his people (Luther 1961e, 391). The debt of suffering and injustice incurred cannot be discharged unless linked to a inward spiritual journey. By enduring hardships and not opposing the inscrutable will of God, the meaning of suffering and injustice is illuminated.

The light of the Old Testament is supplanted by one that illuminates the mystery of God more fully but not yet completely. Hence the need for a third light, more illuminating still than the lights of nature and grace. At the end of history, by the light of *glory*, the mystery of the God who has only partially revealed himself by his Word is illuminated (Luther 1961b, 202–3). The Word, long silenced by the cessation of prophecy and apostolic writing, gives way to the revelation of God that the Word had only partially, though increasingly, disclosed.

Because of the dialectical character of biblical history, each successive light or moment of history cannot be comprehended by the former. Importantly, however, the successive lights are *portended* by the former. The danger, according to Luther, is that Christians will attempt to comprehend the *new* spiritual truth in terms of the Law, the truth of this (post-Incarnation) moment of history in terms of what came before it. The passages on putting new wine in old bottles (Matt. 9:17; Mark 2:22, John 3:6), as well as Hegel's later injunction that "only spirit comprehends spirit" (1948, 255), reinforce the point Luther tries to make. Representationally, the temptation is for the Christian, like Adam, to hide from God (Gen. 3:7–9). And in this epoch, the "terrors of conscience" animate them, in effect, to hide themselves from their spiritual nature (which is disclosed through *passive* Christian righteousness) and to seek refuge in the *active* righteousness of works (Luther 1961d, 79), the pre-Incarnation way to atone for sin:

> Man's weakness and misery is so great, that in the terrors of conscience and the danger of death, we behold nothing else but our works, our worthiness and the law. . . . Thus man's reason cannot restrain itself from the sight and beholding of this active reason or working righteousness . . . nor lift up her eyes to the beholding of passive or Christian righteousness. (1961c, 101)

This picture is complicated, however, by the fact that while Christians attain a foothold in the spiritual realm through Christ in this epoch, they do not become *as God*. Christians retain a "remnant of sin" because they still possess a carnal body. The finite human being remains finite despite the righteousness attained through Christ. In Paul's words, "I see another law in my members, warring against the law of my mind, and bringing me into captivity to the law of sin which is in my members" (Rom. 7:23). The second, penultimate spiritual moment, consequently, remains shrouded in mystery and sin despite the depth of soul attained by the Christian. Christ and the truth of the New Testament deepen the Christian,[2] but fundamental mystery still abounds. The guide, in the midst of this mystery, can only be the Word, the meaning of which will be fully disclosed only at the time of the final Revelation.

TEMPORAL AUTHORITY AND THE ENCHANTMENT OF POLITICS

The political implication of the denial that new wine can be put in old bottles, that post-Incarnation spirit can be comprehended by pre-Incarnation Law, is that temporal authority should not to be allowed to judge the "secret, hidden, spiritual matter" of faith (Luther 1961e, 385). The physical power that is the medium of temporal authority cannot in principle, and ought not in practice, be used to confront the believer. The physical power of earthly kings is of a different order from the spiritual power Christians "possess" through Christ. Heresy, consequently, can never be prevented by force (p. 389). Worldly government extends to life and property, but over the soul it holds

no sway; belief and unbelief are matters of conscience, not power (p. 385).[3]

That Luther (and Locke) should implore that the two realms be separated already implies that they have been confused, mixed together in a way that denies precisely what Christ announced: a separation of the carnal and spiritual realms.[4] The carnal and the spiritual realms have been so confused by humankind, Luther says, that bishops rule over cities while lords rule over the human souls (ibid., 389). The religious task is to separate the two realms so that salvation may be achieved—from which it must be concluded that the justification for separating the political and theological realms is theological, *not* political.[5]

Care must be exercised here, however; for while the post-Incarnation spiritual domain of God the Son is "new wine," the "old bottle" of politics that cannot hold it is despiritualized but not desanctified (as political vacuum theories would have it). In Luther's terms, while the government that rules over Christian souls is an invisible government where "all are alike subject to each other" (1961e, 391), nevertheless, because the great majority of human beings will never be Christian, the invisible government that ministers to the piety of Christian souls must be supplemented by a visible government that produces peace. And this visible government is justified not on "purely" political grounds but, rather, on the basis of God the Father's pre-Incarnation covenant with the children of Israel. There is a need for the *temporal* sword for the children of the world, but this is decidedly not a *secular* sword. Hobbes, of course, would later make much of the Mosaic covenant with God the Father in the *Leviathan*, Parts 2 and 4, as well as in portions of *De Cive*, in order to make precisely this point: the real foundation of government is established in the paradigmatic act of the Mosaic covenant linking the citizens to God the Father through an awful Moses/Leviathan.

Returning to Luther, in biblical terms, Moses' Law was given to the children of this world that there might be order; and this sword is not abolished by Christ. Moses' Law still applies to the unrighteous. The fulfillment of the Law by the Gospel purported to be offered by Matthew's Christ (5:17) pertains to Christians only. The sword, therefore, is necessary for the unrighteous and justified by "divine ordinance" (1961e, 411). Indeed, the wrath of the Father, acting through the temporal prince, is necessary to constrain the Nietzschean beast within. "If anyone attempted to rule the world by the Gospel and abolish all temporal law and sword, . . . he would be loosing the ropes and chains of the wild savage beast and letting them bite and mangle everyone" (p. 370).

Were we to stop here it could be said already that Luther did not wish a wholly disenchanted politics in order to attend more fully to the spiritual realm. The Law is superseded for Luther, but he does not completely sever the link between "spirit" and worldly government. Rather, worldly government is given a meaning other than that adduced by the Roman Church. It is comprehended as a moment within the purview of God the Father—a moment that is part of the higher unity of the Trinity yet different from God the Son, who occasions the separation of the spiritual and carnal realms. Worldly government is for the unrighteous. Following Augustine, only true Christians are citizens of the Heavenly City. The post-Incarnation truth is for the few; but because the few must live with the rest, the pre-Incarnation justification of the right to rule granted by God the Father still stands. All this, I should add, reinforces what I suggested earlier—that speculation about the meaning of Christ's fulfillment of the Old Law is political theory disguised in a form that modern commentators often fail to recognize.

I have pointed out why the carnal, earthly realm cannot rule over the spiritual realm: it cannot comprehend it (new wine cannot flow into old bottles). Against the Roman Church, however, Luther argues that the obverse is equally untenable: where the spiritual has dominion over and in the carnal realm, the result is calamitous. This approach, too, would destroy the distinction between the two realms. When this happens, "the door is opened for every kind of knavery" in its name (1961e, 371–72).

> I maintain, that since the temporal authorities are ordained by God to punish evil-doers and to protect the law abiding, so we ought to leave them free to do their work, without let or hindrance everywhere in Christian countries, and without partiality, whether for pope, bishops, pastors, monks, nuns or anyone else. (1961a, 410)

The work of temporal authorities is one thing; that of the spiritual kingdom, another. By leaving temporal authorities free to take care of their affairs (again, this is justified on the basis of the right to rule established in the Old Testament), obstacles to salvation are removed. By insisting that the Church have no say in temporal matters, the work of this world is left to those most able *and authorized* to perform it. Separate the two realms and both operate without doing violence to the other. The possession and use of power in the realm of politics is justified by the Old Testament, and the spiritual power that fulfills the Old is established by the New. God the Father comprehends power, and God the Son comprehends spirit.

On the basis of this argument for separating the two realms, it should be evident that living under the jurisdiction of God the Father is clearly different from living under God the Son. Living under the Father, however, is decidedly not the same as living in a disenchanted world. What political theorists have failed to grasp is that the distinction between enchantment and disenchantment does not map onto Luther's distinction between carnal and spiritual, between God the Father and God the Son. The carnal realm is not spiritual, to be sure (though it portends it); but the former is nevertheless comprehended by a moment of the triune God.

This point is helpful, if not crucial; but it does not

take us as far as we need to go. Worldly government, we have seen, finds its authorization in God the Father. Here is a justification for rule over the unrighteous. But what of the righteous, of Christians? What are their bonds of union? These are questions that can be best answered, I think, by looking first at the Christian prince (the person who is both Christian *and* prince, the person who acts *in* the carnal realm but is not *of* it) and then considering the bonds of union between the righteous (Christians), who are not compelled to occupy the limit position that the Christian prince must.

THE CHRISTIAN PRINCE

The prince, as I have mentioned, receives authorization for the use of his power from the Old Testament. This does not mean, however, that all things are permissible, as some German princes erroneously inferred from Luther's stark separation between spiritual and carnal realms (1961e, 365–66). Precisely where the limitations of their power lie will not concern me here. Of greater theoretical significance is Luther's discussion of the *Christian* prince; for in this figure must be found both (political) power and (spiritual) power, which latter at first appears to require an utter rejection of worldly power. The theoretical difficulty is how to be both Christian and prince, how to live in accordance with both the pre- and post-Incarnation dispensations.

When Luther appeals to Christian princes to liberate the German Christians who "are oppressed by distress and affliction" (1961a, 404) he is asking them to exercise power in accord with a spiritual realm uncovered only in the abyss of *powerlessness* and denial of the world. In the Christian prince—that locus of the extraordinary conflation of the two realms—was to be found a release from the yoke German Christendom had borne.

First and foremost, because physical power (of the sort possessed by earthly kings) can only be used to oppose, to stand against, great evil will result if this form of power alone is relied upon. The prince cannot *simply* oppose; such opposition is the way of the world, not the spirit. We must not use our own power to oppose the Church, wrote Luther; for "the greater the power we employ, the greater disaster we suffer, unless we act humbly and in fear of God" (1961a, 406). Humility and fear are, of course, the functional equivalents of the pity Rousseau speaks of in *Emile* (1978, 220–30): they arrest pride before the person whom pride dominates gets lost in the temptations of the world. But power must be exercised, nevertheless; for physical power is needed to oppose the equally physical power exercised by the Roman Church over the German states.

This may seem like a trivial matter to the modern ear, but it was not trivial to Luther; for the prince who relied only on the physical power at *his own* disposal was bound to lose sight of the Source of all power and so suffer hubris, thus foreclosing an encounter with the abyss of Christ, which alone could make him a Christian prince. Weber's definition of political power as "the monopoly of the legitimate use of physical force within a given territory" (1946, 78) may seem axiomatic to us today; but the idea would have seemed ludicrous to Luther. (We tend not even to see it as a theoretical difficulty.) Luther would say that the Weberian view could only countenance hubris and that it is un-Christian—to which Weber would emphatically answer "Yes."

The intractability is resolved by enjoining princes to rely on the power of God, not their own power, to oppose the oppressors: "We must not begin by assuming we possess much strength or wisdom, even if we had all the authority in the world. For God cannot and will not suffer a goodly enterprise to be begun if we trust in our own strength and wisdom. God will surely abase such pride" (Luther 1961a, 405). The power that is to be used *in* the world must come from beyond it;[6] only when the power used to oppose the Roman Church comes from God, in other words, *can* German Christendom be released. The foreign threat can be averted only with God's assistance. A higher power must be the source of worldly power if worldly power is to have a beneficent effect at all. Only with assistance from God can a true defense be made against "the ravening wolves who come dressed in sheep's clothing" (p. 422).

In domestic affairs, too, both power itself and (more importantly) the pattern of its use must come from God, rather than from the prince's person. The Christian prince must act in a manner befitting both an earthly king and a Christian subject. The prince must exercise power, it is true; but the use to which such power is put is informed by the prince's experience as a Christian. He is not powerless toward his subjects, but his servitude before Christ establishes the true nature of his relationship with his own subjects. Christology offers a pattern for polity.[7] The Word, at any rate, offers guidance. Because God once spoke as an ass, for example, the prince must respect people of even the lowest station; but because God's highest angel fell, the prince must trust no one (Luther 1961e, 395). Christian subjects, Luther enjoins, are both Christian and subjects. In their latter capacity they occasionally conspire to acquire the power of earthly kings. Respect and distrust go hand in hand. Here we see the political wariness of Federalist 10 (1961, 77–84) still wearing overtly theological garb. Both are dubious about the beneficence of human motivation—and yet both would carve out a domain in which the self was sacrosanct.

Guidance for political life, I have said, comes from the Word. This does not mean, however, that the actual laws instituted by the Christian princes must be taken verbatim from the Bible. Tocqueville reminds us, after all, that the Gospel enjoins only general principles (1966, 445). The right of making law is justified by the Word of God, but the content of such laws is not to be found in Scripture itself.

Just as new wine cannot be put in old bottles, so, too, the spiritual lesson of Christ that must serve as a

guide to the Christian prince cannot be reduced to the letter, to lawful commands. This was Hegel's point in "The Spirit of Christianity and Its Fate" (1948, 209); and it is a crucial insight for the reformers. Reformation political theology could not simply construct a politics drawn from Scripture alone. Strauss completely misunderstood this when he said that had Locke wished to develop a political theory based on the Bible, he would have written a *Politique tirée des propres paroles de l'Écriture Sainte* (like Bossuet) rather than the *Two Treatises* (1974, 205). Locke and the other reformers could not lift chapter and verse from Scripture and make them political laws; for that would have violated the very principle that the Reformers insisted upon, namely, that the spirit and the Law are different yet dialectically related. The laws of the Christian prince, therefore, divulge something of a living tension between the letter of the Law and the spirit that informs it—one that is resolvable only by relying upon God rather than self:

> We should keep written laws subject to reason, whence indeed they have welled as from the spring of justice, and not make the spring dependent on its rivulets, nor make reason captive to the letter.
>
> I know of no law to prescribe for a prince, but will simply instruct him what the attitude of his heart and mind ought to be . . . so that if he governs himself thereby, God will surely grant him the power to carry out all laws . . . in a proper and Godly way. (Luther 1961e, 394, 402).

The laws of the Christian prince, then, are not simply his own. They issue from his person but are guided by God; and in the post-Incarnation epoch such "guidance" should involve a recapitulation in the (domestic) political domain of the pattern established by Christ and all Christians: "Thus a prince should in his heart empty himself of his power and authority, and interest himself in the need of his subjects, dealing with it as though it were his own need. Thus Christ did unto us; and these are the proper works of Christian love" (ibid.).

BETWEEN THE RIGHTEOUS: BONDS OF UNION IN CHRIST

For Luther, Christians cannot have a genuine affiliation with others without the assistance of Christ. Only through a "marriage" between a single Christian and Christ where no others are involved does the basis for human community come to view. At one level, of course, this tentatively confirms what Rousseau would later insist—that the "other" for Christians is Christ, not other citizens—from which no genuine political affiliation could come. But this is not the whole of the matter. Indeed, Rousseau's error stems from not seeing how the pattern of the singular relationship, the marriage, turns back onto the world, so to speak, and offers a pattern for the right relationship between human beings.

In a word, the obligations that bind one human being to another derive from the relationship between the individual Christian and Christ. Through a marriage with Christ that excludes all others, the Christian is able to—and indeed must—include all others; the pattern established by the bridegroom (Christ) is reproduced in the bride's (the Christian's) relationship to all others:

> As our heavenly Father has in Christ freely come to our aid, we also ought freely to help our neighbor through our body and works, and each should become as it were a Christ to the other that we may be Christs to one another and Christ may be the same in all, that is, that we may be truly Christian. (Luther 1961d, 76)

The marriage with Christ, as it turns out, provides the basis for a twin orientation—toward the Divine and toward all others: "A Christian lives not in himself, but in Christ and his neighbor. . . . He lives in Christ through faith, in his neighbor through love. By faith he is caught up beyond himself unto God. By love he descends beneath himself unto his neighbor" (ibid., 80).

The relationship to the neighbor is not based on love alone, however. The righteous Christian has no need for the sword and would endure suffering from it if it came to that. With regard to self, the Christian must be powerless; but with regard to the neighbor, the Christian must punish the enemy who harms. There is no right of revolt (this would come later with Locke), but there is the right to protect: "[Although] you have no need [of the temporal sword and the law] and are not to have them, [you are] to serve those who have not progressed as far as you and still need them. Although you do not need to have your enemy punished, your weak neighbor does" (ibid., 374).

These examples should make clear that the coming to presence of the spiritual realm, the marriage with Christ with which Luther was so concerned, was to *bear fruit* in the (carnal) realm—the central motif of the Gospel of Matthew. It is to be the occasion for reproducing the pattern established by Christ in the relationship with the neighbor. The Christian "labors on earth not for himself but for his neighbor" (ibid., 373). Through Christ the Christian enters the spiritual realm, which occasions both freedom from, and supersession over, the carnal realm; the Christian attains a freedom from all *things*, that is, everything carnal. Here, the Christian reproduces the pattern established by Christ and, like Him, becomes a servant to all. Christians depart from the carnal world to return to it grounded in the spiritual realm that provides the wherewithal for their authentic return. Only "a pattern set up in the heavens" (Plato 1985, 284) makes possible genuine sociality.

CONCLUSION

Consider Rousseau's thoughts on Christian politics in *The Social Contract*:

> But I err in speaking of a Christian Republic; for each of these terms contradicts the other. Christianity preaches only servitude and submission. Its spirit is too favorable to tyranny for tyranny not to take advantage of it. True Christians are made to be slaves; they know it and they hardly care; this life has too little value in their eyes. (1984, 184)

While partially correct on the matter of servitude and submission, he clearly failed to understand that even the seeming otherworldliness of Protestant Christianity, insisting on a separation of the political and religious realm, could provide the bonds of union for a community. Rousseau recognized Protestantism's separation between the two realms but did not recognize the bearing of God the Son (in addition to that of God the Father) upon political life.

Straining Rousseau somewhat, in the idiom I have been using here, the problem he sought to redress was that God the Son had overpowered God the Father, that people had turned to the utterly internal realm of faith (the Son) to the detriment of political life (the Father). This is, of course, ironic; for Luther had argued that while God the Father authorized earthly kings to chastise the unrighteous, genuine community was possible only through God the Son.[8] The bonds of union that Rousseau thought so essential could only come, according to Luther, from the mediator of faith (the Son), who also occasions a City of God *next to* the City of Man. Were Luther to answer Rousseau he might have said that if Christians were only servile and unable to form a viable political community, it was because they understood the path (servitude), but not what it led to (being brothers in Christ). In effect, the problem would be that they were not Christian enough. As Christ was a stumbling block to the Jews (1 Cor. 1:22–23), so was He to Rousseau also.

My intention here, however, is not simply to contrast Luther and Rousseau. It is to suggest that the kind of error Rousseau makes—that of attributing to Protestantism a wholly otherworldly orientation—is the same kind of error made by adherents of one or another version of the political vacuum theory about which I spoke at the outset. Protestantism does not disenchant the political world; the world is the Father's, spirit is the Son's.[9] The republican spirit that Rousseau so hoped to see supplement the "pure and simple religion of the Gospel" (1984, 181) cannot be understood to have diffused into the evacuated political space left in the wake of Protestantism's separation of the political and religious realm. A wholly disenchanted republican politics does not emerge victorious as a result of Protestantism's attempt to return to an uncorrupted Christianity; that is, if the republican spirit came to predominate in certain regions, the mechanism by which it came to do so was not an evacuation of religious content from the political sphere accomplished by Protestant thought. The world remains enchanted in Protestant thought, for different reasons than it had been in Catholic thought, to be sure; but the prima facie case that Protestantism's indictment of the world is equivalent to its desantification does not hold up under scrutiny. For the unrighteous, the world remains under the sway of God the Father; for the righteous few, the world may perhaps for a time be modeled after the pattern established by God the Son.

Notes

1. A perusal of Augustine's *City of God* 5.12–19 (1961, 196–212) suggests that the contrasts Pocock makes are simply too stark. Augustine suggests that Rome itself was necessary in order for salvation through Christ to occur. The biblical passage here is Galatians 4:4 ("But when the fullness of time had come, God sent forth His Son"). Augustine argues—much as Tocqueville would later (1966, 446)—that there was something about Rome's activities in the world that made it possible for Christianity to take hold. *The fullness of time* suggests that at a certain moment the carnal world was able to accommodate spirit. And Augustine thought that the greatness of Rome was that it prepared the way in time for the advent of Christ. His interest was not wholly in turning allegiance away from the City of Man (Rome, literally and symbolically); for it was through Rome, through the carnal realm, that the spiritual realm came to presence. In a word, contra Pocock, the separation between redemption and history, salvation and society, soul and body, is not unequivocal. As Augustine well knew and noted in his *Confessions* (1961, 100), the road to God must pass through Rome (literally and symbolically).

2. Consider Nietzsche on this matter, especially in regard to my earlier comments about the second light of history (the light of grace of Christianity) disclosing the meaning of suffering. "The meaninglessness of suffering," Nietzsche says, "*not* suffering itself, was the curse that lay over man so far—*and the ascetic ideal* [Christianity] *offered man meaning*" (1967, 162). This ascetic ideal "brought fresh suffering with it, deeper, more inward, more poisonous, more life destructive suffering: it placed all suffering under the perspective of *guilt*" (ibid.). This agreement between Luther and Nietzsche about the deepening of humankind accomplished by Christianity may be their only common ground. Nietzsche's contempt for the Reformation, that epoch in which the notion of the equality of all finally triumphed, is all too apparent (ibid., 54).

3. Locke would later make a similar claim in his *Letter Concerning Toleration* (1955, 18). His argument there and in *The Reasonableness of Christianity* (1803) is based on a reading of Christ's fulfillment of the Old Law and in *this* respect covers no new theoretical ground. I have discussed this elsewhere (Mitchell, 1990).

4. Hobbes, of course, was at great pains to deny that there was a spiritual realm at all. Christianity, in a word, did not occasion a separation between two realms. Prior to Christ, as well as after his first coming, there is only the world—the carnal realm (1962, 340).

5. See Smith 1991 for the disturbing implications this has for the constitutional debate about religious freedom. On his view, religious freedom today, "at least as it has come to be understood, forbids governmental reliance upon religious justifications as a basis for public policies or decisions. Therein lies the paradox—our constitutional commitment to religious freedom undermines its own foundation; it cancels itself out by precluding government from recognizing and acting upon the principle justification supporting that commitment" (p. 200). Theologically, the world (politics) cannot stand without a deeper (nonpolitical) foundation.

6. Compare this to Augustine's "When You are our strength we are strong, but when our strength is our own we are weak" (1961, 89).

7. The Leviathan about which Hobbes speaks can be seen as a recapitulation of this pattern. The Leviathan, king of the children of pride, like Christ, takes in upon himself the pride of the children. What Hobbes wanted above all was to quell

human pride; he thought a Leviathan who, like Christ, was a mortal–God (1962, 132), was the only figure capable of doing so. Hobbes's genius and Achilles heel was to have transposed a religious symbol into a temporal one. Instead of a religious mode of atonement for the problem of pride, he offers one that is temporal.

8. Locke would later argue the same point. Prior to Christ and Christian virtue, he asserts, the Law "reached no farther than those things, that would serve to tie men together in subjection" (1803, 225).

9. Most importantly, because the Word was *made flesh* in the Son (John 1:14), those who confess the way of Christ are offered a way—indeed the only genuine way—to form a community. From Augustine to Luther to Bonhoeffer (1955) in the twentieth century, this does not change.

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THE THEODICY OF THE *SECOND DISCOURSE*: THE "PURE STATE OF NATURE" AND ROUSSEAU'S POLITICAL THOUGHT

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Since Rousseau, and perhaps because of him, political theory has often been characterized by a disjunction between considerations of human nature and questions of justice and law. Yet, reexamining the Second Discourse *as a theodicy forces us to rethink how the essential questions of political theory are related just where they seem to become separated. The core of the theodicy is Rousseau's view that we are, by nature, physical beings embedded unproblematically in nature—good, or ordered, beings in a good, or ordered, whole. Although Rousseau presents his explicitly political thought as a juridical doctrine seemingly separated from his understanding of human nature and history, his political thought is founded upon his portrait of man's existence in the "pure state of nature." This portrait serves as a positive formal model to enable us to remake our corrupted existence through the legitimate state—itself modeled by Rousseau on the divine or natural whole.*

As citizens and students of the liberal regimes of modern times, we are heirs to a problematic political project and political science. Modernity, especially as it comes out of the Enlightenment, might be characterized as an undertaking both dedicated to the establishment of liberty and founded upon a reductionist study of humans and their political life. The problematic character of modernity and liberalism is thrust to our attention today as those who have thrown off their shackles look to our political experience and political science for guidance at the same time that they question its substance and sufficiency. Now is the time not for self-satisfaction but self-knowledge.

Jean-Jacques Rousseau faced the dilemmas of modern politics and political science with a clarity seldom rivaled. He was the first important critic of modernity and liberalism within the modern and liberal fold itself. Rousseau is at the font of the peculiar self-questioning character of modernity and perhaps its most profound and influential critic. His acute queries and his troubled gaze have influenced critics of modernity on both left and right, and his questions continue to be our own. In order to begin better to understand our predicament and ourselves, therefore, a serious consideration of the political thought of Rousseau is warranted. I want to begin such a consideration here through an inquiry into the relationship between Rousseau's understanding of human nature and his political theory.

The relationship between Rousseau's discussion of human nature and history in his *Discourse on Inequality*, or *Second Discourse*, and his explicitly political thought as presented especially in his *Social Contract* is a vexed issue in the scholarship of his thought. Why does he write the *Social Contract* as if he had never written the *Second Discourse*? More specifically, why does what he describes as the core of his thought as a whole—the natural goodness of man, and the personal dependence which is the primary cause of our degeneration from good to evil beings—seem to be virtually absent from the most comprehensive presentation of his political thought? This issue can be rephrased to bear on the development of political theory after Rousseau and to address our need to understand how we have arrived where we are today.

Readers of Western political thought since the time of Hobbes have become accustomed to the concept of the *state of nature*. Hobbes portrays the state of nature to make us squarely face ourselves, giving us a correct understanding of our nature and its bearing for the proper erection of the state. Similarly, Locke made the state of nature central to his theory, in particular using it to show the origin of property in appropriation and the need to secure it and ourselves through the formation of a political society. In other words, Hobbes, Locke, and their successors used the state of nature at the same time as a juridical concept and a hermeneutic to explore human nature, two issues that are inextricably related in their thought. Rousseau, however, offers not one state of nature but two. In the part 1 of the *Second Discourse* he describes our original or natural condition through a portrait of savage man's basically solitary existence in the *pure state of nature*. In the part 2 he discusses the *state of nature* simply—the prepolitical condition eventually ended by the social contract. The state of nature in the second sense, shorn of almost everything except its status as a legal concept, is what Rousseau brings to the *Social Contract*, thus splitting his inquiry into human nature from his political theory. The relevance of the pure state of nature and our natural goodness is unclear in Rousseau's thought—a question raised by Voltaire when he quipped that Rousseau's work made him want to walk on all fours, though he regretted that his ill health prevented him from doing so (cited in Rousseau 1959–69, vol. 3, p. 1379).

The bifurcation of questions about human nature and concepts of justice and law apparent in Rousseau's work has infected political thought to this day. This split has been exacerbated by Rousseau's introduction into political thought of the notion of humanity as a historical phenomenon. He summarizes the lesson he would have the reader of the *Second Discourse* take from the "immense space" between the two states of nature he offers: "the human race of one age" is not "the human race of another" (1964, 178). We are historical beings. His pronouncement has radically affected the place of the social contract in political thought in particular and the character of political thought in general. The concept of the state of nature was sublated into the dialectic of history and the history of philosophy or dismissed as a wrong-headed hermeneutic, a quaint or even reactionary chimera, as when Marx wrote: "Let us not go back to a fictitious primordial condition. . . . We proceed from an economic fact *of the present*" (1964, 107). Even with the waning of the historical schools, we are left with an almost universal explicit or implicit acceptance of historicism, a heritage traceable to Rousseau. Where it survives as a concept in political thought, the state of nature remains a mere hypothetical legal starting point, independent of any elaboration of the truth about human nature (e.g., Rawls 1971). In sum, contemporary political theory is generally characterized by a reluctance to inquire into human nature and an emphasis on questions of justice, fairness, procedure, and the like detached from any explicit theory of our nature.

If questions about human nature and such concepts of political theory as justice and law are discussed separately today, the appearance of their separation in the political theory of Rousseau is deceptive. Rousseau's political thought and his constructive philosophy in general are founded upon his understanding of our original nature as seen in his portrait of natural man's existence in the pure state of nature. By examining how Rousseau's legalistic political theory is related to his understanding of human nature, I hope to throw light on how these questions of politics are inseparable from those of human nature at the point in the history of political thought where they seem to become separated. Whatever the validity of Rousseau's own understanding of human nature, he forces us to rethink how the essential questions of political theory are related.

I will argue that the bearing of Rousseau's pure state of nature for his political thought can be seen by examining how he conceives his demonstration that we are naturally "good" to be a justification of nature, or (as he explained his work to Voltaire) a justification of God and his providence (1959–69, vol. 4, p. 1061)—in short, how the *Second Discourse* is intended to be a theodicy. The core of Rousseau's theodicy is his conception that we are by nature physical beings embedded unproblematically in the physical whole of nature. By nature, we are good, or ordered, beings in a good, or ordered, whole. Rousseau's portrait of our original position as good beings in a good natural whole serves (especially in contrast to Hobbes and Locke) as a positive formal model to enable us to remake our corrupted existence through the legitimate state, itself modeled by Rousseau on the divine or natural whole.

ROUSSEAU'S METHOD IN THE *SECOND DISCOURSE*

The announced subject of the *Second Discourse* is the origin and foundations of inequality among men. At the outset of the work Rousseau asks, "How can the source of inequality among men be known unless one begins by knowing men themselves? And how will man manage to see himself as nature formed him, through all the changes that the sequence of time and things must have produced in his original constitution, and to separate what he gets from his own stock from what circumstances and his progress have added to or changed in his primitive state?" (1964, 91). In order to see man as nature formed him, therefore, Rousseau must strip away the changes produced by our "progress." The *Second Discourse* takes the form of a history of mankind from our original condition to our present state.

Rousseau claims that his account in the *Second Discourse* is "conjectural" or "hypothetical." The character of his hypotheses about our original condition and subsequent development is a difficult and contentious subject. Rousseau's intentions regarding the historical status of his account are not, however, relevant for the present discussion because of how his historical and psychological investigations in the work are related. For example, after revealing that the "human race of one age" is not "the human race of another," he explains how the attentive reader will come to this conclusion: "He will explain how the soul and human passions, altering imperceptibly, change their nature so to speak; why our needs and our pleasures change their objects in the long run; why, original man vanishing by degrees, society no longer offers to the eyes of the wise man anything except an assemblage of artificial men and factitious passions which are the work of all these new relations and have no true foundation in nature" (1964, 178). The *Second Discourse* is not essentially or only a historical account, but is most importantly a psychological inquiry into human nature as it may be found universally through proper investigation.[1]

History, for Rousseau, is the successive conditions that decisively shape what the individual comes to be with his fellows. We are historical beings in the sense that the extent and direction of the development of the individual are determined by the extent and character of his relations with his fellows, (i.e., the particular condition into which he is born), especially how those relations and conditions determine the character and extent of his needs, passions, and reason. Rousseau uses a revealing image to elucidate the manner of his search at the outset of the *Second*

Discourse: "Like the statue of Glaucus, which time, sea, and storms had so disfigured that it looked less like a god than a wild beast, the human soul, altered in the bosom of society . . . has, so to speak, changed its appearance to the point of being nearly unrecognizable" (1964, 91). We change "in the bosom of society"; society dramatically (and perhaps irreversibly for the individual) changes what "man" appears to be. We must remove the "encrustations" of society in order to see our true form. Rousseau's description of our original nature and condition and the conditions of our subsequent development is an account not only of what we were and have become but of what we still are. In other words, every child begins life at the point as natural man and would develop into that being apart from the conditions that will actually cause him to develop differently. This is the premise of Rousseau's *Emile*. "Emile is not a savage to be relegated to the desert," Rousseau writes: "He is a savage made to inhabit cities" (1979a, 205). The genetic account of the *Emile* and the historical account of the *Second Discourse* are compatible and complementary (see Starobinski 1964, lv; idem 1988, 12–21).

Rousseau's explanation of how we are historical beings is related to his criticism of his predecessors and indicates the method he uses in his work. He claims that "the philosophers who have examined the foundations of society have all felt the necessity of going back to the state of nature, but none of them has reached it." They have not "found" the state of nature because they have not hypothesized that human nature is actually radically different from what it appears to be: "All of them, finally, speaking of need, avarice, oppressions, desires, and pride, have carried over to the state of nature ideas they had acquired in society: they spoke about savage man and they described civil man" (p. 102). Rousseau claims to be the first to have correctly seen human nature. Speaking of himself in the *Dialogues*, he writes:

> The prejudices that did not subjugate him, the factitious passions to which he was not prey did not hide from his eyes as they did from others those original traits so generally forgotten or misjudged. These traits so novel for us and so true once they are traced could still find, deep in people's hearts, the attestation of their correctness, but they would never have sought them out themselves if the historian of nature had not started by removing the rust that hid them. (1990a, 213–4)

Those who have failed to see themselves, then, have failed to remove the "rust" that has obscured their true nature. They have taken appearance for reality: "They all seek their happiness in appearances, none is concerned about reality. They all place their being in appearance" (*ibid.*).

Rousseau maintains that previous thinkers failed to recognize that "the human race of one age" is not "the human race of another," that "the soul and human passions, altering imperceptibly, change their nature so to speak." By looking at men as we see them around us, we only see individuals that are the product of certain conditions found at a certain time. This is the error made by Hobbes, who claimed that "whosoever looketh into himself . . . he shall thereby read and know, what are the thoughts, and Passions of all other men," so that his state of nature is an "Inference, made from the passions" (*Leviathan*, intro., chap. 13). Despite an apparently similarity of method, however, Rousseau and Hobbes come to drastically different conclusions. In an unpublished writing, the so-called *État de guerre*, he groups his predecessors together, the natural law theorists along with the philosophers of the state of nature, and explains that they all make the same methodological error in their use of the "analytic method." The "error of Hobbes and the philosophers" is to have confounded "natural man with the men that they have before their eyes, and of transporting into one system a being that can subsist only in another" (1959–69, vol. 3, p. 611–12). As Rousseau warned a critic of the *Second Discourse*, "Men differ so much according to times and places that . . . inferences from the particular to the universal are liable to lead to quite contradictory and inconclusive conclusions" (p. 235).

Through his own portrait of savage man's solitary existence in the pure state of nature, then, Rousseau recounts the conditions that will reveal our fundamental nature. His own inquiry into the various causes that shape human nature leads him even so far as to wonder whether any of the faculties taken by his predecessors to be distinctively human are in fact constitutive of mankind.

PHYSICAL MAN AND MORAL MAN

Rousseau claims in the *Second Discourse* that "the philosophers who have examined the foundations of society have all felt the necessity of going back to the state of nature." However, it is not true that "all" the philosophers have felt the need to investigate the state of nature in this way, or even at all. The scholastics and later natural law theorists and jurists, for example, considered the state of nature and even pure state of nature, but not as a necessity in explaining the foundations of society. Only with Hobbes and Locke, most prominently, could such a method be said to have become obligatory for understanding human nature and politics. Rousseau therefore indicates that his true antecedents are Hobbes and Locke, while at the same time, by referring to both the state of nature and pure state of nature, he evokes his more traditional predecessors. Rousseau's transformation of the the terms of the natural law theorists points to how he will challenge the understanding of nature and human nature implicit in perhaps all previous views of human nature.

Aquinas and Suarez—later, Hooker, Grotius, and Pufendorf, among others—all considered the state of nature in one way or another. They conceived of that state as the human condition prior to the Fall (the state of innocence, as opposed to the state of sin and to the state of grace or redemption). They also sometimes entertained, in addition, a wholly speculative

and hypothetical pure state of nature, or the state of nature without or prior to divine dispensation. Neither this strictly conjectural pure state of nature nor the lost state of nature as conceived by these thinkers could provide much practical guidance for political society, precisely because these conditions were viewed as either hypothetical (i.e., conflicting with the scriptural account) or irretrievably lost because of man's Fall. The principal role of the hypotheses about these two conditions in these theories, then, was to elucidate the character of the universally available and applicable natural law.[2]

Rousseau indicates his awareness of the background of the concept of the state of nature when he writes, "It did not even enter the minds of most of our philosophers to doubt that the state of nature had existed, even though it is evident from reading the Holy Scripture that the first man . . . was not himself in that state; and that giving the writings of Moses the credence that any Christian philosopher owes them, it must be denied that even before the flood men were ever in the pure state of nature." His complaint, however, is not that the philosophers of the state of nature have proceeded in supposed ignorance of or opposition to Scripture, but that they have not pursued their inquiries far enough in that direction: "They spoke about savage man and they described civil man" (1964, 102–3).

Unlike those who first discussed the state of nature and pure state of nature, Rousseau's pure state of nature is not the condition without or prior to divine dispensation but without or prior to any truly social or moral relations among humans. The pure state of nature in the *Second Discourse* is the condition where humans exist as purely physical beings, that is, possessed solely of the instincts, passions, and faculties with which they are endowed by (physical) nature. Humans originally or naturally lack moral needs or passions, that is, needs or passions with particular, conscious regard for their fellow humans. Such needs and passions, Rousseau argues, only develop when individuals begin to have constant relations with their fellow humans. Rousseau defines a moral being as "intelligent, free, and considered in its relations with other beings" (1964, 94). The natural man whom he describes in the pure state of nature is manifestly not a moral being according to his definition: while he may be said to be "free" in the sense that he enjoys "natural freedom" (p. 127)—or what Rousseau equates with "an unlimited right to everything that tempts him and that he can get" (*Social Contract* 1.8), he is not "intelligent" nor does he have "relations with other beings" or the passions associated with such relations.

The distinction between the physical and the moral in general is intimated by Rousseau at the outset of the *Second Discourse* in a way that presages the structure of the work as a whole: "I conceive of two sorts of inequality in the human species: one, which I call natural, or physical, because it is established by nature and consists in the difference of ages, health, bodily strength, and qualities of mind or soul; the other, which may be called moral or political inequality, because it depends upon a sort of convention and is established, or at least authorized, by the consent of men" (1964, 101). The natural, or physical, being of man consists of attributes "established by nature" in the individual, including both bodily and psychological attributes. In part 1 Rousseau speaks of man in the pure state of nature as a physical being with solely the endowments of physical nature. The moral, in turn, seems to be divided into two sorts: that which is moral in the sense "established" by "a sort of convention" as men begin to associate together and another, more "political" form that is moral in the sense of being "authorized, by the consent of man." In part 2 Rousseau shows individuals coming together and becoming moral beings in the first sense, eventually to become political beings through the convention of the social contract (hence moral in the second sense; see *Social Contract* 1.8). The distinction Rousseau draws between physical or natural inequality and moral inequality is therefore founded on his understanding of the way our passions and faculties change character as we develop from physical to moral beings: "The same study of original man, of his true needs, and of the principles underlying his duties, is also the only good means one could use to remove those crowd of difficulties which present themselves concerning the origin of moral inequality" (1964, 96). The apparent historical structure of the *Second Discourse* with its progress from the pure state of nature to the state of nature to the civil state parallels and obscures a more fundamental argument about human nature and the (historical) conditions that shape it. The dichotomy between the physical and the moral runs throughout Rousseau's writings and is, I believe, the most fundamental argument about human nature in the *Second Discourse*.

While today we may not be so sensitive to the radical character of Rousseau's argument that we are by our original nature merely physical beings, the novelty and importance of Rousseau's argument about human nature was not lost on his contemporaries, as is made especially clear by one of the more orthodox readers of the *Second Discourse*, the Abbé Castel. Castel, an acquaintance of Rousseau's, was best known for his translations of Newton, his own work on optics, and his "ocular clavicord," which Rousseau ridicules in his *Essay on the Origin of Languages*. A year after the appearance of Rousseau's *Second Discourse*, Castel published a reply to the work, *L'homme moral opposé à l'homme physique de M. Rousseau*.[3]

As the title of Castel's work indicates, he takes particular exception to Rousseau's distinction between our physical and moral being and virtual glorification of the physical. He accuses Rousseau of seeing only the animal in man and not the image of God. He asks Rousseau at the outset, "Do you thus regard man as a wholly physical being?" and later calls him "the apologist of bestialness" (1782, 129, 182). "Nothing proves better that you harm religion,"

he admonishes Rousseau, "than that you take as positive the state of your solitary and animal savage as being the state of primitive innocence, as being the state itself of felicity itself and a terrestrial paradise" (pp. 144–45, 263–65). He contrasts Rousseau's solitary being in the pure state of nature with the social and familial being of Scripture and the natural law theorists ("It is not good for man to be alone," Genesis 2:18). He especially objects to the "brutality" of Rousseau's discussion of human sexuality and claims that the distinction Rousseau draws between physical and moral love clearly shows how he has misunderstood human nature (pp. 136, 159–89, 316–24). Finally, Castel perceptively observes that Rousseau's portrayal of man as a physical being that is somehow good is connected to his statement in the *Second Discourse* about "the little care taken by nature to bring men together." Far from "justifying" nature, as Rousseau claims to do through his work, Castel claims that his misdirected thesis about our physical nature as solitary and somehow good beings "calumniates nature itself, and God too" (p. 167).

From Rousseau's perspective, all of his predecessors have overestimated the needs, passions, and rational capacity natural to us. More importantly, they have assumed that we are naturally social or moral beings in one way or another—that we have social and moral passions and faculties. In other words, his predecessors have all assumed humans to be moral beings in the qualified sense he employs. Rousseau offers a radical reinterpretation of human nature through his portrait of natural man as a physical phenomenon in the pure state of nature. He presents a new understanding of our place in the natural whole that will, he claims, prove that we are naturally good, that our ills come from ourselves, and that nature is thereby justified.

NATURAL MAN AS PHYSICAL PHENOMENON IN THE PURE STATE OF NATURE

The Character of the Natural Whole

Rousseau begins his account of the pure state of nature with a description of natural man. Stripping him of "all the supernatural gifts he could have received and of all the artificial faculties he could only have acquired by long progress—considering him, in a word, as he must have come from the hands of nature," he finds "an animal": "I see him satisfying his hunger under an oak, quenching his thirst at the first stream, finding his bed at the foot of the same tree that furnished his meal; and therewith his needs are satisfied" (1964, 105). From the very outset of his portrayal of our natural condition, Rousseau shows us to exist peacefully and unproblematically in the abode of nature.

Rousseau's first portrait of our natural condition reveals a hospitable nature in which our needs are readily satisfied: "The earth, abandoned to its natural fertility and covered by immense forests never mutilated by the axe, offers at every step storehouses and shelters to animals of all species" (1964, 105). Nature and human intervention or art are here opposed: nature provides sufficient food and shelter for man and the other animals, while the axe "mutilates." Rousseau adds a note to his description of nature's plenty suggesting that it is Locke, in particular, whom he is opposing: "the fruits of trees furnish animals with more abundant food than other forms of vegetation can: an experiment I made myself, in comparing the products of two fields equal in size and quality, the one covered with chestnut trees and the other sown with wheat" (p. 187). This experiment appears to be derived from a comparison suggested by Locke to prove precisely the opposite point about natural fertility versus human cultivation. In England, Locke says, an acre of cultivated land "that bears here Twenty Bushels of Wheat" so that "the Benefit Mankind receives . . . in a Year, is worth £5" can be compared to the produce of the same acreage of comparable uncultivated land in America, the produce of which he says would be "possibly not worth a Penny; at least, I may truly say, not 1/1000 the value of the cultivated land" (*Second Treatise* 5. par. 43). Whereas Rousseau suggests nature's plenty and the destructive character of man's artful intervention, Locke implies a parsimonious nature that must be cultivated by us if it is be of any use. The full significance of Locke's argument about our place in nature and relation to nature's God—and thus of Rousseau's response to it—can be gleaned from a closer examination of Locke's theory of property as presented in the *Second Treatise*.

Locke initially speaks of the "Plenty God had given" us and his example of that plenty is the "Acorns he pickt up under an Oak." Acorns are Locke's first and archetypical example of natural appropriation of property by labor, in this case the eating of what is originally common, thereby making it intimately private.[4] Locke's state of nature seems at first to be one of provident plenty, but this impression is soon undermined. Acorns become Locke's primary example not only of man's natural right to appropriate the goods of nature through labor but of his absolute need to do so: they are representative of the scanty provision of nature. The initial appropriation of acorns as food is only the first sort of necessary labor. We must earn our bread by the sweat of our brows not because of any divine punishment but because of the character of the divine or natural whole. Locke uses the example of America given in his comparison of cultivated and uncultivated land: although nature has furnished the Americans "as liberally as any other people, with the materials of Plenty," because they have not cultivated the land, "a King of a large and fruitful Territory there feeds, lodges, and is clad worse than a day Labourer in *England*." He also recurs to the example of acorns as the "Plenty" given to these Americans by nature: "Acorns, Water, and Leaves, or Skins, must be our Bread, Drink and Clothing, did not *labour* furnish us

with these [latter,] more useful Commodities. For whatever Bread is more worth than Acorns, . . . that is wholly *owing to labour*"; for "*labour makes the far greatest part of the value* of things, we enjoy in this World," since "Nature and the Earth furnished only the almost worthless Materials, as in themselves" (*Second Treatise* 5; see Pangle 1988). Locke's portrait of our unaltered place in nature and his emphasis on the benefits of appropriation or human art increasingly becomes an indictment of the providence of nature and nature's God. Rousseau's description of natural man "satisfying his hunger under an oak," in turn, is a vindication of the providence of nature.[5] In going back to the pure state of nature, Rousseau argues that Locke has misunderstood the character of the natural whole within which we exist.

More important than his argument about the character of the natural whole, however (for Locke allows that nature's provisions may be sufficient for man's bare needs), Rousseau objects that Locke and others have underestimated our ability to provide for ourselves in our natural condition and (most significantly) overestimated and mistaken the natural needs and passions for which we must provide: "Let us therefore take care not to confuse savage man with the men we have before our eyes" (1964, 111). Rousseau even imagines a fight between these two and argues that it would be tremendously unequal. This comparison is not only meant to support his argument that savage man can survive in his solitary existence in the pure state of nature but to point to a more fundamental difference between these beings. He concludes the comparison by explaining that savage man is an animal who (especially as compared to civil man) has "the advantage of always carrying oneself, so to speak, entirely with one" (p. 107). Savage man is an entirety, a being who lives as a physical whole in the natural whole. Rousseau explains the difference between civil and savage man similarly in *Emile*. Civil man, he says, "is only a fractional unity" whose "value is determined by his relation to the whole, which is the social body," while savage man "is entirely for himself. He is a numerical unity, the absolute whole" (1979a, 39–40).

Natural man is a purely physical being, and his relations with his fellows are limited to the physical. He discerns only "physical good and evil" and makes comparisons of "force" in his encounters with other beings (Rousseau 1964, 107). His fellow humans are equivalent in his mind to physical objects. Only when he develops moral passions, notably pride (*amour-propre*), do such encounters become more than purely "natural events" (p. 222). Until then he makes no moral judgments based on either intentionality or acquired notions of "the useful, the suitable, and the good" (see Rousseau 1979a, 39, 166–67, 213). Throughout his account, then, Rousseau argues that humans are limited by nature to needs for which they can easily provide and lack the needs and passions for which they might need others—moral needs and passions. His argument about the character of human needs, passions, and reason is the crux of his argument about human nature and his quarrel with his predecessors.

Human Needs, Passions, and Reason

Rousseau presents his account of the character and extent of our natural faculties and passions in explicit disagreement with Hobbes. He makes his criticism by utilizing the same basic understanding of the dynamics of human psychology that Hobbes himself employed against his predecessors—most importantly, the ancients and the natural law theorists. His argument with Hobbes about the extent and character of our natural faculties and passions points to their different understandings of our nature and place in nature.

In Hobbes's account of the state of nature, man's asocial and self-regarding passions make that condition a state of war not only in the juridical sense of lacking a superior to judge or of being "without a common Power to keep them all in awe" but also in the more tangible sense of living in an uncertain and violent condition of "continuall feare, and danger of violent death; And the life of man, solitary, poore, nasty, brutish, and short" (*Leviathan* 13). Humans are asocial in the sense that each individual looks primarily to his own self-preservation. This asociality results in a contest over scarce natural resources for both present and future consumption or enjoyment. Foresight and the related quest for power to obtain goods makes all resources, in principle, scarce. Hobbes concludes, "I put for a generall inclination of all mankind, a perpetuall and restlesse desire of Power after power, that ceaseth only in death" (chap. 11). Men are also self-regarding: they have a "mutual will of hurting" arising from both fear and vainglory. There are thus three main sources of quarrels: competition, diffidence, and glory (scarcity, fear, and pride). The very passions and faculties that make the state of nature incommodious Hobbes points to as separating humans from the beasts: he describes man variously as the sole animal with *vanity*, especially in contending for preeminence in reason; *speech* in the sense of a conventional art of words; and both *reason* and the natural *passion of curiosity*—foresight about causes and effects in general and thus the ability to develop Science (chaps. 2–6, 12, 27). The faculties Hobbes points to as distinctive of humans obviously require social relations. When he claims that humans are naturally asocial, then, Hobbes does not deny that they live in constant, if unstable, relations with their fellows. He means that individuals are naturally selfish and self-regarding, or prideful (see *De cive* 1.2, addendum). Indeed, Hobbes's description of the state of nature seems to presuppose that we normally or naturally live in some sort of families and social groups (see *Leviathan* 20). By pushing Hobbes's argument about our natural asociality even further, Rousseau comes to deny that we possess the speech or reason that Hobbes believes is not only natural to humans but distinctive of them.

Rousseau first introduces his quarrel with Hobbes

when he imagines the "unequal fight" between a savage and a civil man: "Hobbes claims that man is naturally intrepid and seeks only to attack and fight"—against which, Rousseau cites Cumberland and an "illustrious philosopher" (Montesquieu) that "nothing is so timid as man in the state of nature" (1964, 106–7).[6] He suggests in a note to this passage that Hobbes has mistaken savage man for civil man because he has incorrectly understood the natural state of the human psyche: "All knowledge that requires reflection, all knowledge acquired only by the linking of ideas and perfected only successively, seems to be altogether beyond the reach of savage man for want of communication with his fellow men—that is to say, for want of the instrument which is used for that communication and for want of the needs which make it necessary" (pp. 188–89). As a being who carries himself entirely with himself, savage man does not require the assistance of his fellow humans physically, emotionally, or otherwise. His thorough integrity is based most importantly on his lack of the "needs" that lead to association with his fellow humans in the first place.

Rousseau develops his argument against Hobbes about the natural state of our minds by adopting the general theory of the dynamics of the mind employed by Hobbes, Locke, and others against their own predecessors. "Whatever the moralists may say about it," Rousseau begins, "human understanding owes much to the passions, which by common agreement also owe much to it" (1964, 115–16). The moralists assume in one way or another that humans are naturally and in some way primarily rational. They argue that we are universally subject to the moral dictates of the natural law as discovered by reason. Following Hobbes, in particular, then, Rousseau criticizes these thinkers for establishing the natural law so that "it is impossible to understand the law of nature and consequently to obey it without being a great reasoner and a profound metaphysician: which means precisely that men must have used, for the establishment of society, enlightenment which only develops with great difficulty and in very few people in the midst of society itself" (p. 94). As we have seen, however, Rousseau's own definition of a moral being is more capacious. He therefore pushes the psychological theory of Hobbes and Locke further, accusing them of failing to realize that humans are not by nature moral beings in any sense.

Rousseau plumbs further the insight of Hobbes and Locke by analyzing the relationships among needs, passion, and reason. First of all, the passions influence reason: "It is by their activity that our reason is perfected; we seek to know only because we desire to have pleasure." These passions, in turn, are based on desires; hence, "it is impossible to conceive why one who had neither desires nor fears would go to the trouble of reasoning." These desires are based on "needs," both natural ("by the simple impulsion of nature") and conventional ("through the ideas one can have of them"). Man is by nature limited to his natural needs: "His desires do not exceed his physical needs: the only goods he knows in the universe are nourishment, a female, and repose; the only evils he fears are pain and hunger. I say pain and not death and its terrors because an animal will never know what it is to die; and knowledge of death is one of the first acquisitions that man has made in moving away from the animal condition" (1964, 115–16). Rousseau's remark about man's lack of any fear of death is directed against Hobbes, who would make the fear of death the most forceful element of human psychology. To his statement that natural man's desires "do not exceed his physical needs," Rousseau attaches a short note that elucidates his disagreement with Hobbes and others:

> That seems totally evident to me, and I am unable to conceive whence our philosophers can derive all the passions they impute to natural man. With the sole exception of the physically necessary, which nature itself demands, all our other needs are such only by habit, having previously not been needs, or by our desires; and one does not desire that which he is not capable of knowing. From which it follows that savage man, desiring only the things he knows and knowing only those things the possession of which is in his power or easily acquired, nothing should be so tranquil as his soul and nothing so limited as his mind. (p. 213)

Rousseau turns Hobbes's reasoning against the moralists against him. "Hobbes saw very clearly the defect of all modern definitions of natural right," Rousseau says, but he "improperly included in the savage man's care of self-preservation the need to satisfy a multitude of passions which are the product of society and which have made laws necessary. . . . Hobbes did not see that the same cause that prevents savages from using their reason, as our jurists claim, prevents them at the same time from abusing their faculties, as he himself claims" (1964, 129). Hobbes and (less so) Locke describe our natural condition as improvident not only because of the scarce provision of natural goods but also because of the rich provision of contentious passions. Rousseau argues, in turn, that our unique "faculty of self-perfection, a faculty which, with the aid of circumstances, successively develops all the others" (p. 114), is activated only through the social or moral relations that arise from the contact with our fellows that we lack in our natural condition; that is, our uniquely malleable needs and passions develop and change their character to become moral needs and passions. As basically solitary beings, humans are devoid of reason, speech, foresight, contentious passions, and pride. The absence of these faculties and passions is what most importantly enables us to live a basically solitary and peaceful existence in our natural condition. By denying that we naturally have these needs, passions, and faculties, Rousseau is again able to describe the pure state of nature as a providential condition.

Sex and Moral Proofs in Matters of Physics

Rousseau cannot deny that humans completely lack needs or passions for one another, for the species depends upon them. Rousseau remarks that among man's "physical needs" is "a female." In the *Second Discourse* Rousseau speaks, at least initially, almost always of "man"—the male.[7] He speaks almost exclusively of males in his description of savage man's existence in the pure state of nature because he wants his account to be as far as possible genderless (as he sees it). "In everything not connected with sex," he claims in *Emile*, woman is man" (1979a, 357). In the *Second Discourse*, he explains that only after the establishment of families was "the first difference was established in the way of life of the two sexes, which until this time had had but one" (1964, 147). Rousseau describes natural man as a male at least in part because males can more easily be conceived of as solitary. When females are considered as females, questions arise concerning the ability of mothers to survive alone while caring for infants, the relatively long period of infancy among humans, and thus the possible need for relatively stable families and perhaps more extended social groups if humans are to survive.

Rousseau's psychological argument is presented in tandem with his characterization of our basically solitary natural existence and may even appear to depend upon it. However, he does not absolutely insist on his claim about our natural solitude. For example, he summarizes his argument about man in his original condition, "Let us conclude that wandering in the forests, without industry, without speech, without domicile, without war and without liaisons, with no need of his fellowmen, likewise with no desire to harm them, *perhaps* never even recognizing anyone individually, savage man, subject to few passions and self-sufficient, had only the sentiments and intellect suited to his state" (1964, 137; emphasis mine; see also p. 119). This passage also reveals that it is less important for Rousseau that his savage be solitary than that whatever minimal relations he might have with his fellow humans do not serve to develop his mental and psychological state. Rousseau's argument is thus not primarily that savage man does lead a solitary existence but that he is capable of doing so because of the character of his faculties and passions (see Masters 1968, 130–36, 172–75).

Rousseau directs his argument that constant relations between men, women, and children are not natural against Locke, who seems to assume the existence of relatively stable families and other social groups to be characteristic of our natural condition. How Rousseau makes his argument further elucidates his conception of the difference between our physical and moral being and of the mistake of Hobbes, Locke, and others.

Rousseau's discussion of the character and extent of sexual and familial relations in the pure state of nature is an outgrowth of his description of the relations among needs, passions, and reason. Reason and speech are intimately related. Languages obviously require some sort of social relations, and the family is the place we most associate with learning to speak. Rousseau thus raises the problem of language in conjunction with his denial of any stable social or familial relations in the state of nature. He offers his own description of life in the pure state of nature: "In the primitive state, having neither houses, nor huts, nor property of any kind, everyone took up his lodging by chance and often for only one night. Males and females united fortuitously, depending on encounter, occasion, and desire, without speech being a very necessary interpreter of the things they had to say to each other; they left each other with the same ease" (1964, 120–21). In a note to this statement, Rousseau transcribes at length a passage from Locke's chapter "Of Political or Civil Society" in the *Second Treatise*.[8] The part of passage to which Rousseau replies in detail pertains to Locke's claim about the nature of our sexual and familial relations, apparently even in the state of nature. Locke argues that the "chief, if not the only, reason" for the longer conjunction of male and female among humans than other animals is "because the female is capable of conceiving, and is commonly with child again and brings forth, too, a new birth, long before the former is out of dependency for support on his parent's help, and able to shift for himself," so that the father, in order "to take care for those he hath begot, and to undertake that care for a long time, is under an obligation to continue in conjugal society with the same woman by whom he has got them, and to remain in that society much longer than other creatures" (Rousseau 1964, 213–14).

Most importantly for the present purpose, Rousseau undermines the assumptions that he claims Locke makes about our natural psychological state: "Locke proves at most that there could well be in a man a motive for remaining attached to a woman when she has a child; but he does not prove at all that he must have been attached to her before the delivery and during the nine months of pregnancy." According to Rousseau, therefore, Locke "evidently supposes what is in question"—not only the existence of some kind of relatively stable society, which would provide the basis for such knowledge and foresight, but the very reason and foresight in the first place. Rousseau states that "this kind of memory, by which one individual gives preference to another for the act of procreation, requires, as I prove in the text, more progress or corruption in human understanding than can be supposed in man in the state of animality in question here" (1964, 218–19).

When he alludes to what he "proves" in the text, Rousseau is referring to the distinction he draws between physical love and moral love somewhat after this note. There he asserts that by nature the sexes have no need for any prolonged association because they have no more than an occasional need to couple in order to preserve the species. Physical love, or simple copulation, is limited to purely physical im-

pulses and relations. Moral love, in turn, is based on reasons and opinions conceived through the imagination; and these only develop in society. Limited to the physical in love, male and female in their natural state meet sporadically and, with their physical needs satisfied and no knowledge of the possible consequences of their act, they return to their basically solitary existence (1964, 134–36).

The distinction Rousseau draws between physical and moral love is part of the more general dichotomy between the physical and the moral in the *Second Discourse*. His allusion to what he "proves" about physical and moral love helps us to understand what he says at the outset of his criticism of Locke in the note: "I shall observe first that moral proofs do not have great force in matters of physics, and that they serve rather to give a reason for existing facts than to prove the real existence of those facts" (1964, 215–16). Once again, Rousseau is accusing Locke of making the same sort of mistake as the natural law theorists, or those whom Rousseau earlier termed the moralists. Assuming humans always to have been the beings he sees around him, Locke has improperly attributed to us faculties and passions that (Rousseau argues) we do not possess by our original nature.

Rousseau reiterates his general criticism of Locke at the very end of the note in a different form:

> Locke's reasoning therefore falls apart, and all the dialectic of the philosopher has not saved him from the error committed by Hobbes and others. They had to explain a fact of the state of nature, that is to say, of a state where men lived isolated and where a given man has no motive for living near another given man, nor perhaps to live near one another, which is much worse; and they did not think of carrying themselves back beyond the centuries of society, that is to say, of those times when men have always had a reason to live near one another, and when a given man often has a reason for living beside a man or a given woman. (1964, 219–20)

Despite his suggestion that Locke and Hobbes have made a historical mistake, not "carrying themselves back beyond the centuries of society," Rousseau's argument against Locke's understanding of man's *psychological* state is evident, as well. When he criticizes Locke for not going "back" far enough, he indicates that Locke has not properly conceived of how different conditions (conditions that tend to develop in a certain general direction over time) affect the character and extent of man's "needs." The change that takes place in man's "need" for his fellow human, especially that for a particular member of the opposite sex, is crucial. (Note the change from two given men in the first state to a "given man" and a "given woman" in the second.) In other words, Rousseau denies that humans by nature have what he calls moral needs or passions—needs and passions with particular, conscious regard for other individuals, of which moral love is his archetypical example.

Rousseau's argument about moral proofs in matters of physics can be generalized and applied to his criticism of the views of Hobbes and Locke (among others) about mankind's relation to the natural whole within which they exist. The state of nature as envisioned by Hobbes and Locke hides within it a cryptoteleology. Supposing that because of the character of nature or because of the passions natural to humans the state of nature is inimical to us, their state of nature is paradoxically teleological in that its insufficiencies point to another condition that is in a sense higher—the end of the state of nature through the artifice of the social contract. Rousseau, in contrast, describes how we can survive as purely physical beings in our natural state, as good beings that exist unproblematically in nature and that need never have developed out of that condition.

THE NATURAL GOODNESS OF MAN

Rousseau claims that he has demonstrated that man is naturally good and that he has thereby justified nature (1964, 193). He later explains that the natural goodness of man is central to his system of thought, as when he exposed the "fundamental principle" of all of his writings, namely, "that man is naturally good, loving justice and order, that there is no original perversity in the human heart, and that the first movements of nature are always right" (1959–69, vol. 4, pp. 935–36).[9] Rousseau's demonstration of the natural goodness of man in the *Second Discourse* consists in his portrait of savage man's natural condition in the pure state of nature as a physical being embedded in (physical) nature. Before turning to the meaning of his claim to have thus justified nature and even God, let me take up what Rousseau means by our natural goodness.

We usually brand attributions of goodness as moral judgments—even as matters of "value" distinct from those of "fact." Rousseau, however, does not use *good* in a moral sense and even takes it in a sense compatible with matters of physics. Rousseau's conception of goodness becomes clear when, after his description of the pure state of nature, he wonders what kind of mortality we could be said to possess by nature:

> It seems at first that men in that state, not having amongst themselves any moral relationship or known duties, could be neither good nor evil, and had neither vices nor virtues: unless, taking these words in a physical sense, one calls vices in the individual the qualities that can harm his own preservation, and virtues the those that can contribute to it; in which case, it would be necessary to call the most virtuous the one who least resists the simple impulses of nature. (1964, 128)

Although Rousseau immediately claims that he will speak "without departing from the ordinary meaning" of these words, it is clear, since he understands natural man to be a purely physical being, that he also means us to take these terms in the "physical sense" he suggests.

An indication of Rousseau's novel use of these terms is seen by the objections of the Abbé Castel. He complains that Rousseau's savage has "nothing of the moral, of human, of theological or divine; nor any

more of duties, morals, relations; nor any more of good, that is to say, of bad. For here is the system of M. Rousseau well explained in detail and well announced: according to him, the good is bad and the bad is good" (1782, 182–83). Similarly, Diderot, appears to have been thinking along the same lines in his article "Droit naturel" for the *Encyclopedia*, which has been read as a reaction to Rousseau's arguments in the *Second Discourse* "If man is not free or if his instantaneous determinations, or even his oscillations, arise from something material that is external to his soul, he will have in them neither reasoned goodness nor wickedness, even though they may have in them animal goodness or wickedness" (1975, vol. 7, p. 25; see Masters 1968, 261–65).

We are good, says Rousseau, in that we do not resist "the simple impulses of nature." Through our spontaneous instinct for self-preservation alone and perhaps also restrained by natural pity, we are by nature good for ourselves (well-ordered and self-sufficient) and good, or at least not harmful, for others (see Melzer 1990, 15–17; see also Masters 1968, 151–57). We are good by the "two principles anterior to reason"—the desire for self-preservation and pity—which Rousseau explains he has found "by meditating on the first and simplest operations of the human soul" (1964, 95). Together they compose the "maxim of natural goodness": "Do what is good for you with the least possible harm to others" (p. 133).[10] We do not by nature need to force ourselves, through reason or otherwise, to act in a manner that is good. Such force, whether external force applied through education or threat or internal "self-forcing," Rousseau is fond of pointing out, is found in the original sense of *virtue* (see Rousseau 1979a, 444–45). He therefore reserves the terms *virtue* and *vice* for moral beings, that is, humans with the needs and passions that develop in and through their relations with their fellow humans and that cause them to be good neither for themselves nor others.

Rousseau makes his argument about the natural goodness of man in disagreement with Hobbes: "Above all, let us not conclude with Hobbes that because man has no idea of goodness he is naturally evil; that he is vicious because he does not know virtue; . . . nor that, by virtue of the right he reasonably claims to things he needs, he foolishly imagines himself to be the sole proprietor of the whole universe." Rousseau thus concludes, "One could say that savages are not evil precisely because they do not know what it is to be good: for it is neither the growth of enlightenment nor the restraint of law, but the calm of passions and the ignorance of vice which prevents them from doing evil" (1964, 128–30). Ironically, then, the Abbé Castel claims that even Hobbes is "not impious, in that he supposes man capable of impiety. Man having himself neither virtues nor vices, nor moral relations, nor known duties, could not be impious, whatever he should do, not being more than a brute beast and animal" (1782, 171–72). As a number of modern interpreters of Rousseau have noted, his savage man's goodness is amoral, premoral, or even subhuman (e.g., Masters 1968, 152; Starobinski 1964, lxiv; Strauss 1953, 271).

THE THEODICY OF THE *SECOND DISCOURSE*

Rousseau claims that through his portrait of our original nature as asocial, physical, and good beings that he has "justified" nature and even God and his providence. He raises the issue of providence in the *Second Discourse* just before alerting us to his use of *good* in a physical sense. He explains that "from the little care taken by nature to bring men together from mutual needs and to facilitate their use of speech, one at least sees how little is prepared their sociability, and how little it contributed to everything men have done to establish social bonds." Thus Rousseau, the Abbé Castel claims, calumniates God and nature; but Rousseau maintains that nature has thus been kind to us: we are placed in this natural condition "by a very wise providence." To the argument that such a being would be miserable, he says, "Now I would really like someone to explain to me what type of misery there can be for a free being whose heart is at peace and whose body is healthy?" (1964, 126–28). Rousseau reiterates the soundness of man's physical endowments, both mental and bodily, and the harmony in which he lives as an unproblematic whole in the natural whole.

Rousseau treats the question of divine or natural providence in his 1756 letter to Voltaire in response to the latter's poem on the Lisbon earthquake. How can human misery and death be consistent with a beneficent God or well-ordered nature? Rousseau points to the argument in the *Second Discourse*, a work Voltaire had characterized as a "work against the human race." No one, Voltaire wrote, had ever used so much intelligence to make man a beast (quoted in Rousseau 1959–69, vol. 3, 1379). Rousseau's answer is simple: "If it is better for us to be than not to be, this would be enough to justify our existence, even though we would have no compensation to expect for the ills that we have to suffer." Voltaire and others, he says, are wrong to accuse God or nature because "in the comparison of goods and evils," they "always forget the sweet sentiment of existence" (ibid., vol. 4, pp. 1062–63; see Rousseau 1979b, 68–69). In the *Second Discourse*, Rousseau describes the psychological condition of natural man in the pure state of nature: "His soul agitated by nothing, is given over to the sole sentiment of its present existence" (1964, 117). The providence of nature and nature's God exists for us in our natural condition as physical beings unproblematically embedded in the physical whole of nature.

In his letter to Voltaire, Rousseau explains that God's providence is impersonal, "general," and wholly physical in character, that is, not specifically concerned with the individual: "It is to be believed that particular events are nothing here below in the eyes of the master of the universe, that his provi-

dence is only universal, that he is content to preserve the genera and the species and to preside over the whole without being disturbed by the manner in which each individual spends this brief life" (1959–69, vol. 4, p. 1069). Thus, even though Rousseau seems to assume a high infant mortality in our natural condition—"Nature . . . renders strong and robust those who are well constituted and makes all the others perish" (1964, 106; see p. 111)—what is more important is the survival of the species: "The proof that all is well ordered derives from a general and incontestable fact: that all the species survive" (1959–69, vol. 3, p. 237). What is most important in considering the providence of nature or nature's God is the order of the whole. In almost equating goodness with order, both in his discussion of the natural goodness of man and the goodness of nature as a whole, Rousseau appeals to a traditional theological concept. In the "Profession of Faith of the Savoyard Vicar" in *Emile*, for example, he has the vicar explain that "the love of order which produces order is called *goodness;* and the love of order which preserves order is called *justice*" (1979a, 282; emphasis original). He alludes to this same conception in the *Second Discourse* when he writes that men do not naturally have the reason to conceive of "maxims of justice and reason drawn from the love of order in general or from the known will of his creator" (1964, 119). The order of nature, including human nature, is found in our original or natural condition as physical beings embedded unproblematically in the physical whole of nature. We are thus a good part of a good nature: "The first movements of nature are always good and right" (1959–69, vol. 4, pp. 935–36; see Rousseau 1979a, 92).

Previous interpreters who have attended to Rousseau's understanding of his work as a theodicy have come to a different conclusion. Rather than understanding Rousseau's conception of our natural goodness and his justification of nature or God as referring to our original position in nature, they have seen his solution to the problem of evil and providence as eschatological: as coming at the end as a restitution of order. This view gains credence from the argument about providence made by Rousseau's Savoyard vicar. Chief among these interpreters is Ernst Cassirer. Cassirer correctly sees that Rousseau's unique solution to the problem of evil (society as the cause of our ills) is central to his thought. However, he incorrectly believes that Rousseau's resolution of the problem of evil is found in our ultimate reformation in the ethical society where the categorical imperative (the general will) reigns. For Rousseau, he asserts, the goodness of man "was not an original quality of feeling but a fundamental orientation and a fundamental destiny of man's will" and was grounded "in man's capacity for self-determination . . . in the recognition of an ethical law" (Cassirer 1989, 104–5; see also Cassirer 1951, 147, 153–58 and Hendel 1934). More recently, Andrzej Rapaczynski (1987) has made a similar argument, finding in the vicar's eschatological providentialism a clue to Rousseau's thoughts about the human problem.

Cassirer and others have erred, first, because they have misunderstood or misrepresented Rousseau's conception of our historical nature. Seeing in Rousseau the predecessor to Kant, Hegel, and even Marx, all of whom in different ways viewed providence as a historical phenomenon (see Kant 1983, 53–55), Cassirer and others' interpretations of Rousseau have a distorted, proleptic character. Rousseau does not view history primarily as the process of our perfection (for he is decidedly pessimistic about our "progress") but the addition of encrustations on our original nature through changes in the determinants of the extent and character of our malleable passions and faculties, our historical being. He reveals our original condition and our historical nature so that we may see that our ills are not inevitable, that we are good, and that nature is justified. His portrait of nature and of human nature is useful not as a focus of nostalgia but as a model for the conditions of our happiness, in accordance with which it is possible to manipulate the determinants of our nature.

Second, they have erred because their interpretations are based, in part, upon a mistaken assumption about Rousseau's intentions in offering the teaching about providence he has the Savoyard vicar offer and the relationship of that teaching to his own understanding. The vicar argues that an eschatological and restitutive moral providence is the solution to the problem of disorder and evil among men (1979a, 278–82). Cassirer and others have seized upon the vicar's argument as substantially Rousseau's own. The exact relationship between Rousseau and his vicar is an extremely difficult and much-debated subject. However, Rousseau's remark in his letter to Voltaire that our existence is justified "even though we would have no compensation to expect for the ills that we have to suffer" (i.e., even without a doctrine of compensation from a caring deity) indicates that he offers a strictly natural justification of nature and providence.[11] Finally, Rousseau's disagreement with the vicar about the "end" at which providence is found or justified is attested to by the sole reference to the "Profession of Faith" elsewhere in *Emile*. In a passage that explicitly recalls the *Second Discourse* (where he also states that Hobbes called the wicked man "a robust child," p. 129), Rousseau explains, against Hobbes, that children are naturally good: "All wickedness comes from weakness. . . . Of all the attributes of the all-powerful divinity, goodness is the one without which we can least conceive it . . . . See hereafter the Profession of Faith of the Savoyard Vicar" (1979a, 67). The vicar states that "the supremely good Being, because He is supremely powerful, ought also to be supremely just" and goes on to relate goodness with order and justice with the preservation of order. The context of the reference earlier in the work points to Rousseau's solution of the problem of evil and understanding of providence as lying in our beginning as a good part of a good nature.

The notion of providence seems to require an intelligent and willing being and thus to be misapplied to nature. Just as his quarrel with Hobbes shows how Rousseau could speak of goodness in a physical sense, so it reveals what sense it makes to speak of natural "providence." This can be seen by contrasting Rousseau's view of human nature and our place in nature with Hobbes's.

Hobbes discusses the issue of providence and man's place in nature and relation to God as the "author of nature" in *Leviathan* 31, "Of the Kingdome of God by Nature." Hobbes notes the metaphorical application of the term *providence* to the "Power of God" as the "Author of Nature." Nonetheless, he proceeds to consider "the Right of Nature, whereby God reigneth over men, and punisheth those that break his Lawes," which he reduces to God's "Irresistible Power." Central here is Hobbes's reduction of right to power, evident earlier in the work in his discussion of natural right (chaps. 14, 21; see also Spinoza, *Theologico-Political Treatise* 6, 16). Rewards and punishments are central to every conception of providence, most especially the rewards and punishments of an afterlife thought necessary to resolve the problem of evil and justify God and his providence: "Punishments are consequent to the breach of Lawes; Natural Punishments must be necessarily consequent to the breach of the lawes of Nature; and therefore follow them as their naturall, not arbitrary effects" (*Leviathan* 31). However, with Hobbes's essential reduction of natural law to a "general rule of reason" or prudence, and incorporation of natural law into natural right (chap. 14), natural rewards and punishments become equivalent to simple pleasure and pain as events in the "chayn of Consequences" of our actions or as consequences of our motion simply (chap. 31). For Hobbes, however, our natural condition is a state of war of every man against every man because of the passions and faculties natural to us. Though intended to procure pleasure or at least avoid pain, our actions produce pain and even death. Our natural condition is not a providential one in any positive sense. Rather than ordered beings in an ordered whole, Hobbes finds beings whose natural impulses lead them to disorder and death.[12]

Like Hobbes, Rousseau's conception of natural providence is metaphorical. Unlike Hobbes, however, natural providence has a positive character. For Rousseau, our natural passions and needs lead to our survival and even flourishing. Only when we depart from nature with our fellows out of the hands of nature's providence does a process begin that makes "a being evil while making him sociable, and from such a distant origin finally bring man and the world to the point where we see them" (1964, 140). Or, as Rousseau explains of his system, all of his writings have been founded on "the great principle that nature made men happy and good, and that society depraves him and makes him miserable" (1990a, 213). Rousseau's portrait of our natural, providential condition in the pure state of nature serves as a positive model for the remaking of our miserable existence.

THE BEARING OF ROUSSEAU'S PURE STATE OF NATURE

The state of nature constitutes what might be called a negative standard in the political theories of Hobbes and Locke. The state of nature is no longer a hypothetical prelapsarian state of innocence created by God and lost by men as in the scriptural and natural law traditions that they confronted, but a state of want and war. The state of nature in their theories is far from the creation of a provident Deity; it is scanty in its physical provision and rich in its provision of quarrelsome human passions. Hobbes in particular replaces the reasoned duties and ends of the natural law theorists with the solid and sublime beginnings of the passions, especially the fear of pain and death. Reason serves as the guide—the star and compass—to the ends determined by the passions. Right—each individual's natural right to self-preservation and the means to it—is prior to duty. But thus, there appear to be no true ends in themselves: the most important end is paradoxically the beginning point of *the* end—death. Life becomes characterized by a desire to maintain motion; the most important goal is to avoid the end of motion.

The negative standard of the state of nature that may be said to be characteristic of the thought of Hobbes and Locke affects the bearing of liberal thought. The primacy of natural right along with the state of nature with which it is frequently allied might be said to provide no positive standard for politics.[13] They provide to the natural individual a sort of fence that cannot be crossed or a way of judging the relationship between the individual and the state. They do not tell us what sort of political entity to construct except (for the most part) to say what cannot be done. They do not offer any, or much, advice about what sort of life the citizens of the state should lead or what type of beings they should be. There is in liberal thought and its emphasis on natural right a tendency, as in Hobbes, to provide for the continuation of motion without giving that motion any determinate direction or end. The existence of the individual is characterized by the *pursuit* of happiness, while happiness itself remains indeterminate.

Rousseau shares much with his predecessors in the liberal tradition of the state of nature and the social contract. For example, his state of nature also eventually becomes a condition of want and war that must be overcome through art or convention. However, he goes further back to the pure state of nature, a new revelation of what sort of beings we truly are by nature. His natural man is a physical being who exists unproblematically as a good being embedded in a good nature. He is a being characterized by wholeness. Rousseau's pure state of nature and our place

within it are positive standards, of sorts, for his constructive philosophy, which is concerned not only with life, liberty, and the pursuit of happiness but with happiness itself. He envisions the palliation or reconstruction of our corrupted existence by remaking our malleable or "perfectible" nature by imitating our natural condition as whole beings embedded unproblematically in the whole of nature—albeit on different foundations from those of our physical existence in the pure state of nature. Thus, although Rousseau calls the savage societies of the state of nature the "happiest and most durable epoch" for mankind (1964, 151) and although some interpreters have recognized how those primitive societies serve as a model for at least a portion of Rousseau's constructive thought (e.g., Shklar 1985), still, even that community with its relatively whole or unified beings living in a whole shares a formal similarity with our natural existence in the pure state of nature.

That Rousseau views the existence of natural man as a positive standard has, of course, often been noted. So far, however, interpreters have concentrated on the unity of soul of the individual alone as Rousseau's standard (Melzer 1990; see also Strauss 1953). They have failed to understand how his view of our place in nature in general serves as a positive standard, a theodicy. This criticism applies to Starobinski's (1988) somewhat similar interpretation; for his reading of Rousseau as concerned with a "transparent" relationship of an individual with his fellows and surroundings and the problem of the "obstruction" in that relationship is at bottom a radically individualistic reading of Rousseau (as should be evident from his psychoanalytic approach to Rousseau to begin with). Only the present interpretation can fully account for both the unity of intention and similarity of method behind the various solutions Rousseau offers in his constructive philosophy. He entertains three distinct and ultimately problematic possibilities of our "perfectibility," all in some sense imitations of the savage man's existence in the pure state of nature: the solitary walker (*Reveries of the Solitary Walker*), the retreat of the domestic society (*Emile* and *Julie, ou la nouvelle Héloïse*), and the legitimate political association (*Social Contract*). Let me say something about Rousseau's political solution for our ills.

THE PURE STATE OF NATURE AND ROUSSEAU'S POLITICAL PHILOSOPHY

Rousseau's political teaching in the *Social Contract* seems to be elaborated independently of the psychological and historical investigation of the *Second Discourse* and, indeed, of the main tenets of his system of the natural goodness of man. Thus, even where the historical and psychological arguments appear in his explicitly political work, they are phrased narrowly: the social contract becomes necessary when the obstacles to self-preservation become too great (e.g., *Social Contract* 1.6). However, as Arthur Melzer (1990) has perceptively argued in his recent work, the cast of Rousseau's argument is misleading. Most importantly, Rousseau can be seen to be concerned in the *Social Contract* to alleviate, among other things, the main cause of our evils as we have developed into moral beings, namely, personal dependence. "Man is born free, and everywhere he is in chains. One who believes himself the master of others is nonetheless a greater slave than they," Rousseau announces in his opening words. Even mastery is a form of slavery because of personal dependence. Although his paradoxical statement that "whoever refuses to obey the general will shall be constrained to do so by the entire [political] body; which means only that he will be forced to be free," is infamous, its continuation is less well known: "For this is the condition that . . . guarantees him against all personal dependence" (1.8). As Melzer notes, Rousseau can present his political teaching in the *Social Contract* as though it were separate from the main tenets of his system and based on the narrow problem of self-preservation, because "the political requirements of preservation and happiness perfectly coincide" (1990, 114–15). Building on Melzer's interpretation, I shall add that in his political thought, Rousseau intends to solve the problem of personal dependence by making the state an imitation of the divine or natural whole.

Rousseau would palliate the ills from personal dependence by making the citizens of his regime as far as possible independent of one another and dependent upon the laws alone. The laws ought to make the relationship of the citizens to one another "as small as possible" and the relationship of the citizens to the state and its laws "as large as possible" (*Social Contract* 2.12). The independent civil freedom of the citizens in Rousseau's state is an imitation of the independent natural freedom of the savage in the pure state of nature (see 1.8, 2.11). Rousseau intimates in *Emile* how the relationship of the citizens to the political whole within which they live is intended to be an imitation of savage man's existence in the pure state of nature:

> Dependence on things, since it has no morality, is in no way detrimental to freedom and engenders no vices. Dependence on men, since it is without order, engenders all the vices, and by it, master and slave are mutually corrupted. If there is any means of remedying this ill in society, it is to substitute law for man and to arm the general wills with a real strength superior to the action of every particular will. If the laws of nations could, like those of nature, have an inflexibility that no human force could ever conquer, dependence on men would then become dependence on things again; in the republic all the advantages of the natural state would be united with those of the civil state, and freedom which keeps man exempt from vices would be joined to the morality which raises him to virtue. (1979a, 85)

In the first draft of the *Social Contract* and in a parallel passage in his essay "Political Economy," Rousseau explains, in his discussion of "the marvels of the laws," that we must "imitate here on earth the

immutable decrees of the divinity." "It is this celestial voice" of the laws, he explains, "that tells each citizen the precepts of public reason, and teaches him to behave according to the maxims of his own judgment and not to be constantly in contradiction with himself" (1978, 177–78).

The laws of Rousseau's political whole are to be modeled on the laws of the divine or natural whole; and our relationship to them is similar except that in the case of the civil state, the citizens themselves may legislate them through our general will as sovereign. The parallel between the political whole and the divine or natural whole in Rousseau's conception is further strengthened when we consider that the origin of the general will, the source of the laws, as a theological term concerning the divine will and providence, a heritage most thoroughly investigated by Patrick Riley (1986). The "voice of the people is the voice of God," Rousseau exclaims in his "Political Economy" (1978, 213; see Rousseau 1959–69, vol. 3, pp. 862–63), giving the traditional formula a new and radical meaning.

CONCLUSION

I began by wondering about the relationship between Rousseau's description of human nature through his depiction of our natural condition in the pure state of nature and his political thought. Despite certain appearances to the contrary, his juridical political theory and the legitimate state he derives from it, far from having been developed in isolation from his demonstration of the natural goodness of man, depends decisively upon it. Rousseau's projected solutions to our ills, including his political remedy, are formally modeled on the natural condition of the human race as a good part of a good nature—as we can see when we understand that Rousseau intends the *Second Discourse* to be a theodicy.

The connection between Rousseau's reflections on human nature and his political theory force us to reconsider how these subjects may be inseparable, even at the point in the history of political thought where they appear to diverge. Indeed, since Rousseau's understanding of human nature is bound up with his conception of nature and questions about the divine, he offers an opportunity to consider a return to the grand philosophical tradition in politics. As our liberal politics and political science triumph, it is imperative that we come to understand their foundations better, build on them more solidly, assess their possible structural limitations, and grasp their possibilities.

Notes

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1. Throughout this paper I will discount the quasi-Lamarckian theory of the changes in our physical constitution through time that Rousseau seems to contemplate in the *Second Discourse* but puts aside (1964, 104–5, 183–86, 203–9). In any case, if Rousseau puts forward his hypotheses about human history in general and changes in our constitution in particular in a scientific sense (as I believe he does), the thesis that I will maintain is the main one of the *Second Discourse*, namely, that we are by nature physical beings, would only be strengthened.

2. For background on the state of nature and the pure state of nature, see Gierke 1950, 96–105 and notes; Gourevitch 1988, 25–33; Strauss 1953, 184–85. For the original formulations, see Aquinas, *Summa theologica* I–II. 109.8, as well as 102. 3–12, 109. 2–4; Suarez, *De legibus ac Deo legislatore* 1.3.11–12, 2.8.8–9; Hooker, *Of the Laws of Ecclesiastical Polity* 1.10.1; Grotius, *De jure belli ac pacis* 2.5.15, 3.7.1; Pufendorf, *De officio hominis et civis* 2.1.1–7; idem *De jure naturæ et gentium* 2.2.1–4.

3. Castel's work was originally published in Toulouse in 1756 as *L'homme morale opposé à l'homme physique; ou Réfutation du discours sur l'origine de l'inégalité: Lettres philosophiques.* It is included in a supplemental volume (no. 29) to the 1782 *Collection complètes des oeuvres de J.-J. Rousseau* edited by Moultou and Peyrou, to which I refer here. My attention was first drawn to Castel by Hamilton 1975, 86–89.

4. Locke perhaps chooses acorns as his representative example of property by appropriation because of Pufendorf's parallel use of acorns in his theory of property deriving from the universal consensus of mankind (*De jure naturae et gentium* 4.4.13). This parallel was drawn to my attention by Peter Laslett (Locke 1988, 288n.), who, in turn, takes it from John Gough (1950, 78n.). The centrality of the oak and acorn to descriptions of man's primitive state is also to be found in Lucretius: "Their hearts were content to accept as a gift what the sun and showers had given, and the earth had produced spontaneously. Often they stayed their hunger among the acorn-laden trees" (*De rerum natura* 5.937–40). It may have been Lucretius who suggested this image to Pufendorf, who quotes this passage from Lucretius at length in his *De jure naturae et gentium* (2.2.2). It has often been noted that Rousseau's depiction of man's natural state is modeled on the fifth book of Lucretius' poem (e.g., Morel 1909; Strauss 1953, 264, 271); and among the many parallels between the accounts is this picture of man resting beneath an oak. However, although Lucretius and the other Epicureans, like Rousseau, deny natural teleology, their account of man is not wholly antiteleological: unlike Rousseau, they locate our natural condition not in our beginnings but in our fulfillment, or *summum bonum,* which is not found in our beginnings (see Melzer 1990, 122). Rousseau thus understands the Epicureans to "deny all providence" (1959–69, vol. 3, p. 46n.).

5. I shall use the metaphor of "providence" when analyzing nature in Rousseau's understanding, for this is what his claim in the *Second Discourse* to have "justified" nature must mean. The account in the *Second Discourse* is a strictly natural one, hence Rousseau does not explicitly discuss God or divine providence in the work. In his 1756 letter to Voltaire, which I shall discuss, he extends his claim about the *Second Discourse* and says that he has justified God and divine providence in some way, as well.

6. By citing Montesquieu against Hobbes, Rousseau might be said to exploit a breach in Hobbes's theory of the state of nature opened but not fully exploited by Montesquieu. In the first book of his *Spirit of the Laws,* Montesquieu takes exception to Hobbes's characterization of man's polemic nature: "But one feels that what can happen to men only after the establishment of societies, which induced them to find motives for attacking others and for defending themselves, is attributed to them before that establishment." By Hobbes's premises about the primacy of the desire for self-preservation, Montesquieu says, one should, instead, see that man is extremely diffident, not quarrelsome, in his natural state (1.2;

see 30.19). Rousseau's complaint against Hobbes and Locke is similar. Montesquieu does not, however, provide an explanation of how man developed from that condition to the one described by Hobbes and acknowledged by Montesquieu as later (though not originally) correct. Rousseau may be said to offer such an explanation in the *Second Discourse*. His explanation is both historical (How did diffident and basically solitary men come together in the savage societies that eventually decline into a condition of war?) and, more importantly, psychological (How did man change from a timid and wholly asocial being to a contentious and other-regarding being?).

7. This apparently highly gender-specific description of the savage has been noted by a number of readers and received different reactions. Some have chastised Rousseau for taking the male as somehow representative of humanity and excoriated him for taking the female to be the deficient sex, in particular, in his statements about the education of Sophie in *Emile* 5 (e.g., Keohane 1980). Others applaud him for what is seen to be an ultimately genderless description of savage humans and accuse him of inconsistency in his discussion of the differences and inequalities between the sexes after they have developed (e.g., Okin 1979, 108–11). Without attempting here to defend or accuse Rousseau in these matters, it should be noted that his analysis of the differences between the sexes relies on his understanding of the interaction between nature and convention, especially of how conventional differences between men and women may or may not be founded on natural distinctions brought out only in unnatural conditions. In other words, Rousseau's discussion of human sexuality, whatever its ultimate validity, centers around issues still in contention.

8. Rousseau transcribes an anonymous French translation of Locke's *Second Treatise* 7. pars. 79–80. The translation is fairly close to the original and will be followed here as translated by Masters in Rousseau 1964.

9. While Rousseau claims that together his works form a "system," he is not a "systematic" philosopher in the sense, say, of such seventeenth-century thinkers as Leibniz or Malebranche. He distinguishes between the "systematic spirit" and the "spirit of observation": the former is characterized by the tendency to construct systems from abstract and universal rules and hasty overgeneralizations; the latter, to which he adheres, entails careful empirical observation and cautious generalizations (1959–69, vol. 4, pp. 1090–93). Rousseau's discussion of theodicy and providence is therefore, quite different from that of, Leibniz, for example, as Rousseau himself implies (see vol. 3, pp. 232–34). Whereas Leibniz might be said to begin from questions of metaphysics and even logic, reasoning deductively from a priori attributes of the divinity (e.g., in his *Theodicy*), Rousseau characteristically begins with the human problem and our very real ills. For the character of Rousseau's system, see Lanson 1912, 7; Machado 1968, 33–36, 83–85; Melzer 1990, 9–12; Starobinski 1988, 40–41.

10. While pity serves as the basis for human morality, it does not itself appear to be a moral sentiment or passion in Rousseau's understanding. There is some question to what extent Rousseau believed that pity would be active among the savage men of the pure state of nature. For at least reasons of exposition, the activity of pity he suggests in the *Second Discourse* (1964, 131–33) seems significantly different from his account in the *Essai sur l'origine des langues* (1990b, chap. 9) and *Emile* (1978, 221–23), where he denies its initial activity, although I would argue that close examination of his argument in the *Second Discourse* would remove this apparent contradiction. Finally, and in a manner that may explain both why pity is not a moral sentiment and the ambiguities of his presentation, Rousseau suggests in *Emile* that all the passions, apparently including pity, can be derived from the natural sentiment of self-love (1979a, 212–13).

11. I show at length elsewhere through a close analysis of his letter to Voltaire, the "Profession of Faith," and other writings that Rousseau offers a strictly natural account of the whole and our place in it (Scott 1992). Rousseau puts forward his arguments about an immortal soul and a caring Deity with particular moral providence as at best doubtful and indicates that he does not ultimately believe them. He presents these arguments as useful and even essential for the virtue and happiness of most individuals and all political communities. This is the spirit in which he discusses civil religion in the *Social Contract* (4.8).

12. Hobbes has a parallel discussion in *De cive* 15. Pufendorf locates divine providence in our being led to sociability through our natural reason and natural law (*De officio hominis et civis* 1.3.10–11).

13. This cannot be said without qualification (for at least Locke), however; for the natural freedom he shows us to enjoy in the state of nature is the model for his description of civil freedom. This is true of Montesquieu, as well (*Spirit of the Laws* 11.3–5). Rousseau's understanding of the relationship between natural and civil freedom is similar (*Social Contract* 1.8).

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POSTMODERNISM AND THE PUBLIC SPHERE

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The idea of the public sphere, of an institutionalized arena of discursive interaction, is central to democratic theory and practice. The modern age has, however, witnessed the erosion of a public realm distinct from the state and the market. In response to this erosion, public realm theory, notably the work of Arendt and Habermas, attempts to theorize the minimal conditions necessary for a discursive realm free of structural coercion or manipulation. The resulting normative conception of the public sphere has come under sharp attack by postmodern theorists, including Foucault, Lyotard, and Baudrillard, who question the basic presuppositions of public realm theory. I examine their objections and show how the public realm theory of Arendt can be viewed as motivated by concerns similar to the postmoderns'. Against Habermas, I argue that Arendt's public realm theory is less concerned with the question of legitimation than with the theorization of an agonistic political subjectivity.

As a motto, "the recovery of the public realm" captures, more or less adequately, the primary goal of Hannah Arendt's political philosophy and the critical theory of Jürgen Habermas. Both Arendt and Habermas see the public sphere as a specifically political space distinct from the state and the economy, an institutionally bounded discursive arena that is home to citizen debate, deliberation, agreement, and action. Yet they also see this sphere as overwhelmed by the antipolitical forces unleashed by modernity. For Arendt, the modern rise of "the social" effaces what was once a strong distinction between the public and the private and tears down the boundary between the realms of necessity and freedom (1957, 33–49). The result is that the public sphere is "devoured" by "household" concerns—the maintenance of life and economic reproduction—and politics is reduced to the function of "household administration," a function fulfilled by the state (pp. 45–47).

For Habermas, modernity is essentially an epoch of rationalization, as Weber described it. Yet this rationalization is one-sided. The universalization of instrumental rationality—its application to all spheres of social life—converts practical/political questions into technical ones (Habermas 1970, 96). The result is the destruction of the space of democratic decision making, a realm now "colonized" by technical–administrative imperatives (Benhabib 1986, chap. 7; Habermas 1985, 1989).

Reacting to these trends and the withdrawal of the political in the modern age, Arendt and Habermas refined their respective conceptions of the public sphere in order to provide rigorous criteria for determining a *genuine* public realm. By insisting upon the distinctiveness of this realm and the kind of interaction that occurs within it, Arendt and Habermas are able to elucidate the necessary minimal conditions for (to use Arendt's phrase) "the sharing of words and deeds"; that is, by theorizing the conditions necessary for uncoerced deliberation and decision amongst diverse equals, Arendt and Habermas provide an ideal of the public sphere that serves to highlight the forms of asymmetry, coercion, violence, and communicative distortion that presently characterize *our* public realm.

I shall examine Arendt's and Habermas' related conceptions of the public realm and postmodern theory's skeptical response to them. Increasingly, poststructuralist, or postmodern, theorists have raised objections to the fundamental presuppositions of the *type* of project Arendt and Habermas are engaged in. I shall examine what I consider to be the three most basic and forceful objections raised by poststructuralism/postmodernism against Arendt and Habermas. The first, originating in the work of Foucault, I shall refer to as the *power* objection, which radically questions the idea(l) of a coercion-free space of deliberation by retheorizing the nature of power in the modern age (Foucault 1977, 1980a, 1980b, 1982). The second objection, the *epistemological*, finds its most forceful formulation in the work of Lyotard, who challenges the very possibility of a unified, consensus-based public realm in an age that has witnessed the death of legitimating metanarratives and the corresponding fragmentation of the discursive realm into irreducibly heterogeneous language games (1984, 1985, 1988). The third objection, the *ontological*, in some ways the most telling, can be derived from the work of Deleuze or the popularization of certain Deleuzian themes in Baudrillard (Baudrillard 1983; Baudrillard 1988, chaps. 5, 7; Deleuze 1980). This objection questions the peculiar reality attributed to the public realm as a "common space of appearances" (Arendt), a *world* without transcendental or metaphysical support yet available to all its citizens/inhabitants. From the point of view of Deleuze or Baudrillard, the Arendtian conviction that "appearance . . . constitutes reality" betrays a "nostalgia for reality," for a world well lost (Arendt 1957, 57; Baudrillard 1988, chap. 7). This nostalgia appears to permeate Arendt's entire project (the analysis of modernity in terms of "worldlessness"), and Haber-

mas is able to evade it only by offering an abstractly procedural account of discursive rationality.

While these objections are powerful, I believe they are also answerable, at least from the point of view of Arendt's project. Indeed, I will argue that the apparently clear opposition between Arendt and Habermas on the one hand and poststructuralism/postmodernism on the other has been overdrawn thanks, in large part, to the polemical stance taken by Habermas in *The Philosophical Discourse of Modernity* (1987). In that work, Habermas creates a stark opposition between communicative and subject-centered reason, between the paradigms of mutual understanding or intersubjectivity and that of the philosophy of consciousness (chap. 11). In his view, neither Hegel nor Marx succeeded in extricating themselves from the "horizon of the self-reference of the knowing and acting subject"; but then, neither do such critics of the philosophy of consciousness and the modern project as Heidegger, Derrida, and Foucault. All remain, from the Habermasian perspective, either caught up in the metaphysics of subjectivity (Hegel and Marx with their demiurgic conceptions of self-externalizing subjects) or endlessly tracing the transcendental/empirical bounds of the "humanist" paradigm (Heidegger, Derrida, Foucault). According to Habermas, the postmoderns, like their predecessors, fail to effect the transition to "the paradigm of mutual understanding" and remain locked within an exhausted episteme (pp. 295–96, 310).

This mapping of the world of modern/postmodern theory neatly locates Arendt's work on the intersubjective side of the divide. I shall question the adequacy of this mapping, suggesting that the Arendtian theorization of the public realm harmonizes in unexpected ways with the critics of the strong normative conception of the public sphere. This invites a rethinking of the relation between public realm theory and postmodernism/poststructuralism (see my earlier discussion, Villa 1992).

ARENDT, HABERMAS, AND THE NORMATIVE CONCEPTION OF THE PUBLIC SPHERE

In his appreciation of Hannah Arendt's contribution to the theorization of power, Habermas emphasizes how she "dissociates the concept of power from the teleological model of action" (1983, 173). Contra Max Weber, who "defined power as the possibility of forcing one's own will, whatever it maybe, on the conduct of others," Habermas argues that Arendt "understands power as the capacity to agree in uncoerced communication on some community action" (p. 171). Habermas is referring to Arendt's "On Violence," where she draws a rigorous distinction between *power*, (defined as "the human ability not just to act, but to act in concert") and strength, force, and violence—all of which are uniquely instrumental (Arendt 1968b, 143). This definition points to a model of action quite different from the teleological one, whose means–end structure makes the achievement of a pregiven goal the organizing principle of all action. In contrast, Arendt understands action as "the sharing of words and deeds" among equals. In Habermas' formulation, action is essentially *communicative*. On *this* model—the communicative versus the teleological or instrumental—"the parties are oriented toward agreement and not just toward their respective success" (1983, 173). *Political* power—the power to act together, to act in concert—is generated through the common action of individuals whose interaction is structured in accordance with norms of reciprocity, respect, and equality and for whom agreement or consensus stands "as an end in itself for all parties" (p. 175).

This reading of Arendt's theory of action and concept of power stresses the parallels between her reassertion of the Aristotelian distinction between praxis and *poiesis* and Habermas' fundamental distinction between communicative and instrumental action. It focuses upon the dialogical dimension of Arendt's understanding of politics and asks us to view her notion of the public sphere as one that sets the basic conditions for what Habermas refers to as "coercion-free communication." This is important; for unless political action as communication *is* coercion-free, the consensus that issues from it will have no binding force: it will simply be another expression of insidious or overt violence. Habermas sees the delineation of the public sphere presented in Arendt's *Human Condition* as a phenomenologically inspired attempt to "read off the general structures of unimpaired intersubjectivity in the formal properties of communicative action or *praxis*" (Habermas 1983, 175). Unsurprisingly, Habermas interprets Arendt's conception of the public realm as a prefiguration of his own "ideal speech situation"—the counterfactual elucidation of the formal conditions of coercion-free communication, abstracted (so he claims) from the very structure of communicative action (1987, 315). Theory functions, in both cases, to uncover the gap between current practice and the implicit telos of communicative interaction, the reaching of a "rationally motivated agreement" (p. 315).

While I shall argue that the "consensus" model of politics Habermas attributes to Arendt is problematic, one must acknowledge that Habermas does capture an important dimension of Arendt's theorization of the public realm, as I have argued elsewhere (Villa 1992). In *The Human Condition* and *On Revolution* Arendt consistently emphasizes the idea of the public realm as a space of "tangible freedom"—a space that is physically separate from those of work and labor, bounded or constituted by law, and that can serve as an arena or stage for political action. Not only coercion and violence but all relations of hierarchy or command are excluded from this sphere. The public realm is the space of freedom precisely because it creates an artificial *equality* among persons whose individual talents make them naturally unequal and because it preserves the human condition of *plurality*

by relating and separating persons simultaneously. The public realm is that artificial "world" common to us as citizens, an "in-between" that makes both equality and individuality possible (1957, 7, 26, 52; 1963, 31–32).

Here we encounter the seemingly irreducible *spatiality* of the public realm as conceived by Arendt. Unless this sphere has the power to gather us together, to remove each of us from the private while simultaneously guaranteeing each of us a distinct *place* in this "common world," genuine politics is not possible. Where this sphere is invaded by matters and ends that are not essentially open to debate, plurality is effectively neutered and the public realm loses its three-dimensionality: the "world" ceases to be constituted from a variety of perspectives (1957, 55; see Villa 1992, 286–87). Where this space is forcibly collapsed or eliminated (as Arendt argues it is under totalitarianism), the "boundaries and channels of communication between individual men" are replaced by "a band of iron which holds them so tightly together that it is as though their plurality had disappeared into One Man of gigantic dimensions" (Arendt 1968c, 164). The space between individuals is destroyed, with the result that action is no longer possible. So long as the spatial dimension of the public realm is preserved and the "boundaries and channels of communication" remain intact, freedom in the distinctively Arendtian sense is possible.

Richard Bernstein has argued that Arendt's depiction of the public realm in terms of plurality and equality results in a normative conception of politics that is virtually indistinguishable from that of Habermas (1983, 182–223; 1986, 221–37). Plurality and equality open a space of deliberation oriented toward mutual agreement in which the "force of the better argument" (Habermas) can carry the day. Bernstein glosses Arendt's claim that "debate is the very essence of political life" by offering, as a characterization of what happens in the Arendtian public space, that politics

> involves "no rule," the mutual and joint action grounded in human plurality and the isonomy of citizens where individuals debate and attempt to persuade each other. Persuasion, not force or violence, is the quintessence of political life. Persuasion is not the manipulation of others by image-making. Persuasion involves free, open debate among equals in which we seek to form, clarify and test opinions. (1986, 224)

This gloss has the effect of identifying Arendt's public realm with an arena of communicative rationality in which opinion is gradually purified through the "non-coercively unifying, consensus-building force of a discourse in which participants overcome their at first subjectively biased views in favor of a rationally motivated agreement" (Habermas 1987, 315). While Arendt formally eschews Habermas' moral cognitivism, Bernstein points out that this reading finds support in her Kantian theory of "impartial" political judgment (1983, 216–23; 1986, 228–31).

If Bernstein reveals a fundamental continuity between the Arendtian and Habermasian conceptions of the public realm, Seyla Benhabib reinforces that assimilation by arguing that Arendt's concept of plurality must be seen in opposition to the Hegelian–Marxian model of a "supersubject" externalizing and reappropriating itself, a model that reduces plurality to transsubjectivity (1986, 243–44). The phenomenon of plurality, the basic constitutive condition of politics, is covered over by the "work model of action"—Hegel's and Marx's version of the traditional teleological concept. By emphasizing intersubjectivity and the communicative conception of action, the phenomenological ground of politics is recovered: "The communicative model of action does justice to the experiences of human plurality, . . . for communication is the medium through which plurality is revealed. In acting and speaking we show who we are and our differences from each other" (p. 244). The public realm, understood as a discursive space characterized by symmetry, nonhierarchy and reciprocity, both presupposes, and makes possible, plurality and so provides the opportunity for a politics based on mutual recognition and respect for difference.[1]

Postmodern Objections to the Normative Conception of the Public Realm

The account of public realm theory so far offered dwells on the similarities between Arendt and Habermas. In turning to postmodern objections to this type of theory, I shall continue to ignore, for the time being, the substantial differences between them.

Foucault's work can be understood as a sustained critique of public realm theory and its normative presuppositions. As has been widely noted, Foucault's theorization of modern disciplinary power begins with what Nancy Fraser has called a "bracketing of the problematic of legitimacy" (1989, 19; Taylor 1984). By setting aside the fundamental liberal distinction between legitimate and illegitimate power, Foucault brings to light the "local, continuous, productive, capillary and exhaustive" character of modern power (Fraser 1989, 32; see Foucault 1980b, 93–108, 119). Power's "tightly knit grid of material coercions"—its various microtechniques for the production of docile bodies and self-surveilling subjects—remain invisible so long as we treat it as sovereignty (Foucault 1980b, 105). Yet it is just this anachronistic model of state power based on contract and intended for the preservation of rights that liberal theory asserts again and again by its imposition of the legitimacy problematic. In so doing, the liberal paradigm—the "juridical apparatus"—conceals the increasingly insidious forms of modern discipline and domination (Foucault 1980a, 81–91).

Neither Habermas nor Arendt relies upon the discourse of rights to make their critical points, nor can either be accused of holding a "sovereign model" of power. Both point to the possibility of a positive—as opposed to a merely negative or repressive—power, a power that arises from communicative in-

teraction, from "acting together, acting in concert" (Arendt 1968b, 143; 1968c, 172).[2] The negative or repressive aspects are located elsewhere, and identified with strength, violence, hierarchy, and so on. However, from the Foucauldian point of view, this positive conception of power is too clean. It is anachronistic, a democratic analogue to the state-centered liberal model. Both models of power—the positive one proposed by public realm theory and the negative one upheld by liberalism—blind us to the constitutive workings of modern power and its fundamental role in the production of subjects. Thus, the liberal "bounding" of negative/repressive state power through the notion of rights finds its mirror image in public realm theory's specification of the necessary formal conditions for "unforced agreement." In other words, the public realm theory of Arendt and Habermas simply shifts the "power-free zone" of liberalism (power understood as legitimate coercion) from the private realm to the public, redefining power now as to the "ability to act." Indeed, insofar as the public realm is purified of subjective/strategic interests, it is more rigorously coercion-free than the "power-free" private sphere of liberalism. In both conceptions, however, the error is to pretend that we can use power without its using us.

The severe limitations of public realm theory in coming to grips with the nature of modern power are most apparent in its naive reliance upon conditions of symmetry, nonhierarchy, and reciprocity as adequate guarantees of a "coercion-free" space. As Fraser points out, in a "fully 'panopticized' society, hierarchial, asymmetrical domination of some persons by others would have become superfluous; all would surveil and police themselves" (1989, 49; see Dews 1987, 157, 160–61). Foucault enables us to see how hierarchy and asymmetry (the two primary villains according to public realm theory) are not essential to the smooth functioning of disciplinary power. The formation of autonomous individuals—their subjectification—is not so easily detached from their subjugation (Foucault 1977, 29–30; 1980b, 98). Thus, while it is possible to imagine the realization of the ideal speech situation, this would be nothing more than the achievement of "pseudoautonomy in the conditions of pseudosymmetry" (Fraser 1989, 49).[3] A Foucauldian would therefore insist that public realm theory deal with the normalizing character of communicative action. The criterion of consensus is too blunt an instrument for this task, since it leaves unexamined the self-surveillance of the civically virtuous citizen (who has internalized the hegemonic conception of the public good) or communicatively rational agent (who has internalized the hegemonic conception of what constitutes "the better argument").[4]

The second type of postmodernist objection to the normative conception of the public sphere also focuses upon the ideal of consensus—but from an epistemological, as opposed to a power, perspective. Habermas' strategy for rescuing practical/political questions from technocratic usurpation—for preserving the public realm from system colonization—can be reduced to two basic theoretical moves. The first, which draws on Arendt's distinction between acting and making, involves distinguishing between two types of rationalization in modernity: system and communicative rationalization (Habermas 1970, 92–97; Habermas 1985; Wellmer 1985, 35–66). This distinction enables us to view the "disenchantment" of practical discourse in the modern age as a process separate from, yet parallel to, the growth of purposive rationality: *communicative* rationalization consists in the growing endorsement of the principle that validity claims can and ought to be redeemed only through the medium of reflexive speech. The reference to traditional grounds of validation, which presume a prediscursive legitimacy, is no longer possible.

The appeal to communicative rationality (to legitimation through open dialogue and consensus) works, however, only if Habermas is able to convince us that this rationality is universal (and so able to guarantee the possibility of reaching a *genuine* agreement) yet nonsubstantive. Otherwise, he is open to the Nietzschean/Weberian objection that any noninstrumental concept of reason is dogmatic. Habermas' deployment of what he believes is a philosophically viable procedural account of rationality enables him (so he claims) to take full account of the "disenchantment of the world" and the modern separation of value spheres, while avoiding the Nietzschean or Weberian conclusion that will or decision is the only possible source of meaning in a disillusioned age (Benhabib 1986, 258–263; Habermas 1973, chap. 7). A proceduralist account of discursive rationality enables escape from the polytheism of values—Weber's "warring gods"—that characterizes postmodernity. As long as there are standards of argumentative rationality implicit in speech, we will be able to distinguish a genuine consensus from a false one and decisions based on reason from those based upon will or coercion—right from might.

Lyotard, in particular, is sympathetic to the Habermasian attempt to escape the universalization of functionalist reason, the legitimation of knowledge by power. Indeed, his analysis of the principle of "performativity" in *The Postmodern Condition* mirrors much of the standard Frankfurt School critique (1984, xxiv, 47). Nevertheless, he is extremely skeptical of Habermas' attempt to transfer formal rules for what constitutes a genuine consensus from the scientific or theoretical community to the sphere of politics (p. 30).

Lyotard is, first, dubious about the Habermasian picture of the legitimation of scientific discourse (the privileged model of argumentative rationality). "Paralogy," or "permanent revolution," is opposed to consensus (1984, 60–67; Rorty 1985). Second, and more important for my purposes, he points out how Habermas' attempt to avoid an irreversible fragmentation of the public sphere by providing a procedural mechanism that mediates between the "warring gods" and specifies the formal properties of the

"better argument" is, in fact, parasitic upon an Enlightenment metanarrative of emancipation. This metanarrative, Lyotard asserts, *was* capable of integrating the language games of science and politics through its deployment of consensus via open debate as *the* criterion of legitimate knowledge and institutions (1984, 32, xxiv). Yet once this metanarrative comes under suspicion (and, as Lyotard famously defines it, postmodernity *is* "incredulity toward metanarratives"), the idea that a model of argumentative rationality and agreement derived from science can provide context-independent criteria of validity is itself cast in doubt.

The Habermasian end run around decisionism and the "polytheism of values" depends upon the availability of a metadiscourse that could, potentially, render the "heteromorphorous" language games of social life—each with its own context-dependent rules of legitimation, competence, and application—commensurable. Against the assumption that "discourse" holds the key to identifying context-independent criteria of validity, Lyotard states that "there is no reason to think that it would be possible to determine metaprescriptives common to all of these language games or that a reasonable consensus like the one in force at a given moment in the scientific community could embrace the totality of metaprescriptions regulating the totality of statements circulating in the social collectivity" (1984, 47; see also 1985, 58). Habermas cannot conceive of legitimation in terms other than a "search for universal consensus." As a result, he *must* assume "that it is possible for all speakers to come to agreement on which rules or metaprescriptions are universally valid for language games" (Lyotard 1984, 47).

For Lyotard, this assumption reduces the irreducible heterogeneity of language games. It is an attempt to neutralize the "diversity of discursive species," each subject to its own set of pragmatic rules. Moreover, Habermas assumes that "the goal of dialogue is consensus," that "humanity as a collective (universal) subject seeks its common emancipation through the regularization of 'moves' permitted in all language games, and that the legitimacy of any statement resides in its contributing to that emancipation" (ibid:, 65–66). *This* assumption not only does violence to the heterogeneity of language games, it does violence to the heterogeneity of the players—their plurality—by subjecting all to a regime of discursive practice designed to overcome what Lyotard sees as the *agonistic* dimension of those games (p. 10). The spontaneity, initiation, and difference that characterize agonistic speech are repressed as a model of Kuhnian "normal science" is imposed upon the public sphere. Because of this flattening, antiagonistic, antiinitiatory character of the consensus model, Lyotard concludes that "consensus has become an outmoded and suspect value" and opts instead for a "pagan" politics that breaks decisively with all attempts to ground action and practical decision in a theoretical discourse of justice or legitimacy (1984, 66; 1985, 19–43, 28).

From Lyotard's perspective, the fragmentation of the public sphere—the emergence of discordant language games that results from the postmodern death of totalizing metanarratives—opens the way to forms of political practice and judgment freed from the tyranny of science or *episteme* (1984, 28–29; 1985, 27, 58). "Paganism"—Lyotard's code word for an antifoundationalist politics of opinion, of judgment "without criteria"—rescues the political by unmasking the ideal of a grounded consensus and the theoreticist/Platonist constraints it implies. Lyotard's epistemological rejoinder to the Habermasian construction of the public sphere parallels Foucault's objection in its bracketing of the legitimation problematic. (Lyotard sees this problematic as encompassing any attempt to derive the political from the theoretical, the just from the true, even where "truth" is seen as a function of consensus; see 1985, 25.)[5] Public realm theory conceals the disciplinary underside of "acting together" and covers over the antiagonistic, antiinitiatory implications that flow from the regularization of moves in any language game.

The final objection to public realm theory raised by postmodernism is ontological, so-called because it is directed against the specific reality claimed for the public sphere and the objects within it. It is more directly applicable to Arendt's theory, which presents us with what could be called an ontology of the public realm. Eschewing Habermas' attempt to delineate the public sphere in terms of a specific form of interaction and the rationality appropriate to it, Arendt defines the public phenomenologically, as a "common space of appearances." Plurality has a constitutive, as well as a consensual, dimension. Deliberation and debate from a diversity of perspectives constitutes a "shared world of appearance," a world whose reality is a function of incessant speech about that which lies between us (Arendt 1957, 50–58). From Arendt's point of view, that which "appears in public and can be seen and heard by everybody" is "real." This conviction is expressed in her dictum that "for us, appearance—something that is being seen and heard by others as well as ourselves—constitutes reality" (p. 50).

In making this claim, Arendt is self-consciously aligning herself with Nietzsche's undermining of the Platonic metaphysical distinction between appearance and reality (1977, vol. 1, 10). But where Nietzsche sees this distinction as expressing an ascetic devaluation of life, Arendt sees it as expressing a philosophic hostility to politics. Her desire to "save the phenomena"—her insistence that meaning and reality reside *in* the appearances, not behind or above them—follows Nietzsche's revaluation of the world of becoming (Villa 1992, 287–88). However, Arendt backs away from the conclusion Nietzsche drew when he traced the nihilistic logic set in motion by the institutionalization of the Platonic distinction, the two-thousand-year process by which the "real world becomes a fable": "We have abolished the real world: what world is left? The apparent world perhaps?

. . . But no! with the real world we have abolished the apparent world!" (1979, 41). For Nietzsche, try as we might to divorce it from any transcendental signified, our concept of appearances is irreducibly metaphysical. To presume, as Arendt does, that one can hold onto an "objective," shared world of appearances once all anchors to a transcendental reality are cut loose is, ultimately, to remain entangled in the web of metaphysics.

Arendt believes that the reality of the world, of a common space of appearances, is guaranteed so long as this in-between is the arena for spirited debate and deliberation. Where public spirit and "care for the world" animate a community, Arendt believes, the *sensus communis* remains strong, providing a nontranscendental ground for action and judgment (1957, 208–9, 274–85; 1968a, 221; 1982, 66–72). Such a "common feeling for the world" transcends the local, concretely contextual nature of Aristotelian ethos. At the same time, it makes no claim to providing a cognitive ground for moral/political judgment. Yet this "feeling for the world" is vulnerable, the easy victim of the modern subjectification of the real that Arendt traces in *The Human Condition* (1957, chap. 6, esp. 280–88). Hence the mournful Arendtian leitmotif of modern "worldlessness," the peculiar alienation from the world that accompanies the withering away of "common sense."

Here Deleuze's interpretation of Nietzsche and Baudrillard's theorization of simulacra show us how the "loss of the world" remains trapped within the logocentric/metaphysical logic of the sign. From this perspective, Arendt does not overcome the terms, she merely inverts them. The Arendtian public sphere is a space of genuine appearances, of signs whose play and self-referentiality is restricted by their being "seen and heard by everyone." This implies and preserves an opposition between an authentic, real in-between and its inauthentic, manipulated counterpart—the former both condition and result of action and speech, the latter nothing more than the proliferation of signs/images that only serve further to destroy our feeling for the world. Baudrillard's point is that this way of looking at things (of seeing the "world" as increasingly "dimmed-down" or covered over) does not make a final break with the representational episteme. Postmodernity, on the other hand, has left the "order of appearance" behind: "reality" (including the reality of appearances Arendt wants to preserve) is presently generated as a simulation effect. The "decisive turning point," Baudrillard states, is "the transition from signs which dissimulate something to signs which dissimulate that there is nothing" (1988, 170). Arendt's ontology of the public realm prevents us from seeing just how well and truly lost the world is. Her nostalgia for the "world," for worldliness, is a nostalgia for a common referent—a nostalgia that power is able to exploit for its own ends, in order to "rejuvenate the fiction of the real" (ibid., 178).

REDRAWING THE MAP: RESPONSES AND REPOSITIONINGS

I began by suggesting that the relation between public realm theory and postmodernism/poststructuralism was more complex than Habermas' mapping of it allowed. By starkly opposing the paradigm of the philosophy of the subject and that of mutual understanding, Habermas can present postmodernism/poststructuralism as the coda to an exhausted project, the closure of subject-centered reason, rather than what comes after (1987, 296). Public realm theory stands, in contrast, ready to aid in the positive realization of the modern project, abandoning totalizing critique for the new paradigm.[6]

The danger of this polemical opposition is that it blinds us to the possibility of a different configuration. Once we detach Arendt's conception of the public realm from the problematic that Habermas constructs, a more complex articulation of public realm theory and postmodernism/poststructuralism becomes possible. If we step back from the issues of legitimation and consensus, Arendt's theory reveals a narrative structure that hooks up, in unexpected ways, with the concerns of postmodern/poststructuralist critics.

With respect to Foucault's power objection, one must first acknowledge that Arendt and Foucault are simply talking about different things when they use the word *power*.[7] True, Arendt's related concepts of violence, strength, authority, and so on are scarcely suited to capture the mechanisms that Foucault wants to investigate. Nevertheless, it is obviously inappropriate to presume a common referent here and then to argue that one theorist is right, the other wrong. More important, however, is that Habermas is wrong to treat Arendt's definition of power ("the ability to act together, to act in concert") as implying a consensus-centered conception of the public realm. From Arendt's point of view, plurality is not just a condition, but also an *achievement* of political action and speech: these activities give public expression to difference. The theory of political action presented in *The Human Condition* takes as its ideal an agonistic subjectivity that prizes the opportunity for individualizing action (1957, chaps. 2, 4). Contra Habermas, Arendt views action as an end in itself: its value in no way hinges upon the formation of a rational general will. Like Foucault and Lyotard, she fears that the regularization/rationalization of communicative action leads to the creation of docile subjects.

This concern about the denaturing consequences of a regularized, consensual politics comes across most strongly in Arendt's analysis of postrevolutionary American politics in *On Revolution*. Here she traces how the advent of a representative system transforms the public sphere, substituting voting and judicial decision making for spontaneous, agonistic action (1963, chap. 6). The loss of the "revolutionary spirit" yields a routinized politics of interests and general welfare: *actors* are transformed into *clients* who are

more or less effective in persuading a de facto oligarchy to favor their particular interests. The overall effect of the representative system is, from Arendt's perspective, virtually to eradicate the public space for agonistic action and to redirect political or public energies to the pursuit of private or social goals. The result is an essentially passive, depoliticized citizen body.

The regularization of politics (the containment of its spontaneous, popular, and agonistic dimensions) by the mechanism of representation points us to that larger, peculiarly modern threat to the public sphere—the rise of the social. With the emergence of this inclusive, hybrid realm in the modern age, the conditions of possibility for either a genuine public or private sphere are undermined. Moreover, the ubiquitous functionalization that accompanies the rise of the social imposes its own constraints on agonistic political action. As Arendt puts it:

> It is decisive that society, on all its levels, excludes the possibility of action, which formerly was excluded from the household. Instead, society expects from each of its members a certain kind of behavior, imposing innumerable and various rules, all of which tend to "normalize" its members, to make them behave, to exclude spontaneous action or outstanding achievement. (1957, 40)

It is here that Arendt's concerns most nearly intersect with Foucault's. Once we see Arendt's public realm as a space for agonistic action denatured by the normalizing power of the social, we can make suggestive connections to the Foucauldian story about the take-off of disciplinary power in the modern age. Arendt hardly shares Foucault's desire to develop a politics of everyday life. Nevertheless, they are both concerned to tell the story of how an essentially theatrical space (the agonistic public realm of Arendt, the premodern public realm of spectacle in Foucault) is colonized by a new form of disciplinary or "socializing" power, a power that substitutes an institutionally dispersed and normalizing regime of panoptic visibility for a centralized space in which *action* is seen and heard by all. The telos of this transformation, for both Arendt and Foucault, is the better preservation, cultivation, and management of the state's most precious resource, its populace. There is, I would suggest, a direct line to be drawn from Arendt's conception of the state as "national household" to Foucault's notion of biopower (Foucault 1980a, 140–43; 1988).

Foucault and Arendt are linked, moreover, by a concern to preserve forms and spaces of popular, spontaneous action ("counter-power") from encroaching bureaucratic structures. (Compare, for example, Arendt's emphasis on the spontaneous, popular nature of revolutionary action in *On Revolution* to Foucault's defense of direct forms of popular justice in *Power/Knowledge*; see Arendt 1963, chap. 6 and Foucault 1980b, 27–32.) Reading Arendt and Foucault in this manner suggests that Arendt's highly specific, exclusive notion of political action is not *contradicted* by Foucault's account of power. Rather, they present complementary narratives about the closure of the space for action in the modern age. From this standpoint, the Foucauldian concept of "resistance"—of local struggle against power/knowledge regimes—can be seen as a successor concept to Arendt's notion of political action. Where the space for action is usurped—where action in the strict sense is *no longer possible*—resistance becomes the primary vehicle of spontaneity and agonistic subjectivity.[8]

The reading I have just sketched moves Arendt's public realm theory from the legitimation problematic to that of agonistic subjectivity. The "structures of unimpaired intersubjectivity" that Habermas stresses give way to a performance-centered conception of action framed in terms of a narrative about the closure of the public space in the modern period. This displacement reveals similar continuities with Lyotard's project. A consistent theme in Arendt's work is the hostility of truth to politics. At no point in her thought on the nature of political action does she embrace moral cognitivism or indicate a belief in *theoretical* criteria that could distinguish a genuine from an inauthentic consensus. For Habermas, the Arendtian repudiation of the will to truth in politics is evidence of an anachronistic (Aristotelian) conception of theory (1983, 194). This objection, however, surely misses the point. The reason that Arendt sees "a yawning abyss between knowledge and opinion that cannot be closed by rational argument" is that her conception of political action is antiteleological (p. 194). Like Lyotard, she disputes the Habermasian assumption that "the goal of dialogue is consensus." Her account emphasizes action as an end in itself—the *performance*, as opposed to the *product*, of praxis.

Arendt, then, shares Lyotard's generally agonistic conception of speech. For both, "to speak is to fight, in the sense of playing, and speech acts fall within the domain of a general agonistics" (Lyotard 1984, 10; see Arendt 1957, 41–42). Moreover, her emphasis on the initiatory dimension of action ("to act . . . means to take an initiative, to begin") parallels Lyotard's focus upon the speaker's ability and need to create new moves, invent new criteria (Arendt 1957, 177; Lyotard 1985, 61). Arendt views the obsession with forming a rational general will as inevitably normalizing and destructive of plurality. In order to preserve this plurality from the regularizing force of consensus and theory, she endorses a "pagan" politics of opinion, of judgment "without criteria." Lyotard's slogan that there can be no "derivation of the political from the theoretical" captures the spirit of Arendt's own vehement anti-Platonism (Lyotard 1985, 26–27).

But if Lyotard's idea of a politics founded "on the basis of opinion" seems largely derivative of Arendt, there remains one seemingly irreconcilable difference. Lyotard celebrates the fragmentation of the public space; he glories in the heterogeneity this fragmentation produces. The only criteria of legitimacy he recognizes are local and context-specific, first-order narratives, as opposed to metanarratives/metadiscourses. In *Just Gaming*, this position brings

him very close to identifying the Aristotelian idea of an ethos as the sine qua non of a politics "without the Idea of Justice" (1985, 26). Arendt, whose own theory of political judgment presents a similar mix of Aristotelian, Kantian, and Nietzschean elements, avoids the relativist consequences of postmodern pluralism by appealing to the Kantian idea of a *sensus communis*, a common feeling for the world, which provides a nontranscendental basis for judgments making a universal claim of validity (Arendt 1968a, 220–21; see Villa 1992, 301–2). Lyotard views the appeal to the *sensus communis* as an act of bad faith: the withering away of common sense in postmodernity means, simply, that "there cannot be a *sensus communis*" (1985, 14). According to Lyotard, there is no escaping the fact that *we* "are in the position of Aristotle's prudent individual, who makes judgments about the just and the unjust without the least criterion" (ibid.).

As in the case of Foucault, the opposition here is less dramatic than it first appears. Arendt does not dogmatically assert the existence of "a common feeling for the world" in a last-ditch effort to avoid acknowledging the inescapable fragmentation of the public sphere. If, as with Foucault, we see Arendt as essentially providing a narrative about modernity, then we also can see that she hardly disputes the postmodern assertion that reliance upon the *sensus communis* is no longer possible. The last part of *The Human Condition* is an extended meditation on how the energies of modernity have worked to dissipate our feeling for the world. The "worldliness" presupposed by the *sensus communis* is *not* a distinguishing characteristic of the *animal laborans* and the late modern polity. Indeed, Arendt would go further than Lyotard on this score: the "blurring of boundaries" effected by the modern rise of the social destroys the integrity of language games and local contexts. The ground necessary for anything approximating an ethos is lacking (Arendt 1957, chap. 2, sec. 6 and chap. 3, sec. 17).

This brings us, finally, to the ontological objection. If, as I have suggested, Arendt's public realm theory shares the Foucauldian/Lyotardian concern with the preservation of agonistic subjectivity—if we see her as calling for a politics that stresses plurality, difference, spontaneity, and initiation against the regularizing apparatus of consensus—*then* the relation between her theory and poststructuralism/postmodernism is quite different from that suggested by the Habermasian mapping. Can a similar "shift" be effected with respect to the ontological objection? I would answer *no*: here we confront *the* insurmountable difference between Arendt's public realm theory and postmodernism. This difference is *not*, as some might suggest, the result of a misplaced foundationalism or realism on Arendt's part. Rather, it is a question of profoundly different attitudes toward a single phenomenon, the "de-worlding of the world" (to use Heidegger's phrase). Arendt mourns what the postmodern celebrates, the loss of certain ontological dimensions of human existence (action, the shared public world, the self as performance). The postmodern blurring of boundaries, the effacement of any meaningful distinction between the authentic and the inauthentic—reality and its simulacra—renders the Arendtian idea of the phenomenal integrity of a distinctly *political* realm simply nostalgic.[9] Arendt chooses, if not to *recover* the public realm, at least to preserve its memory. The "affirmative nihilists" of postmodernity wish to have done with such guilty nostalgia, viewing it as one more symptom of essentialism. There *is* a choice here, but it is not the one offered by Habermas. It is the choice between a politics of mourning and a politics of parody, a politics that remembers the *res publica* and a politics engaged in the endless subversion of codes.[10]

Notes

I thank Bill Connolly, Fred Dolan, Bob Gooding-Williams, Tom Dumm, and Austin Sarat for their comments and criticisms.

1. In a recent essay, Benhabib (1992) contrasts Arendt's model of an agonistic public space to Habermas' "discursive" model. Having previously argued that Habermas' theory of communication does justice to the phenomenon of plurality (Benhabib 1986), she attempts to show how the agonistic dimensions of Arendt's conception presuppose untenable distinctions (e.g., between the social and political) and exclusions. The upshot of both treatments is that Habermas absorbs what is valuable in Arendt's public realm theory, while avoiding its more "anachronistic" (Greek) features. In contrast, I believe Habermas has radically foreshortened the phenomenon of plurality and that the expression of this phenomenon demands something like Arendt's agonistic conception of political action. Cf. Connolly 1991.

2. In this regard, see the interview between Foucault and Rabinow, Charles Taylor, Martin Jay, and Richard Rorty (Foucault 1984, 373–80). Foucault is asked to comment upon the "Arendtian/Habermasian" conception of power (p. 377). His response is minimal: he admits the need to hold onto the notion of "consensual politics" as a regulatory principle, a statement that echoes his view in "The Subject and Power" (1982), where he takes a stand against nonconsensual power relations while remaining wary of the concept of consensus *überhaupt*. He fudges the question with specific regard to Arendt by stating: "It seems to me that in many of the analyses that have been made by Arendt, or in any case from her perspective, the relation of domination has been constantly dissociated from the relation of power. Yet I wonder whether this distinction is not something of a verbal one" (p. 378).

3. It should be added that Fraser rejects this possibility.

4. Hence, Foucault's ambivalent or ironic attitude toward the notion of consensuality. See n. 2. See Dumm 1988.

5. This bracketing leads Seyla Benhabib to mount a criticism of Lyotard parallel to Fraser's critique of Foucault. See Benhabib 1984, 114–15.

6. In fairness to Habermas, it should be noted that this mapping (at least as regards public realm theory) has been largely accepted by poststructuralist/postmodernist critics.

7. Janicaud (1992) points out that the critique Habermas offers of Foucault's theory of power substitutes the German *macht* for the French *pouvoir*, thereby eliding the distinction between *puissance* (strength) and *pouvoir* (power). This elision, similarly found in the English translation, facilitates the critique of Foucault as "normatively confused," a charge made by Habermas (1987), Fraser (1989), and Taylor (1984). But, Janicaud points out, *pouvoir* is typically reserved for "specific juridical–political forms and moralities" (1992, 284). Thus, while Foucault and Arendt use *power* to designate two

different phenomena (the juridical–disciplinary apparatus and concerted popular action), there is hardly the generalized conflation of power, strength, violence, and so on in Foucault that some of his critics charge. As Foucault makes clear in a number of places (esp. 1982, 220), he is sensitive to the kind of distinctions Arendt makes in "On Violence"; indeed, he considers them essential. The fact that he prefers to call popular action, à la Arendt, "counter-power" needs to understood against the background of his thesis concerning the ubiquitous, capillary, and productive nature of disciplinary power.

8. I should note that for Arendt, resistance *is* a form of action (1968a, pref.).

9. There is an obvious parallel here to Derrida's charge that Heidegger, while deconstructing the metaphysics of presence, nevertheless betrays a nostalgia for presence in his employment of the metaphorics of nearness and farness (of Being). See Derrida 1976, 22, 143; Derrida 1980, 130, 133. For a response to Derrida's critique, see White 1991, 65.

10. Fraser (1990) offers a critique of the Habermasian public sphere along quasi-Lyotardian lines, one that suggests an alternative beyond the purely mournful or subversive attitudes I have sketched. Arguing that the accordance of normative status to a singular, comprehensive public sphere enables domination via discursive means, Fraser opts for a more flexible theorization of the public sphere that makes room for a plurality of competing publics. This move from a strong normative conception to a more phenomenological account has the virtue of expanding our view of available and politically relevant discursive spaces by including "subaltern counterpublics" as crucial alternative forms of publicity. Fraser believes that participatory parity is better achieved by the cultivation of multiple publics that interact agonistically, rather than by the attempt to encompass all within one deliberative paradigm. Whether or not one finds Fraser's attempt to split the difference between postmodernism and public realm theory persuasive, it does serve to highlight one important consequence of Arendt's shift away from the legitimation problematic. Once this shift is made, the idea of the "public sphere" ceases to function in an essentially normative capacity. No longer setting the standards for genuine agreement or a just being-together (except in a minimal way, e.g., the prohibition of force and violence), public realm theory, in Arendt's hands, transforms itself into narrative, telling a story that keeps alive the memory of an agonistic public realm. If we overcome Arendt's nostalgia for an integral public realm, narratives such as those offered in *The Human Condition* and *On Revolution* can be seen as enabling us to identify, and see as significant, practices and spaces that would otherwise be overlooked: wherever we find agonistic spontaneity, resistance, and theatricality, there we find a *political* practice. The result is the cultivation of an agonistic political ethos that avoids claiming more for "subversive practices" than these practices can bear but that also eschews the alternative of mournful withdrawal.

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RESEARCH NOTES

SOCIOECONOMIC CLASS BIAS IN TURNOUT, 1964–1988: THE VOTERS REMAIN THE SAME

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We address the question of whether class bias in the American electorate has increased since 1964. We analyze the Census Bureau's Current Population Survey and the National Election Studies for seven consecutive presidential elections, 1964–88. Our results show that conclusions regarding changes in class bias are sensitive to which measure of socioeconomic class is used—income, education, or occupation. We argue that income is the appropriate measure since government policies that discriminate based on socioeconomic class are most likely to do so based on income and there are measurement problems associated with using either education or occupation over time. Our analysis shows that there has been almost no change in class bias in the electorate since 1964.

Who votes? has endured as a central question throughout several decades of electoral research. Much of this research has focused primarily on the demographic correlates of voter turnout. The common wisdom that education, income, and age are positively associated with turnout in presidential and off-year elections has been demonstrated most persuasively by Wolfinger and Rosenstone (1980) and supported by a score of other analyses (Cassel and Hill 1981; Conway 1981; Teixeira 1987; see also our work, Leighley and Nagler 1992).

While these analyses imply that voters are of higher socioeconomic status than nonvoters, the magnitude of the difference and, more importantly, its stability over time are disputed. Burnham (1982, 1987a), Bennett (1991), and Reiter (1979) argue that turnout decline since the 1960s has been significantly greater among low-status groups than high-status groups, while both Cavanagh (1981/82) and Teixeira (1987), using different measures and different time periods, claim otherwise. Any claim of disproportionate decline in turnout among one class of the potential electorate has troubling normative implications: if a predominantly upper-class actual electorate emerged from the low level of turnout, then we would expect policies chosen by elected officials to reflect such an elite bias; and representational ideals would suffer (Key 1949). Alternatively, had the decline in turnout since 1960 occurred equally across all social classes, then, at least in terms of representational equality, the character of the democratic process would not be altered.[1]

We challenge the conclusion that the post-1960s decline in voter turnout is marked by a large increase in socioeconomic class bias in the electorate. We examine the relative turnout rates of different demographic groups from 1964 through 1988 and demonstrate that there is actually remarkable stability in these relative rates over this period. Using multivariate analysis, we show that the effect of income on turnout has remained stable. In addition, we demonstrate why other scholars have reached a different conclusion regarding the change in class bias in the electorate.

ANALYZING TURNOUT

There are several possible reasons for disagreement regarding changes in the effects of socioeconomic status on turnout. First, observed effects will depend upon the choice of election years. Examining different years may generate conflicting findings for either of two different reasons: the sample estimates may be unstable from year to year, or the true population parameters may change from year to year. Attempts to solve the first problem by pooling the data across elections make the strong assumption that the effects of demographic characteristics on turnout do not change from year to year (see Teixeira 1987). If this assumption is incorrect, then, pooling the data sets may solve the problem of unstable coefficients but introduce yet another: the pooled model will be badly misspecified; hence, the resulting estimates will be biased and inconsistent (Johnston 1984).

A second possible reason for differing findings is that analyses of turnout often use reported, rather than the validated, vote. To the extent that misreporting is associated with other hypothesized correlates of turnout, findings based on the reported vote alone may simply be wrong (Hill and Hurley 1984). Katosh and Traugott (1981) and Sigelman (1982) assert that most inferences based on reported vote will remain true when the validated vote is examined. However, Abramson and a number of colleagues have shown quite persuasively that blacks are more likely than whites to overreport (Abramson and Claggett 1984, 1986; Silver, Anderson and Abramson 1986; Anderson and Silver 1986). Since race and income are correlated, inferences about the effect of income on turnout based on the reported vote may be biased, as well.

Third, disparate findings may arise from the choice and measurement of variables included in multivariate models. Since many of the demographic factors commonly included in studies of turnout are highly correlated (e.g., race, income, and education), the estimated effects of each factor will be extremely sensitive to the inclusion or omission of other factors (Johnston 1984). For instance, including occupation as a variable will alter estimates of the effects of income, education, and sex on turnout. Thus, comparison of findings regarding the latter three items across studies that are not consistent with regard to the inclusion of occupation becomes impossible.

Finally, conclusions regarding the representation of the socioeconomic groups in the electorate will necessarily be affected by the validity of the measurement of socioeconomic status. As shown by Wolfinger and Rosenstone (1980), the three standard indicators of socioeconomic status are related to turnout in different ways. This suggests that conclusions regarding socioeconomic class bias may be sensitive to which measure of socioeconomic status is chosen. Much of our analysis is devoted to demonstrating this point.

THE TURNOUT DATA

We analyze voting turnout in the presidential elections of 1964 through 1988 using the Current Population Surveys (CPSs) from the Bureau of the Census, as well as the American National Election Studies (NES) of the same years. We begin in 1964 because it marks the beginning of the long-term post-1960 decline in presidential turnout. Thus, we are able to examine whether that aggregate turnout decline has been distributed unevenly among different socioeconomic groups. Since 1964 is also the base year used in Burnham's (1987a) calculations, we can compare our findings to his. The CPS is a monthly survey of individuals conducted by the Bureau of the Census. Each November of even-numbered years the survey includes a set of questions about individual voting behavior, as well as the standard battery of monthly questions about respondents' income, education, occupation, and other demographic characteristics. Because the original data from the 1964 and 1968 CPS surveys are no longer available, our analysis for those years is limited to information available in Census Bureau reports. For the 1972–88 elections, we are able to provide a significantly richer analysis of changes in the electorate.

The CPS data offer several advantages in analyzing turnout in presidential elections due to sample sizes. In each election year there are from 89,000 to 115,000 valid respondents, approximately 50 times the number of respondents available in comparable NES data sets. This means that estimates of population parameters are much more precise. We are able to determine whether year-to-year variations in parameter estimates are the result of actual changes in the true parameters (i.e., changes in the real world), rather than simply being artifacts of the limited precision of sample estimates. While the NES data allow for estimating the effects of highly correlated measures, the limited sample size precludes firm conclusions regarding the *stability* of the population parameters from year to year.[2]

We compute turnout from the CPS as the percentage of respondents who report that they voted in the recent presidential election relative to respondents who provide valid responses to the question. This is slightly different from the Census Bureau's procedure.[3] The Census Bureau treats individuals with no response to the vote question as nonvoters for the purposes of their turnout calculation. Hence, our estimates of reported turnout are consistently higher than the Census Bureau figures.[4]

Since the NES is the standard source for studies of voter behavior, we compare our central findings to similar analyses of the NES data. In addition, we compare our findings based on the reported census vote to an analysis based on the NES validated vote for each election from 1964 through 1988, excluding 1968 and 1972.[5]

In the analyses that follow, we analyze class bias in six ways: (1) we report turnout rates for different socioeconomic groups; (2) we plot the relative turnout rates for different income groups over time; (3) we compute modified Gini coefficients based on income quintiles to obtain income-based measures of class bias that are based on the entire population, rather than the extremes of the income distribution; (4) we compare the turnout gap across all three types of measures (income, occupation, and education) to assess the sensitivity of findings to the choice of measure; (5) we analyze misreporting rates across class groups to test the robustness of our findings based on the reported vote; (6) we estimate a multivariate model using both the reported vote and the validated vote, to test for stability in the marginal effect of each measure of class.

TURNOUT AND SOCIOECONOMIC STATUS

Exactly how the socioeconomic composition of the electorate might have changed between 1964 and 1988 is not obvious. Examination of Census Bureau data shows that the percentage of high school graduates in the bottom income group rose 29.2% from 1964 to 1988, compared to only 16.5% for the top income group. Similarly, the percentage of high school graduates among blue-collar workers and white-collar workers increased by 35.4% and 14.3%, respectively. Since increases in levels of education have occurred more among the lower socioeconomic groups, we would expect the participation gap between the lower and upper socioeconomic groups to *decrease*. Alternatively, the poor were more likely to be young and single and to reside in female-headed households in 1988 than they were in 1964. We would

expect the first two of these changes to lead to lower voting rates among the lower-income group, that is, ceteris paribus, a *larger* participation gap in 1988 than in 1964.

The last, in fact, is the claim that Burnham makes: "The post-1960 turnout decline has been concentrated among those who had already participated least in the 1940–60 period" and is "strikingly selective" (1987a, 124–5). Burnham argues that after 1968, the low-cost information supplied by partisanship for voters became irrelevant in the new image-based politics. According to Burnham, the electoral consequences of these changes have not been equal across social classes because the participation of low-status individuals is more heavily dependent upon political institutions (e.g., party mobilization) than on personal resources.

Burnham (1987a) provides evidence for this thesis using data on working- and middle-class turnout in presidential elections in Boston from 1964 through 1984 and census data on the turnout of various occupational groups at two points in time. Census data for 1964 and 1980 show that the turnout rate of men classified as manual labor (blue-collar) dropped from 66.1% in 1964 to 48% in 1980. During this same period the rate for white-collar men dropped from 83.2% to 73.0%. Thus, while blue-collar male turnout dropped 18.1%, white-collar male turnout dropped only 10.2%. This differential decline is what Burnham refers to as "selective demobilization."

Reiter (1979) also claims that the class bias in turnout has increased. Rather than using occupation as a measure of class, he compares income quartiles for white NES respondents from 1960 through 1976. According to these figures, the gap between the voting rate of the top and bottom income quartiles has gone from 18.2% in 1960 to 28.7% in 1976. However, these findings are restricted to whites and hence may not provide an accurate picture of the entire electorate. This is particularly important because the time span Reiter focused on encompasses the large change in black turnout following the enactment of the 1965 Civil Rights Act. Basing conclusions on only the top and bottom quartiles of the income distribution also ignores possible changes in the turnout pattern of the middle two quartiles.

Bennett (1991) measures class bias in voting by education level and draws the same conclusion as Reiter for *whites under the age of 35*: class bias has increased. He reports that from 1964 to 1988, turnout among college graduates in this group went from 71% to 59%, while among whites without any college years, turnout dropped from 63% to 31%.[6] Bennett's findings bear less directly on the question of socioeconomic class bias in the electorate because the distribution of education in the population has changed so much from 1964 to 1988. According to the NES data, in 1964 only 30% of whites under 35 had some college experience. In 1988 the size of this group increased to 45% of whites under age 35.[7] Hence, Bennett is comparing a relatively high-status group in 1964 to a relatively lower-status group in 1988; persons who had attended college were rare in 1964 compared to 1988. Thus, the temporal nature of education as a measure of socioeconomic status can thwart attempts to measure change in the socioeconomic voting gap.

Finally, Cavanagh (1981/82), who reports no net change in class representation since the 1950s, provides evidence contradicting these conclusions. Dividing the NES data into thirds rather than quartiles, he computes a class bias ratio defined as the ratio of the turnout rate of the top third income group to the turnout rate of the bottom third income group. He points out that the years 1960 and 1964 were relative lows in class bias for the postwar period and that in fact the class bias scores for 1972 and 1976 are approximately the same as those for 1952 and 1956. This evidence highlights how critical the choice of years is in drawing inferences regarding changes in socioeconomic class bias. It is also interesting to note that the time period that Cavanagh describes as being marked by unusually low socioeconomic class bias includes the election year (1964) that Burnham uses as a base for his calculations.

That Cavanagh's claim of no change in class bias is based on an income measure suggests that the choice of socioeconomic status measures may be critical for the conclusions drawn regarding socioeconomic class bias. Burnham (1987a) based his conclusion regarding the 1964–80 period on only one measure of socioeconomic status, occupation.[8] There are three reasons to prefer income to occupation as the relevant measure of socioeconomic status. The first reason is fundamental to the entire exercise; income is the more relevant measure with regard to government policy. Few government programs convey benefits based solely on occupation. Instead, government policies are debated in terms of their effect on the rich versus the poor; and many government policies explicitly distinguish between individuals based on income: the tax code, Medicaid, Aid for Families with Dependent Children, and so on. The second and third reasons are based on the ambiguity in measuring occupational status. Some occupations may be difficult to classify as either white-collar or blue-collar in a given year. Finally, the socioeconomic class rankings of occupations may not be stable over time.

Table 1 shows the overall turnout rate for presidential elections from 1964 through 1988, as well as the turnout rate by different measures of socioeconomic status: income, occupation, and education. Individuals' family income is coded into quintiles, using the overall family income distribution among the potential electorate (i.e., among individuals over the age of 18) for 1964–88. Quintiles of income are used, rather than reported family income for each election year, so that the indicator reflects socioeconomic status instead of absolute wealth. This is especially appropriate for comparisons across time, since an individual's ranking in the income distribution is more comparable over time than is the individual's absolute income level. (See Table A-1 for the exact size of each income quintile.)

TABLE 1

Turnout by Demographic Characteristics (1964–88, Current Population Surveys)

DEMOGRAPHIC GROUP	1964	1968	1972	1976	1980	1984	1988
Total	70.5	70.3	64.9	62.6	62.3	63.1	60.1
Income							
1 (Low)	53.7	57.5	50.7	47.4	47.7	47.8	42.7
2	63.1	67.0	55.9	55.1	54.2	57.5	52.2
3	72.8	74.2	63.3	62.4	60.5	63.5	58.9
4	78.7	79.7	71.6	69.9	68.9	70.3	67.5
5 (High)	85.2	85.4	80.7	78.4	76.1	77.6	76.9
Gini coef.	—	—	.0941	.0984	.0997	.0985	.1112
Occupation							
Service work	65.9	62.7	58.6	52.8	51.3	52.9	47.2
Blue-collar	65.6	62.3	54.2	49.8	48.0	49.4	44.5
White-collar	82.1	79.8	76.4	72.1	70.9	70.8	68.5
Education							
No high school	59.4	56.4	48.9	46.8	44.5	45.1	38.4
Some high school	65.9	63.1	53.7	50.1	48.4	47.0	43.3
High school grad.	76.5	74.2	66.1	62.0	61.0	61.0	56.6
Some college	82.6	80.2	76.0	70.1	69.5	69.9	66.4
College grad.	—[a]	84.7	83.6	81.1	80.1	79.3	75.8
Post-college	88.2	86.9	87.4	85.2	84.9	84.4	83.9

Note: Table entries are the reported turnout rates for each demographic category.
[a]For 1964 *college grad* and *post-college* are only available in combined form.

In any given year, the socioeconomic class bias in turnout is clear, with the highest-income group typically voting at over 70%, and the lowest-income group typically voting close to 50%. However, the wide range in turnout across income groups does not imply a significant change in *relative* turnout rates over time. From 1964 to 1988, turnout of the lowest-income group dropped 11%, while turnout for the highest-income group dropped 8.3%, suggesting only a slight increase in socioeconomic class bias.

If we consider the time period 1964–80 (used by Burnham 1987a), we find that turnout dropped 6% for the lowest-income group and 9.1% for the highest-income group. This suggests that based on income, class bias actually *decreased*. These findings highlight how critical the choices of time period and measures of socioeconomic status are to conclusions regarding class bias (points to which we shall return shortly). Moreover, they provide preliminary evidence that Burnham's conclusions regarding class bias across the 1964–80 period are largely a function of his choosing occupation as a measure of socioeconomic status.

Figure 1 graphs the difference between the turnout rate of each income group and the overall turnout rate for 1964–88. This provides a measure of the changes in each income group's turnout relative to changes in the overall level of turnout for each year. Disproportionate decline in the turnout of one class would be reflected in the difference between that class's turnout and overall turnout becoming increasingly negative over time. The larger the negative turnout difference, the greater the underrepresentation of that class. Thus, if a group's representation among voters is declining, as Burnham (1987a) claims, we should see a negative slope to its line in Figure 1.

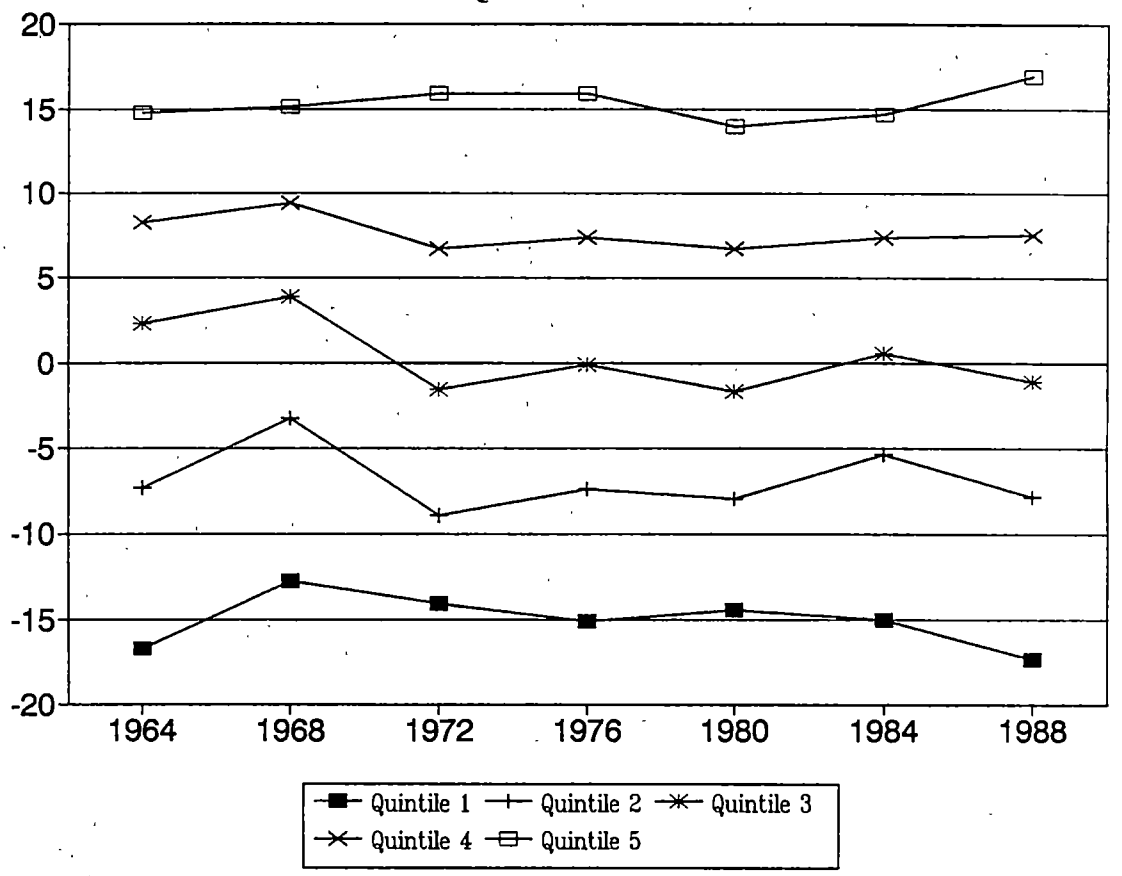
This is not the case for lower-income groups. Between 1964 and 1988, there is only a marginal change in the difference between turnout rates for the two lowest-income groups and the overall turnout rate—from 16.8% and 7.4%, respectively, in 1964, to 17.4% and 7.9%, respectively, in 1988. The greatest decline in turnout relative to the overall rate between 1964 and 1988 actually occurs for the middle-income quintile, with a change from +2.3% to −1.2%, while the only increase in turnout occurs in the highest income group (from 14.7% to 16.8%).

Each of these differences, however, is exaggerated by relatively large changes between 1984 and 1988. If the turnout differences were compared for 1964 and 1984, we could report that relative turnout of the two lowest-income groups had increased, the middle-income group had declined slightly, and the relative turnout of the upper-income groups had basically remained stable.

If we restrict the analysis further to the 1964–80 period used by Burnham (1987a), we would, again, reject the selective demobilization claim. Between 1964 and 1980, the relative turnout of the lowest-income group actually increased slightly—the only group to do so. Slight declines in the turnout difference for the upper two income groups, as well as the next-to-lowest-income groups, occur, while the middle-income group experiences the largest relative decline in turnout across the 1964–80 period. Thus,

FIGURE 1

Difference between Turnout for Each Income Quintile and the Overall Turnout Rate



Source: Computed from the Current Population Surveys, 1964–88.

restricting the time period to that used by Burnham does not change our initial conclusion that socioeconomic class bias in the electorate has not increased since 1964.

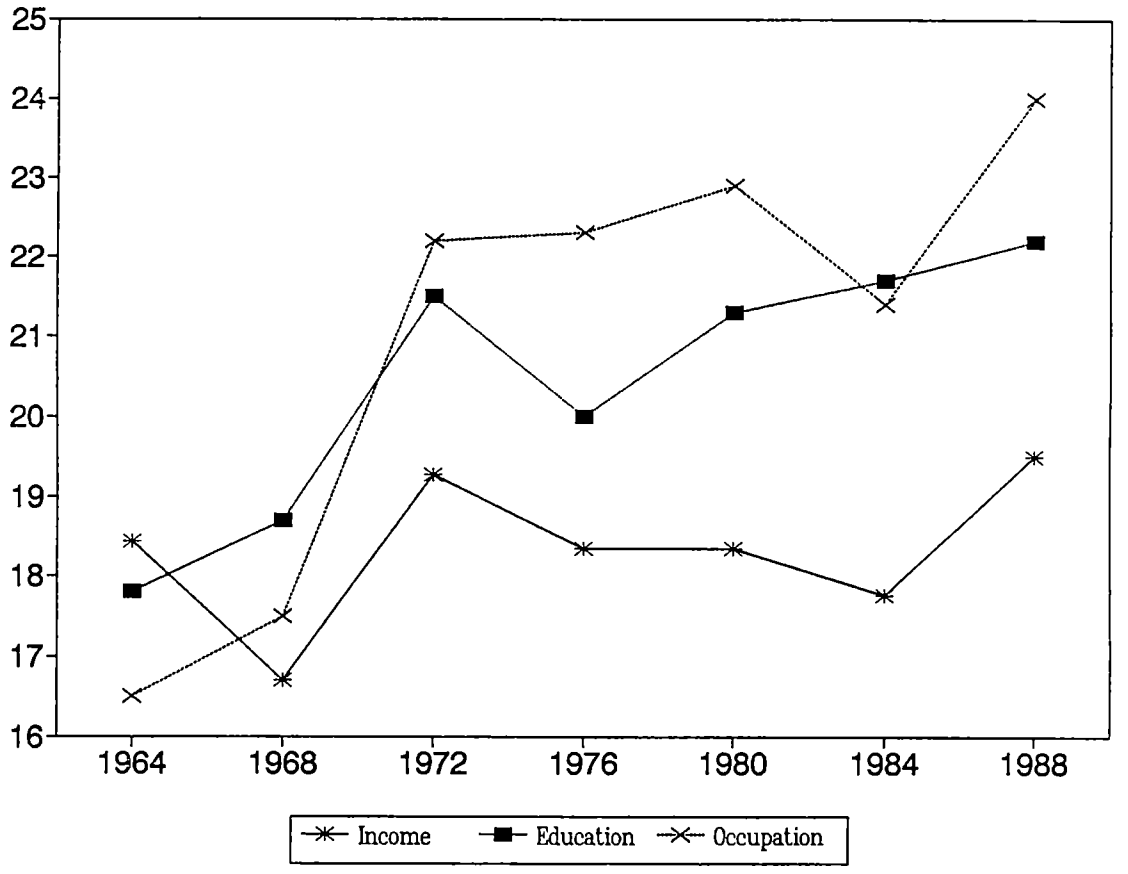
A more precise measure of the relative representation of low-income versus high-income individuals in the electorate is derived from the CPS data using a modification of the Gini coefficient. Gini coefficients are usually used as measures of income inequality of societies (e.g., the amount of wealth held by the richer, compared to the amount held by the poorer, members of society). We have modified the calculation of it slightly to make it a measure of electoral representation for different income groups. Though the Gini coefficient is not typically used in analyses of turnout, it offers two advantages. First, it is a calculation that makes use of all the voters, rather than merely the extremes of the income range used when turnout difference scores are calculated based on the turnout of the highest- versus lowest-income groups. Second, it gives a bounded measure that can be compared across years. These advantages are especially valuable to resolve the class bias dispute, given that findings based on simple comparisons of the lowest- to the highest-status groups may depend upon the size of the groups chosen.

Each year, the CPS categorizes respondents in one of 14 income groups. We computed 14 pairs of points for each year—the percentage of persons at or below each income level paired with the percentage of voters at or below that income level. The pairs were ordered by ascending income level, then plotted on the x- and y-axes. The Gini coefficient is defined as twice the area between this curve and a 45-degree line. If equal percentages of persons voted regardless of their income level, then the pairs should trace a 45-degree line; and the Gini coefficient would be zero. Alternatively, if the only persons who voted were those persons in the highest-income group, then all points except the final one would lie on the x-axis, and the Gini coefficient would be one. Thus, the Gini coefficient is bounded by zero and one, with zero representing perfect equality of representation by income groups and one representing a situation where all of the votes are concentrated in the hands of the top-income group: the higher the Gini coefficient, the more unrepresentative the voting population is.[9]

The Gini coefficients, reported in Table 1, follow a pattern that is the inverse of overall levels of turnout; that is, when turnout decreases, the Gini coefficient increases, and vice versa. During the 1964–88 period,

FIGURE 2

Trends in Class Bias, Using Various Measures of Socioeconomic Status



Note: Income bias is the difference in turnout rates between the top and bottom halves of the income distribution. Education bias is the difference in turnout rates between high school graduates and non–high school graduates. Occupation bias is the difference in turnout rates between white-collar and blue-collar workers.
Source: Computed from the Current Population Surveys, 1964–88.

there is again no secular trend of increasing unrepresentativeness of the electorate based on income. Were the lower-income groups voting less or the higher income groups voting more, we would see a consistent increase in the Gini coefficients over time. Instead, the Gini coefficient is almost unchanged from 1976 to 1980 and drops from 1980 to 1984. It is only in the 1988 election that the Gini coefficient shows a sizable increase.[10]

SELECTIVE DEMOBILIZATION AND ALTERNATIVE MEASURES OF SOCIOECONOMIC CLASS

So far, we have determined that with income as a measure of socioeconomic class, there has been virtually no increase in class bias between 1964 and 1984 and that 1988 stands out in contrast to that stability. To the extent that this finding is solely a function of using income, rather than occupation, as a measure of socioeconomic status, we have not resolved the class bias controversy but, instead, merely added to it. We now explicitly consider Burnham's (1987a) claim regarding selective demobilization by comparing estimates of class bias using occupation groups, education, and income as alternative measures of socioeconomic status. For ease of comparison, we have plotted turnout differences between upper- and lower-status groups using each of the three measures. Figure 2 plots these differences for (1) the top 50%, versus the bottom 50%, of the income distribution; (2) white-collar workers versus blue-collar workers; and (3) high school graduates versus individuals with fewer than 12 years of school.[11]

So far as education, income, and occupation reliably measure the same underlying trait (socioeconomic status), each indicator of socioeconomic class should generally follow the others across time. According to Figure 2, the level of socioeconomic bias associated with occupation increased significantly between 1964 and 1988. The lower occupation group (manual labor) has a sharply reduced voting rate vis-à-vis the higher occupation group (white-collar workers) in 1988 compared to 1964; the gap is 24% in 1988 compared to 16.5% in 1964. Consistent with

TABLE 2

Turnout by Demographic Characteristics (1960–88, National Election Studies)

DEMOGRAPHIC GROUP	1960	1964	1968	1972	1976	1980	1984	1988
Total	82.2	77.7	75.8	72.8	71.6	71.4	73.6	69.7
Income								
1 (Low)	65.2	63.8	60.5	60.1	54.3	56.4	52.7	47.4
2	72.4	72.9	66.1	62.9	65.4	68.0	69.3	58.9
3	85.0	79.0	79.3	70.5	70.9	72.1	74.5	71.0
4	89.9	85.8	86.8	86.2	80.4	81.4	84.4	82.4
5 (High)	96.2	87.8	92.5	90.4	91.4	86.7	91.3	95.6
Occupation								
Blue-collar	81.3	74.4	69.1	68.3	65.1	63.9	64.4	54.7
White-collar	91.9	85.2	86.4	82.1	82.2	81.4	83.3	81.2
Education								
No high school	70.5	67.8	60.4	58.0	59.8	58.6	58.0	50.0
Some high school	75.2	71.3	68.6	60.8	57.2	54.8	56.1	49.8
High school grad	89.2	80.3	82.7	74.7	67.0	61.4	65.9	62.3
High school plus training	93.6	85.1	85.8	76.5	73.9	77.8	73.8	—
Some college	90.5	88.6	79.1	83.9	83.3	75.8	81.5	78.4
Post-college	96.3	87.7	89.4	89.9	87.1	90.7	90.6	92.4

Note: Table entries are the reported turnout rates for each demographic category.

Burnham (1987a), this gap is also evidenced when comparing 1980 to 1964.

Figure 2 also shows that the socioeconomic bias associated with income—the difference in turnout between the top 50% and bottom 50% of the income distribution—rose from 1968 to 1972. However, the increase is less than half the increase shown by the occupation gap. Furthermore, the income gap in 1968 was exceptionally low; the 1972 income gap is actually less than 1% above the 1964 income gap. The socioeconomic bias associated with education—the gap between turnout rates of high school graduates and nongraduates—also increased significantly less from 1968 to 1972 than did the occupation gap, according to Figure 2. Thus, the substantive conclusions drawn regarding selective demobilization are indeed sensitive to the choice of measure used. Claims regarding selective demobilization cannot be sustained when using income as a measure of socioeconomic status.

Several points cause us to question the validity of occupation as a measure of socioeconomic status. First, the majority of the drop in relative voting rates among the occupation groups occurred in one time period: from 1968 to 1972 the voting gap between the two groups increased from 17.2% to 22.2%. This suggests that rather than being a secular trend over the time period identified by Burnham (1987a), the turnout decline for manual labor actually occurred between the 1968 and 1972 elections.[12]

Second, the education gap, as well as the income gap, decreased in 1976, while the occupation gap continued to rise. If the variables plotted in Figure 2 measure the same phenomena—turnout differences as a function of socioeconomic status—then they should move in the same direction (and, ideally, with the same magnitude) from year to year.

Third, were occupation a reliable measure of socioeconomic class during this period, then we should see similar patterns of turnout for occupational groups using the NES. Instead of replicating the CPS findings, the NES shows quite different patterns of turnout for blue- and white-collar workers over time (see Table 2). For instance, between 1968 and 1972, when Burnham (1987a) reports a significant change in relative turnout levels, the NES data show that turnout for blue-collar workers is fairly stable (59.1% in 1968, 68.3% in 1972), while turnout for white-collar workers drops from 86.4% in 1968 to 82.1% in 1972. Thus, the turnout differences between white-collar and blue-collar workers for 1968 and 1972 are 17.4% and 13.8%, respectively. This suggests that across these two elections, with occupation measures from the NES, socioeconomic class bias actually *decreases*.

Having presented evidence suggesting that occupation is less reliable an indicator than education or income, we now offer an alternative explanation for the drop in relative turnout between 1968 and 1972 among the lowest occupation categories observed by Burnham (1987a). Since these two elections spanned a decennial census, the changes the Census Bureau made in their occupation codes could account for the discrepancy between the pre- and post-1970 turnout rates. In the absence of supporting evidence for the sudden drop among the low occupation category and in the face of conflicting turnout patterns using the NES data, the coding change hypothesis must be considered a plausible explanation for the figures.

TABLE 3

Misreporting by Occupation and by Income (1964, 1976–88, National Election Studies)

DEMOGRAPHIC GROUP	1964	1968	1972	1976	1980	1984	1988
Occupation							
Blue-collar	40.3	—	—	31.8	21.2	23.7	16.1
White-collar	53.0	—	—	44.5	45.0	37.5	38.6
Income							
Low	37.4	—	—	31.3	26.1	21.6	16.8
High	45.2	—	—	34.7	33.7	33.6	33.2

Note: Table entries are the percentage of *nonvoting* respondents in each category who claim to have voted.

This reflects the basic problem with the use of the occupation variable: it is fundamentally a categorical, rather than ordinal, measure.

REPORTED VERSUS VALIDATED TURNOUT

To verify that these findings do not result from changes in the tendency of high- or low-income—or blue- or white-collar—individuals to misreport voting, we also examined turnout using the NES validated vote. If low-income individuals or blue-collar workers are now voting in lower numbers but misreporting their behavior more frequently, then we would fail to find an increase in socioeconomic class bias using the reported CPS vote. If the high relative misreporting rate for blacks observed by Abramson and his coauthors is a recent phenomena and extends more broadly to lower socioeconomic groups, then our finding of no increase in class bias could be erroneous. To test our earlier results we calculated the probability of individuals' misreporting for each income and occupation group conditioned on their not having voted. (Almost no one votes and reports that they did not.)

These results are reported in Table 3, which shows that in fact, over time, there have been rather large changes in misreporting probabilities. While both occupational groups show less of a tendency to misreport in 1988 than in 1964, the decline in misreporting is much larger for the blue-collar workers. Misreporting by white-collar individuals decreased from 53.0% in 1964 to 38.6% in 1988; among blue-collar individuals the decrease was even larger: 40.3% in 1964 compared to 16.1% in 1988. Thus, the difference in misreporting *increases* from 12.8% in 1964 to 22.5% in 1988, with higher-status individuals being relatively more likely than lower-status individuals to misreport in 1988 than in 1964. Using the reported vote, differences in turnout between classes would be more exaggerated in 1988 than in 1964.

We find a similar result when comparing misreporting by income. Misreporting for the bottom half of the income distribution decreases from 37.4% in 1964 to 16.8% in 1988, while for the top half of the income distribution it decreases from 45.2% in 1964 to 33.2% in 1988. Hence, the difference in misreporting rates again *increases* from 7.8% in 1964 to 16.4% in 1988. Analyzing reported vote over time would tend to exaggerate increases in socioeconomic class bias since the higher-class groups' turnout would be falsely overreported. Hence, using the reported vote to claim stability in the socioeconomic voting gap is a conservative test and does not call into question the validity of our finding that socioeconomic status bias has remained stable.[13]

MULTIVARIATE MODEL

We now adopt a multivariate model that allows us to assess the marginal impact of each demographic characteristic on voter turnout. As a further test of our claim that there has been little or no change in socioeconomic class bias, we use this model to test the stability of the marginal effect of income (other demographic traits held constant) across elections. In this model, we include the basic individual demographic characteristics found to be correlates of voting: education, income, age, sex, marital status, and race. We also include a dummy variable to denote the South. Since the effect of age on turnout appears to decrease at some point in the individual's life cycle, we also include an age-squared term in the model, with the expectation that its estimated coefficient would be negative.

The probit estimates of this model for the reported vote are presented in Table 4. The stability of the coefficients of income and education (they are bounded between .115 and .139 and between .266 and .283, respectively, for each year) is further evidence that there has not been a major shift in the impact of socioeconomic status on voting. In other words, had high-status individuals become more likely—and low-status individuals less likely—to vote, then the effect of education and income as independent predictors of turnout should have increased over time. That the estimate for income decreases slightly (.126 vs. .115), and the coefficient

TABLE 4

Probit Estimates of the Demographic Model of Turnout (1972–88, Current Population Surveys)

VARIABLE	1972	1976	1980	1984	1988
Constant	−2.612	−2.927	−2.943	−2.884	−3.061
	(.038)	(.040)	(.033)	(.036)	(.038)
South	−.235	−.107	−.103	−.120	−.121
	(.011)	(.011)	(.010)	(.011)	(.010)
Education	.266	.275	.281	.280	.283
	(.0037)	(.0039)	(.0033)	(.0036)	(.0038)
Income quintile	.126	.129	.115	.125	.139
	(.0039)	(.0042)	(.0033)	(.0036)	(.0039)
Age	.052	.056	.054	.052	.049
	(.0015)	(.0016)	(.0014)	(.0015)	(.0015)
Age squared	−.00034	−.00036	−.00033	−.00030	−.00026
	(.000017)	(.000017)	(.000014)	(.000015)	(.000015)
Male	.026	−.018	−.048	−.083	−.055
	(.0095)	(.0100)	(.0084)	(.0089)	(.0092)
Black	.098	.061	.040	.235	.147
	(.016)	(.017)	(.014)	(.015)	(.016)
Marital status	.105	.118	.161	.122	.153
	(.011)	(.012)	(.010)	(.010)	(.010)
Number of cases	83,360	75,644	107,200	95,380	89,423
% correctly predicted	70.88	70.12	70.63	71.01	70.58
% voted	65.44	65.23	63.85	64.90	62.64
Log likelihood	−47,259	−43,327	−60,685	−53,616	−50,593

Note: All coefficients are significant at $p < .01$. The dependent variable is the respondent's reported turnout for each presidential election. Table entries are probit coefficients, with standard errors in parentheses.

for education remains essentially unchanged (.266 vs. .281) from 1972 to 1980—the endpoint for Burnham's (1987a) calculation—suggests that class bias remained essentially the same over that period.

To interpret the probit coefficients, predicted probabilities based on changes in the independent variables should be examined. The predicted probability of voting is equal to $\Phi(X_i\hat{B})$, where $\Phi(X_i\hat{B})$ is the cumulative normal evaluated at the sum $X_i\hat{B}$ (Johnston 1984). Therefore, the predicted probability will depend upon the value of $X_i\hat{B}$, or of the values of all the independent variables. The effect of a change in an independent variable can be calculated by computing two predicted probabilities: an initial probability based on specific values of the independent variables, and an adjusted probability based on a change in the independent variable of interest. For instance, the effect of moving from the middle-income quintile to the next highest quintile for a 30-year-old, single, white northern male with high school education in 1972 would have been to increase the likelihood of voting by 4.8% (from 59.9% to 64.7%).[14] In 1988, the effect would have been to increase the likelihood of voting by 5.5% (from 42.6% to 48.1%), meaning that the effect of income has increased for such an individual by only .7% over the five elections. Thus, even when considering 1988, there is not evidence of a large increase in class bias.

Additional evidence that class bias has not increased significantly is provided by estimating the same multivariate model using the NES validated vote. As reported in Table 5, the coefficient of the income variable is quite unstable over time. In particular, the increasingly large estimates of the impact of income on turnout between 1980 and 1988 derived from the census data are not replicated using the validated vote. Instead, the income coefficient is at its lowest in 1980 (.056), increases to .190 in 1984, and then drops to .104 in 1988. Thus, even were we to extend the time period under consideration to assess Burnham's argument, we find no support for dramatic increases in class bias.

Tables 4 and 5 indicate that the effects of two socioeconomic characteristics other than income, occupation, and education *have* changed as predictors of turnout between 1964 and 1988. The estimates for sex show a noticeable trend toward increased voting by women relative to men. The coefficient of race is much less stable than the other estimated demographic coefficients. According to the CPS data, the effect of being black on turnout is positive, though it decreases in magnitude throughout the 1970s, then increases in the 1980s. The validated NES data suggest that the effect of being black decreases the likelihood of voting, with the magnitude of the impact increasing throughout the 1980s. Thus, while there may be changes in the composition of the electorate over time, they are associated with less class-based measures than income, occupation, or education.

TABLE 5

Demographic Model of Validated Turnout (1964, 1976–88, National Election Studies)

VARIABLE	1964	1976	1980	1984	1988
Constant	−1.793**	−2.432**	−2.498**	−2.045**	2.570**
	(.354)	(.226)	(.289)	(.242)	(.261)
South	−.768**	−.329**	−.204*	−.274**	−.387**
	(.095)	(.068)	(.082)	(.070)	(.072)
Education	.121**	.154**	.240**	.162**	.214**
	(.027)	(.021)	(.027)	(.024)	(.023)
Income quintile	.142**	.165**	.056	.190**	.104**
	(.041)	(.034)	(.041)	(.034)	(.036)
Age	.055**	.059**	.054**	.039**	.058**
	(.015)	(.010)	(.013)	(.011)	(.011)
Age squared	−.0003*	−.0004**	−.0003**	−.0001	−.0003**
	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)
Male	.030	.017	.028	−.171**	−.050
	(.079)	(.060)	(.076)	(.063)	(.068)
Black	.054	.090	−.059	−.069	−.164
	(.141)	(.102)	(.122)	(.101)	(.099)
Marital status	−.014	.189**	.282**	.193**	.262**
	(.105)	(.068)	(.085)	(.069)	(.074)
Number of cases	1,218	1,708	1,301	1,902	1,714
% correctly predicted	70.9	69.2	67.9	68.0	70.4
% voted	65.8	64.5	58.9	62.8	57.7
Log likelihood	−697.9	−1,252.5	−780.9	−1,108.1	−991.7

Note: The dependent variable is the validated measure of the respondent's turnout in each presidential election. Table entries are probit coefficients, with standard errors in parentheses.
*$p < .05$
**$p < .01$

CONCLUSION

The question whether class bias in the electorate has changed over time seems straightforward. However, the debate it has engendered suggests otherwise. We have attacked this question in as direct a way as possible. By tracing the demographic characteristics of all voters (as opposed to particular subsets of voters) over seven consecutive presidential elections and using samples large enough to reduce random error, we found that class bias has not increased since 1964. This conclusion is buttressed by a comparison of different distributions of income over time using Gini coefficients, as well as by multivariate analyses.

Any conclusion regarding class bias in the electorate is dependent upon the choice of measure of socioeconomic status. We argue that from a theoretical perspective, income is the appropriate measure to use, since government policy is more likely to distinguish among individuals based on income, rather than any other single measure of socioeconomic status. And we point out that there are measurement problems with alternative indicators, such as occupation.

We have painted a picture of turnout that is remarkably stable over seven different elections spanning 24 years. Despite different social and political contexts and different candidate choices, the class differences between voters and nonvoters in presidential elections remain the same. These elections include races between incumbents and challengers, as well as two open seat races. During these elections, the electorate faced a broad array of issues, ranging from Vietnam to double-digit inflation to $200 billion budget deficits. The one deviant election we observed was 1988, where socioeconomic class bias in turnout did seem to increase. The class consequences of Reagan administration policies beginning in 1981 may have influenced the composition of the electorate in 1988 (see Phillips 1990). Alternatively, campaign-specific effects such as the content of television advertising or the targeting strategy of either campaign could account for the distinctiveness of the 1988 electorate. Whether these differences persist in the 1992 election is obviously an unanswered question.

One can interpret these findings either optimistically or pessimistically. From a normative perspective, the lack of an increase in class bias in the electorate is good news. However, from the representational perspective of desiring an electorate able to adapt to changing circumstances and choices, the stability is not necessarily good news.

TABLE A-1

Size of Income Quintiles: Percentage of Eligible Voters in Each Group

INCOME	1964	1968	1972	1976	1980	1984	1988
Current Population Surveys							
1 (Low)	19	27	18	17	20	22	17
2	20	24	22	23	15	21	22
3	28	19	15	24	25	16	19
4	16	20	25	15	14	24	25
5 (High)	18	10	19	21	26	17	18
National Election Studies							
1 (Low)	19.1	15.7	19.2	14.1	17.4	15.2	19.1
2	18.4	21.0	11.8	19.2	15.6	17.7	13.2
3	26.0	31.5	37.2	31.0	36.2	35.5	35.4
4	29.5	26.7	26.8	30.5	25.4	26.0	29.7
5 (High)	7.0	5.2	5.1	5.2	5.4	5.6	2.5

APPENDIX

The exact size of each income quintile is shown in Table A-1.

Notes

This is one of several joint papers on turnout in American national elections; the ordering of the authors' names reflects alphabetic convention. We appreciate constructive comments on earlier versions of this paper from Frank Baumgartner, Wendy Hansen, Kim Hill, Patricia Hurley, Rod Kiewiet, Rebecca Morton, Doug Rivers, and Larry Rothenberg. We would like to acknowledge the research assistance of Sam Hudson and typesetting assistance of Marcia Bastian. Research support from the Texas A&M University College of Liberal Arts and Supercomputer Center is also appreciated. The data utilized in this publication were made available in part by the Inter-University Consortium for Political and Social Research. The data for the American National Election Studies, 1964–88, were originally collected by Warren E. Miller and the National Election Studies.

1. See Burnham 1982 and Kleppner 1982 for discussions of historical trends in turnout and representation.

2. For instance, to conclude that coefficients from consecutive elections are even significantly different from each other using an NES data set that typically generates income coefficients with t-statistics of about 4—much less to estimate the magnitude of the difference—would require that one estimated coefficient be over twice the size of the other.

3. We have corrected Census Bureau–reported figures for 1964 and 1968 to be consistent with our method of computing turnout.

4. For a discussion of the properties of the Census Bureau estimates, see Jennings 1990.

5. No validation study was conducted in 1968; and though a validation variable is available for 1972, the CPS discourages its use because the validation procedure for the 1972 vote was conducted in 1976 and produced poor estimates of validated turnout (Traugott 1989).

6. Bennett 1991 finds an increase in class bias among older whites, as well, but notes that it is not nearly as large: the gap in turnout between the two groups increases only 4%.

7. Directly comparable census figures are not available. However, according to the census data, 30% of individuals over age 20 had some college experience in 1964, and 38.8% of individuals over age 20 had such experience in 1988.

8. In Burnham's many writings on the subject of social class and American political behavior, he has used a variety of measures of social class, including income, education, housing values, and homeownership (1978, 1987b). But on the narrow question of changes in class bias after 1960, he consistently uses Census Bureau occupation data as a measure of social class.

9. The Gini coefficients essentially incorporate the same information that would be contained in a regression of voting rates against income: the coefficient of such a regression would represent the best way to fit all the voters to the data. Computing regression coefficients for the five years using the voting rate of each income quintile as the independent variable yielded coefficients consistent with the estimated Gini coefficients: the smallest coefficient was for 1984, the largest for 1988.

10. Since the raw data was unavailable for 1964 and 1968, we could only compute Gini coefficients for those years using the six income categories contained in the Census reports. These coefficients (.0856 and .0770) were lower than the Gini coefficients we computed for subsequent years. However, their reliability is extremely suspect, because Gini coefficients calculated with fewer income groups would necessarily be smaller than those calculated with income groups representing a finer breakdown of the income distribution.

11. These income gaps are not exactly the top 50%, versus the bottom 50%, of the distribution, because income is a categorical, rather than continuous, variable in the survey. However, in each year, the difference between the size of the top and bottom group is no more than 8%. The size of the blue-collar group ranges from 37.1% to 30% of the workforce, the size of the white-collar group ranges from 45.4% to 56.7%. In 1964, only 52.4% of respondents were high school graduates; by 1988 they had reached 77.4%.

12. We also examined the sensitivity of the occupation variable to disaggregation by sex, since Burnham only considered the turnout of men. The 1968–72 jump was identical for men and women. After 1972, the gap for women decreased; but the overall trend remained the same as the trend for men only.

13. See Filer, Kenny, and Morton 1991 for estimates of demographic voting based on county turnout figures (hence uncontaminated by misreporting problems).

14. In the CPS data, income quintiles are coded from 0 to 4 (4 being the highest), education is coded from 1 to 8 (8 being highest).

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TOWARD A THEORY OF THE POLITICAL ENTREPRENEUR: EVIDENCE FROM LOCAL GOVERNMENT

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Political scientists have been increasingly interested in entrepreneurs—individuals who change the direction and flow of politics. In this research note, we synthesize aspects of an economic approach to entrepreneurship with concepts used in political science. We then tie these theoretical observations to the emergence of entrepreneurs in local governments and test components of our theory using observations from a large set of suburban municipal governments. Empirically, we identify several conditions that affect the probability that an entrepreneur will emerge in a local government, especially slack budgetary resources that the political entrepreneur can reallocate. We also find that the probability with which an entrepreneur is found in local government is a function of the difficulty of overcoming collective action problems in a community.

Political scientists have been increasingly interested in entrepreneurs—individuals who change the direction and flow of politics. Existing studies of political entrepreneurship rely heavily on case study material. Most common are biographical studies of entrepreneurial leaders, which focus on individuals whose actions produced policy changes that were innovative or unexpected (see, e.g., Doig and Hargrove 1987; Kirchheimer 1989; Lewis 1980; Riker 1986; Weissart 1991). Such studies tend to focus on high-level bureaucrats in governmental or quasi-governmental agencies (Robert Moses, David Lilienthal, Admiral Hyman Rickover) or on dynamic politicians (Abraham Lincoln, Lyndon Johnson). Another case study approach to entrepreneurship is through the use of issue histories, which analyze the role of policy entrepreneurs in the development and implementation of new public programs (Beam 1989; Kingdon 1984; Milward and Laird 1990).

More recently, formal models of leadership have been presented (e.g., Bianco and Bates 1990; Calvert 1987). These formal theories focus on agenda setting, strategic manipulation, and the incentives of leadership. Riker's (1986) notion of "heresthetics" has been particularly important in stimulating this more systematic approach to the study of leadership by political scientists. More theoretic than most biographies or issue histories, yet not fully "formal," Riker's work shows how a political entrepreneur can advantageously transform existing political coalitions, especially by adding a new dimension to political debates. Importantly, this line of research transforms the notion of entrepreneurs from the study of heroic figures to the study of a larger class of individuals who help propel political and policy changes.

Most case studies lack generalizability, and formal models are necessarily abstract and fairly simple compared to real political situations. To understand political entrepreneurship we need to further develop elements of a theory of entrepreneurship and empirically test these theoretical expectations. We shall synthesize aspects of an economic approach to entrepreneurship with concepts used in political science and apply the results to local government.

We argue that there is a population of potential political entrepreneurs distributed across local governments. The size of the local population with entrepreneurial skills and ambitions is a function of the characteristics of the community, such as its income and education level (Ricketts 1987; Ronen 1983, intro.).[1] While we cannot observe the distribution of potential entrepreneurs, our research does identify communities in which entrepreneurs are found. Our empirical research goal is to identify the conditions that affect the probability with which an entrepreneur emerges in any given community.

Potential entrepreneurs have energies and talents that they could invest in alternate spheres of activity. The rate at which they are attracted to the local *political* environment (as compared to some other domain) is a function of the costs they face in entering the political arena and the benefits they garner if they succeed as political entrepreneurs (Ricketts 1987). The level of these costs and benefits are embodied in measurable facets of local government. For example, we shall argue that costs are a function of the collective action problem entrepreneurs face and the ease with which these problems can be solved. Similarly, the benefits (or "profits") entrepreneurs reap are a function, among other things, of the budgetary slack of their local community, which affects the entrepreneur's ability to reallocate resources to achieve the policy goals held by the entrepreneur.

These factors enter the decision calculus of the *individual* entrepreneur. But the decisions of these individuals have wider impacts. On a more *systemic* level, entrepreneurs can affect the political equilibrium in metropolitan areas. For example, within a community an entrepreneur can transform the nature of political debates (Riker 1986; see also Schneider and Teske n.d.). Across communities, the entrepreneur can change the service/tax mix found in communities facing each other in the local market for public goods (see Tiebout 1956; Schneider 1989).

We shall turn first to the conceptually more difficult issue of entrepreneurs and equilibrium, then to an investigation of entrepreneurial profits and the difficulties entrepreneurs face in organizing a collective base to launch entrepreneurial policies. Finally, using observations from a large set of suburban municipal governments, we shall empirically address the question, Under what local conditions are political entrepreneurs more or less likely to be found?

HOW ENTREPRENEURS AFFECT MARKETS

To understand how entrepreneurs affect markets and politics, we begin by considering private sector entrepreneurs, comparing the role of the entrepreneur in the private market to the role of political entrepreneurs. We are directly concerned with entrepreneurs in what we call the local market for public goods, in which local services are delivered by multiple municipal governments in packages that are paid for using a revenue system in which local property taxes are central.[2]

While we develop a variety of theoretical issues that underlie the study of political entrepreneurship, two are central to the empirical analysis we present later. First, we are concerned with the concept of "opportunity": What conditions lead a potential entrepreneur to become active in the local political arena? Most economic theories of the entrepreneur argue that the pursuit of profits drives the entrepreneurial process. Hence, we are interested in identifying the conditions that affect potential profits in the local political arena and thus affect the likelihood that a political entrepreneur will be found. A second issue centers on the organizational milieu of communities that makes entrepreneurship more or less likely. We tie these conditions to the ability of the entrepreneur to solve problems of collective action, problems that are far more critical to the political entrepreneur than to most economic entrepreneurs.

Entrepreneurial Profits and Market Equilibrium

Perhaps the most common focal point in the recent economic literature on entrepreneurship is the emphasis on the "discovery" of market opportunities by "alert" individuals (see Kirzner 1985). From this perspective, entrepreneurs engage in the act of "creative discovery"—they try to take advantage of newly *discovered* or newly *created* possibilities in order to earn entrepreneurial profits (Casson 1982; Ricketts 1987).

In their drive for profits, private entrepreneurs discover and exploit new opportunities through arbitrage, speculation, or innovation. Entrepreneurs are driven by personal motives and the maximization of profits, but their activities affect the overall market in which they operate. Disagreement over the degree of independence of entrepreneurs distinguishes the Austrian school's approach to the study of entrepreneurs from that of the neoclassical "establishment." Moreover, economists do not agree on how entrepreneurs affect market equilibrium.

To summarize a large, sometimes difficult, and often contentious literature, entrepreneurs are not central to much of neoclassical economic analysis. Hayek believed that neoclassical economics neglected entrepreneurs because its core concept of market equilibrium assumes full information, leaving the entrepreneur little scope for innovative activity (Hayek 1949; Kirzner 1985). According to Kirzner, the "neoclassical approach has . . . squeezed the real-world entrepreneur back into the neoclassical full-equilibrium box" (1985, 8–9). In the neoclassical approach, entrepreneurs are "called forth" by conditions in the market in a systematic, if not necessarily predictable, process. In contrast, in the Austrian tradition, entrepreneurs have greater independence of action, and market outcomes are less predetermined. However, in both schools of thought, the relationship between entrepreneurs and equilibrium is central.

On one hand, entrepreneurs can be viewed as seizing opportunities in the market to move it toward equilibrium. While Kirzner's entrepreneur is defined by an "alertness to disequilibrium" and independently injects new elements into markets, ultimately, for Kirzner,"—the changes the entrepreneur initiates are always toward the hypothetical state of equilibrium" (1973, 73). In contrast stands Schumpeter's idea of creative destruction (1942; see also Shackle 1972). For Schumpeter, entrepreneurial activity is most likely to emerge when a market is approaching equilibrium. In this evolving situation, the successful entrepreneur engages in a series of innovations that move the market first toward disequilibrium and then, ultimately, to a new market arrangement that is more profitable for the entrepreneur.

Political scientists studying entrepreneurs are struggling with similar issues. We have very little information about variation in the supply of political entrepreneurs, that is, how and why political entrepreneurs are called forth. Kingdon's work (1984) may be the best current description of how entrepreneurs market their policy ideas to national decision makers. Kingdon describes policy entrepreneurs as constantly shopping around for windows of opportunities through which to shove their preferred policy ideas (also see Walker 1981). In a similar vein, Elder and Cobb (1983) focus on the importance of policy entrepreneurs in agenda setting.[3] In an extension of this approach, Baumgartner and Jones (1991) argue that the outcome of political debates can vary with the arena or "venue" in which debates occur. Because the federal system of the United States presents so many alternate venues, policy entrepreneurs can maximize their likelihood of success by carefully shopping around for the most favorable available venue. While these works focus on the role of entrepreneurs, they do not systematically identify the conditions under which entrepreneurs emerge.

Political scientists also need to explore further the relationship between entrepreneurs, innovation, and equilibrium. Since the concept of equilibrium is at the core of so much economic analysis, the extent to which entrepreneurs move private markets toward equilibrium or disequilibrium is theoretically critical for their enterprise. But the concept of equilibrium is also important in the study of entrepreneurs in *political* marketplaces.

Again, Riker's position is critical. Riker (1982) argues that political situations are normally characterized by an underlying disequilibrium of tastes. His contention is supported by a number of experimental and formal analyses that have severely restricted the conditions under which equilibrium can be expected. The fact that stable "equilibrium" outcomes are empirically evident despite the underlying dynamics of instability presents a conundrum for political scientists. Shepsle (1979) was among the first to show how institutions can induce stable outcomes despite the forces pushing toward disequilibrium. More recently, using field studies exploring the use of common property resources, Ostrom (1990) has shown how a wide variety of societal arrangements can overcome strong inherent tendencies toward disequilibrium driven by conflicts in tastes and strategies.

Thus, the idea of institutionally induced equilibrium provides a line of argument that addresses one part of the equilibrium/disequilibrium conundrum. But this makes Riker's argument linking politicians and equilibrium an even more central issue that must be addressed in any theory of entrepreneurship. For Riker, while equilibrium may appear in political arrangements at any given point in time, such an equilibrium is inherently unstable. This presents openings for Riker's "heresthetician"—an entrepreneurial political leader who "probes until he finds some new alternative, some new dimension that strikes a spark in the preferences of others" (Riker 1986, 64). Riker argues that through agenda control, strategic behavior, and (most importantly) the introduction of new policy dimensions to political debate, the heresthetician can break up institutionally induced and maintained equilibria to create new and more profitable political outcomes. Riker's heresthetician thus resembles Schumpeter's entrepreneur: both engage in creative destruction, tearing apart existing political–economic arrangements in order to create new ones.

Like Schumpeter's entrepreneur, Riker's heresthetician stands to profit from the successful introduction of new ideas or policies into a market that appears stable to others. After the disruption caused by the innovation, a new stability may emerge. But Riker believes that lurking behind this new stability is still the fundamental disequilibrium of taste that can then be exploited by the next generation of entrepreneurial politicians. Similarly, for Schumpeter, as the market absorbs the last round of innovations to approach equilibrium, the potential for entrepreneurial profits increases, giving rise to yet a new generation of entrepreneurs. Empirically, the outcome of innovations pushed by Schumpeter's entrepreneur or by Riker's heresthetician may thus resemble a *punctuated equilibrium*—a process in which institutions are characterized by long periods of stability punctuated by short bursts of rapid change (Krasner 1984). In the realm of politics it is important to remember that during these periodic disruptions, not only are the present politically determined allocations of resources questioned, but the fundamental rules of the game may also be challenged.

In the arena of local governments with which we are ultimately concerned, governments are in competition with one another and use their taxes and services to lure desirable households and business firms (Peterson 1981; see Schneider 1989). In the resulting competitive market, some forms of local political entrepreneurship may temporarily create disequilibrium, upsetting existing local public service and tax arrangements and altering the relationship between communities in a region. These innovations are often then absorbed and institutionalized into new political arrangements and a new balance between communities emerges. As we shall argue, the tax/expenditure limitations movement of the 1970s (e.g., Clark and Ferguson 1983), the concern for economic development policies of the 1970s and 1980s (Eisinger 1988), and the recent no-growth movement (see Logan and Rabrenovic 1990; Schneider 1992) may all be examples of entrepreneurship in local government that display some of the dynamics of punctuated equilibria.

ELEMENTS OF ENTREPRENEURSHIP: PROFITS AND COLLECTIVE ACTION

Before turning specifically to entrepreneurship in local government, we consider in more detail the concept of entrepreneurial profits. In particular, we explore two specific aspects of entrepreneurship that affect profits: private information and entry barriers.

Entrepreneurial Profits

Virtually every theory of the entrepreneur in the private sector emphasizes the importance of the profit motive. In such theories, the entrepreneur enters into market-making activities with a belief that the existing allocation of resources is somehow wrong and that profits can be made by reallocating them in a correct manner. However, a theory of political entrepreneurship based on the concept of monetary profit will not work. Most political entrepreneurs cannot be residual claimants to profits generated by their ideas (but see Niskanen 1975). This means that one of the most important motivating concepts in economic theories of the entrepreneur cannot be easily employed in the study of political entrepreneurs. Instead, a much more complicated utility function for the political entrepreneur must be

developed, including terms related to policy success and status. Formal models of political entrepreneurs pay more explicit attention to this issue than do case-study-oriented analyses. However, these formally derived utility functions are often so complex as to be beyond empirical confirmation. We shall develop two empirical indicators of potential profits for local entrepreneurs; but ultimately, political scientists interested in entrepreneurship must more fully develop the concept of political profits and its indicators.

Private Information. Understanding entrepreneurial profits requires analysis of the ability of the entrepreneur to restrict competition. One way to restrict competition is by maintaining control over information, excluding others from the data necessary to profit in a market.

Thus, one key to entrepreneurial success in the private sector is the protection of private information. Entrepreneurs glean some new piece of information from the environment and then bet on the future based on their insight. However, once this information becomes public, the entrepreneur loses the competitive edge and entrepreneurial profits are bid away. For example, in the process of arbitrage, the entrepreneur sees a discontinuity in the market. Acting on this insight, the entrepreneur enters into a series of profitable transactions. But as the entrepreneur succeeds, the insight becomes more widely known and competitors enter the field, ultimately driving down the entrepreneur's profits. Over time, the entrepreneur's innovation becomes a more or less standardized product, yielding only market rates of return.

Casson argues that to maintain the information advantage, the entrepreneur must be in close contact with *primary* sources of information (1982, 147). The more removed the entrepreneur from such sources, the more distortions enter into the flow of information and the more quickly the entrepreneur's advantage is dissipated. While this observation may have particular relevance for a study of local political entrepreneurs, Lewis's (1980) discussion of Admiral Hyman Rickover presents a clear example from the public sector (also see Bendor, Taylor, and Van Galen 1987 on bureaucratic models of information flow).

Still, there are serious theoretical problems when the concept of private information is carried over to the public sector. What, if any, private information can public sector entrepreneurs monopolize? The very nature of public entrepreneurship often requires that at critical times (especially during elections) the entrepreneur must share information with the public, without being able to protect the rate at which it disseminates to other competing politicians. In order to raise capital, the private sector entrepreneur may have to share his insights with others. But this process of revelation is protected by patent laws, franchising agreements, and so on. In the public sector, there is no similar legal protection on sharing information. And while policy entrepreneurs may emphasize the benefits of their proposals and try to hide the negative aspects (Arnold 1990), the public sector entrepreneur still must disclose more information than the private entrepreneur. In large part, this is because the private sector entrepreneur usually needs to raise capital to operate in the private market—and capital is controlled by a small number of individuals. Most public sector entrepreneurs need votes or support in public opinion to succeed. As Doig and Hargrove (1987) and others note, building and maintaining a political coalition is critical to the success of a political entrepreneur, and revealing information and ideas is central to this coalition process.[4] Indeed, ambitious local entrepreneurs may seek to have their innovations imitated elsewhere, to develop a broader reputation and to increase the rewards of their entrepreneurial activities.

Barriers to Entry. Entrepreneurs will inevitably face barriers embedded in the status quo as they push their innovations. If they are successful, entrepreneurs will themselves seek to establish entry barriers to protect their profits from new entrepreneurs. In the private market, such entry barriers may include maintaining secrecy over technologies and protecting ideas through patents, acquiring monopoly control over critical resources, and creating and maintaining goodwill with customers.

Political science has developed a literature on entry barriers that may be applicable to the study of the entrepreneurial process. While not phrased in terms of entrepreneurship, we know that incumbent politicians use many strategies that restrict competition, limiting the ability of challengers to enter political contests. Most of the tools that incumbents use seek to diminish the number and quality of candidates that might otherwise emerge to challenge the incumbent (e.g., Parker 1991; Tullock 1965). Of particular importance may be the ability of incumbents to advance the interests of their constituents by manipulating bureaucratic services and by controlling the flow of legislation that constituents and contributors need (Fiorina 1977; Parker 1991). The increasing use of personal contact as a form of political advertising also increases the entry barriers challengers face, especially since much of this contacting is subsidized by the government (Fenno 1978; Parker 1986). While the use of the congressional franking privilege is the most-often-cited advantage of political office holders, local government leaders also use the more limited perks of their office to send newsletters to constituents and to dominate local news coverage.

In the arena of local government, entry barriers may also be embedded in the "constitutional rules" that structure local political economies (Oakerson and Parks 1988). Rules governing the incorporation of new communities, the annexation of existing ones, and the types of services local government must supply—all affect the range of competition local government officials face and the opportunities for entrepreneurial activity (Burns 1991). The degree of frag-

mentation and diversity in service offerings in the local area can also affect the incentives and actions of incumbent politicians and the potential for entrepreneurial activity (see Schneider 1989).

The manipulation of entry barriers to limit challenges to incumbents has been most widely studied in terms of Congress, but the effort to protect political incumbency occurs at all levels of government. How local political entrepreneurs overcome existing entry barriers and the extent to which they then try to erect new ones is a critical dimension of a theory of entrepreneurship in local government.

Entrepreneurs and the Collective Action Problem

Organizing collective action in any domain requires that the well-known problems of creating and maintaining organizations be overcome (Frohlich and Oppenheimer 1978; Moe 1980; Olson 1965; Taylor 1982). The stereotypical private sector entrepreneur works alone or in a small organization, thus solving collective action problems by avoiding collectivities. However, a public sector entrepreneur is much more likely to need a collective *group* foundation to survive and prosper in the political marketplace.

Those political entrepreneurs acting in the realm of electoral politics must use rhetoric, the manipulation of public policy goals and the distribution of selective incentives to identify and mobilize a winning electoral coalition. In this electoral domain, in addition to the problems of group formation and maintenance, the entrepreneur faces biases in the distribution of resources and the generation of selective incentives necessary to create new alliances in local politics (see, esp., Stone 1989).

Second, compared to the private sector entrepreneur, public entrepreneurs may be forced to deal with large-scale formal organizations. Lewis argues that public entrepreneurship *begins* in an organizational setting and that the public entrepreneur develops a career both using, then ultimately transforming, the practices of large-scale public organizations: "The public entrepreneur, somewhere during his career, comes to understand that the *large complex public organization is the most powerful instrument for social, political and economic change in the political universe*" (1980, 238; emphasis original). Recent work on "intrapreneurship" (e.g., Pinchot 1985) shows how private sector corporations try to meld entrepreneurs into a complex organizational environment. Similar developments may be occurring in public bureaucracies. While, more generally, we believe that the possible avenues for public entrepreneurship are more diverse than the managerial–bureaucratic route with which Lewis is concerned, a fundamental difference may still exist between the public and private sectors in the size of the organizations that entrepreneurs face as they push their innovations and policies.

ENTREPRENEURS IN LOCAL GOVERNMENT

In local governments, political entrepreneurs can be high-level unelected leaders, such as city managers; elected politicians, such as mayors or members of city councils; leaders of established interest groups; or creators of new groups. These political entrepreneurs develop new and innovative policies and galvanize otherwise difficult-to-organize, dispersed citizens to support their policies.[5]

Others have discussed the role of bureaucrats and managers as entrepreneurs in government, most often at the national level of government. At the local level, Oakerson and Parks (1988) argue that public entrepreneurship often occurs in the context of professional associations that link managers across different local governments. Clearly, much entrepreneurial activity concerning local service efficiency is managerial—especially with regard to the introduction of new technologies within a given service domain.

In addition to this type of managerial entrepreneurship, elected politicians can also play critical entrepreneurial roles. One type of local political entrepreneur active in recent years focuses on the politics of growth. These growth entrepreneurs can be divided into several subclasses. First there is the progrowth politician who advocates using the vast array of traditional tools of local government (such as tax abatements and debt financing) to lure business firms. These politicians had their greatest successes in the 1970s and early 1980s. But eventually, what has been called the "market for jobs" became so fully developed that most opportunities for political entrepreneurs advocating these traditional tools were identified, and entrepreneurial profits bid away, in the intense interlocal competition for economic development (Milward and Newman 1989; see Schneider 1992). Indeed, the use of many of the traditional progrowth tools of local government has been so routinized that they are now handled by a cadre of bureaucratic managers (e.g., Blair, Fichtenbaum, and Swaney 1984; Eisinger 1988). This process replicates that of innovation in private markets: competition leads to the dissipation of initial high entrepreneurial profits, and entrepreneurial leadership is replaced by routine bureaucratic processes handled by managers.

In contrast to the progrowth politician advocating the traditional tools of economic development, a new class of growth entrepreneurs is now emerging. Politicians in subnational governments are now advocating a whole new range of what Eisinger (1988) calls demand-side policies to promote economic development. This evolution in policy tools is congruent with a theory of entrepreneurship in local government. As the last set of innovative tools became routine, the payoffs for these now traditional policies dwindled. In turn, a new generation of political entrepreneurs create and experiment with policies with potentially higher rewards. But to the extent that these policies

are successful, political entrepreneurs will continue entering the local market advocating similar policies. Ultimately, the political profits attached to these innovative policy tools will decline; and they, too, will become standardized bureaucratic products. While this new generation of entrepreneurs advocating innovative approaches to economic development are pushing the boundaries of existing policies, they still support local growth. Even more radical challenges are emerging from antigrowth entrepreneurs—entrepreneurs whose political careers are built on questioning the very value of growth itself (Logan and Molotch 1987; Logan and Zhou 1980; see Schneider and Teske n.d.).

Yet another type of local political entrepreneur focuses more directly on services and taxes, emphasizing efficiency-enhancing reforms. Borrowing from Clark and Ferguson (1983), many of these "new fiscal populists" fit the profile of an entrepreneur; that is, they are politicians who challenge the existing service/tax arrangements in local governments by advocating policy reforms to reduce local taxes and improve the efficiency of local government services. Certainly, there were many such entrepreneurs in local government in the mid-to-late 1970s, but it is empirically unclear how open this field is at present for new entrepreneurs of this type.

In each case, the successful entrepreneur needs to create a political coalition by which to challenge the status quo. The new fiscal populist seized on dissatisfaction with rising local taxes and the middle-class predisposition for the norm of efficiency to challenge existing political/bureaucratic alignments. Often, these new fiscal populists not only challenged the political leadership in their communities but actually changed the rules of the game. For example, through initiatives and referenda these entrepreneurs created the need for extraordinary legislative or voting processes to allow higher taxes. In short, these entrepreneurs created and maintained a political coalition that challenged existing local tax policies and transformed the institutional rules of the politics of local taxes.

Similarly, to be successful, the antigrowth entrepreneur must fracture the existing progrowth political alliance found in many communities and create a new political orientation toward growth. Neighborhood and community organizations that are faced with rapidly accumulating costs of growth usually provide the organizational basis for the antigrowth entrepreneur. These increasing costs provide opportunities for alert antigrowth entrepreneurs to challenge the position of the progrowth politicians who have long dominated local politics (e.g., Elkin 1985; Logan and Rabrenovic 1990; Mollenkopf 1983). This alternate organizational base is often mobilized as the antigrowth entrepreneur introduces a new dimension in local debates over growth, a dimension that emphasizes the use value of land (Logan and Molotch 1987), replacing the traditional concern for its exchange value (see Schneider and Teske n.d.).

STALKING THE LOCAL POLITICAL ENTREPRENEUR

No simple theory exists to explain entrepreneurship in the private market. It follows that a theory of *political* entrepreneurship will necessarily be complex, drawing on work from a variety of perspectives and disciplines. We have developed a core set of ideas that must inform any theory of public entrepreneurship. These concepts include political profits (affected by the extent of entry barriers and the strategic use of information), challenges to entrepreneurs rooted in collective action problems, and the dynamic effect of entrepreneurs on political equilibria.

We believe that most components of our theory are empirically testable. For example, local governments in metropolitan areas can be modeled as interacting in a dynamic Tiebout-like game, in which they must fashion their services and taxes in a manner that makes them competitive with other cities (Peterson 1981; see Schneider 1989). A successful local entrepreneur can change the results of this competition, for example, by introducing more efficient services that affect the competitive position of a city relative to its neighbors. Over time, neighboring cities must respond, changing their own mix of services and taxes to reflect the lower taxes or better services a successful entrepreneur introduces. The extent to which these dynamic (and measurable) changes occur can serve as a test of the effects of local entrepreneurs in the local market for public goods. Similarly, the effects of entrepreneurs on intracommunity equilibria can be explored. For example, by integrating game-theoretic models of the interplay between incumbents and challengers (e.g., Harsanyi 1967–68; Milgrom and Roberts 1982) with Riker's insights and with the literature on the effects of the structure of local government on policy responsiveness (e.g., Grofman and Lijphart 1986; Morgan and Pelissero 1981), specific and testable predictions about the development of policy positions adopted by entrepreneurs and incumbents can be derived.

While we are moving ahead on these fundamental research questions, our present goal is more modest: we limit our empirical analysis to identifying the conditions that affect the emergence of entrepreneurs, concentrating on the first two elements of our theory. We thus address the emergence of entrepreneurs before studying in detail their specific strategies and their policy impact. Specifically, we argue that entrepreneurs are more likely to emerge when there are factors that make entrepreneurial profits more likely and where local conditions facilitate a solution to collective action problems.

To test these relationships, we surveyed municipal clerks in more than fourteen hundred suburban communities across the nation to identify the distribution of political entrepreneurs.[6] Municipal clerks are knowledgeable about the politics and political history of their communities, and we believe this makes them more-than-credible respondents.

Since asking city clerks to identify local leaders who are "alert to disequilibrium" or who engage in "creative discovery" stretches the norms of language (even if the phrasing would be more theoretically pure), we asked clerks whether, during the past several years there had been any individual in their community whose "policy proposals and political positions represented a dynamic change from existing procedures."[7] We received 963 responses, of which 257 (26.7%) nominated someone as a political entrepreneur.[8] Of these entrepreneurs, 43% were mayors, 26% were members of the city council, 23% were city managers or bureau chiefs, and the remainder were from business or from other private activities.[9] We found no significant regional pattern in the distribution of entrepreneurs.

We analyze the emergence of a local entrepreneur as a dichotomous (0, 1) variable (1 = entrepreneur reported in the community) and use probit analysis. In the analysis that follows, we thus identify objective local conditions that affect the probability of an entrepreneur's being found in a given community. Because our questionnaire asked clerks to name entrepreneurs who had emerged over the last several years, we use specific conditions of communities in the early 1980s to predict the emergence of an entrepreneur.

To correct for regional differences in the distribution of community conditions, all the independent variables have been adjusted by metropolitan means and standard deviations (that is, they have been turned into z-scores by Metropolitan Statistical Areas). Through this transformation, the independent variables reflect the standing of a community relative to its neighbors (Collver and Semyonov 1979).

The Effects of Local Conditions on the Probable Emergence of an Entrepreneur

We model the probability that an entrepreneur emerged as a function of local fiscal conditions, local budgetary patterns, and local demographic conditions. The fiscal wealth (tax base) of a community is a fundamental resource that should motivate potential political entrepreneurs. A strong tax base gives communities a wide range of policy options for the entrepreneur to pursue. Since communities with a strong tax base can provide good services at a moderate tax rate or moderate services at a low one (see Schneider and Logan 1982), a strong tax base represents a public resource entrepreneurs can use in pursuing their vision of the future.[10]

A second fiscal variable, the comparative local tax rate, is more likely to reflect the community's potential support for an entrepreneur. When local taxes are high relative to the taxes of other communities, an opportunity may exist for an entrepreneur to advocate innovations that increase the efficiency of local services, limit expenditures, or redirect patterns of growth that residents might associate with higher taxes. Thus, we hypothesize that both a higher comparative tax base and a higher tax rate will increase the probability that an entrepreneur will emerge in a given community.

Entrepreneurs will also be attracted by the possibility of redeploying local budgets to achieve the policy goals they prefer. Following Peterson (1981), we divide local expenditures into three categories: developmental, allocational, and redistributive.[11] According to Peterson, within the types of expenditures that constitute local budgets, allocational expenditures are the most free of political limits. In Peterson's analysis, cities have little leeway in their expenditure decisions over redistributive services (which they must avoid) and over developmental expenditures (which they must pursue in a nonpolitical professional manner). This leaves allocational expenditures as the domain over which local politicians have the most influence. From our perspective, high allocational expenditures create slack resources that increase public sector opportunities for entrepreneurial profits. These slack resources can be most easily reallocated by the entrepreneur to pursue preferred policies.

The probability of an entrepreneur's emerging should also be related to the demographic makeup of the community, because such conditions reflect the relative ease with which entrepreneurs can organize the citizenry into collective political action. One important variable affecting the ease of organizing for collective action is the stake that individual citizens have in local tax and service issues. We hypothesize that communities with a greater concentration of renters (individuals without a location-specific investment in property) will be harder to organize than communities with a larger concentration of more firmly anchored homeowners (Cox 1982).[12]

As we have noted, progrowth and antigrowth politics are increasingly important conflicts in local communities. Rapid growth can strain existing services, increase pressure on taxes, and mobilize local citizens who see the quality of life in the community changing. We thus hypothesize that rapid growth in a community will increase the likely emergence of a political entrepreneur.

We also believe that the growing racial heterogeneity of suburban communities may create the conditions for political entrepreneurship by providing a base for new political demands or by countermobilizing existing residents to the demands of a growing racial minority population. Specifically, we hypothesize that a larger black population can add a new racial dimension to local politics that can increase the probability of local entrepreneurial political behavior. We also include a suburb's distance from the central city as a control variable, since this condition is often directly correlated with a variety of economic and social conditions of suburban communities (Dye and Garcia 1978).

Table 1 shows the results of a probit estimation of the effects of these conditions on the emergence of a local entrepreneur.[13] In the probit estimation, the most important variable affecting the emergence of an

TABLE 1

Probit Estimates: The Effects of Local Conditions on the Emergence of a Local Political Entrepreneur

VARIABLE	COEFFICIENT	SE	PROB > |T|
Fiscal conditions			
Tax rate	.102	.059	.086
True value	.064	.063	.311
Expenditures			
Allocational	.151	.063	.017
Redistributive	−.092	.061	.134
Developmental	−.046	.061	.454
Demographic conditions			
Percentage renting	−.127	.061	.037
Growth	.104	.058	.072
Percentage black	.105	.058	.098
Distance	−.058	.060	.335
Constant	−.619	.054	.000

Note: N = 635; chi squared = 23.43 with nine degrees of freedom (p = .005). Percentage of communities with entrepreneur = 27.5%. All independent variables are z-scores, i.e., they are standardized by regional means and standard deviations. *Tax rate* is the effective local tax rate; the tax base is the equalized value of local property per capita. *Allocational* expenditures are the standardized per capita expenditures on allocational functions, 1982; *redistributive* expenditures are the standardized per capita expenditures on redistributive functions; *developmental* expenditures are the standardized per capita expenditures on developmental activities. See n. 11 for full description.
Sources: State and county tax reports; U.S. Bureau of the Census, 1982 *Census of Governments*, Finance, File A; 1980 *Census of Population* (Washington: Government Printing Office).

entrepreneur is the size of slack budgetary resources measured by allocational expenditures. The importance of this budgetary variable is followed by the size of the rental population, which, we argue, makes the collective action problem more difficult for the political entrepreneur. Using the appropriate one-tailed significance test, we find that the disruptions of population growth and increasing racial diversity also increase the probability of finding an entrepreneur. Finally, note that a higher tax rate does increase the probability with which an entrepreneur will be found—high costs being associated with opportunities for entrepreneurs. However, the tax base variable is not related to the emergence of an entrepreneur at any acceptable level of statistical significance.[14]

Table 2 reports the effects of each individual variable on the probability of finding an entrepreneur. Keeping all other variables at their mean (since they are z-scores, the mean is 0) and looking at the effects of a variable set at one standard deviation below, and then one standard deviation above, the mean, we can get a further idea of the importance of each variable. The range for each variable alone is relatively narrow. Allocational expenditures has the largest effect in changing the probability with which an entrepreneur is found, with a change in the probability of finding an entrepreneur increasing from .22 to .32. This is followed by percentage renting, growth, and percentage black. While the range for each variable individually is narrow, the cumulative effect of all the variables we measure is considerable. If all the conditions in our model are set at one standard deviation from the mean to reduce the probability of finding an entrepreneur, the likelihood of finding an entrepreneur is only .07.[15] When we reverse the signs on the variables to increase the likelihood of finding an entrepreneur as a function of the conditions we measure, the probability increases substantially, to almost .59.

ENTREPRENEURS IN THE LOCAL MARKET FOR PUBLIC GOODS

Entrepreneurs play an important role in the American policy process. While the effect of entrepreneurs has been most widely studied at the national level, we believe that many opportunities for entrepreneurial behavior exist in the thousands of subnational governments in the United States. We further believe

TABLE 2

Sensitivity of Probit Estimates: The Probability of Finding an Entrepreneur, Given a One-Standard-Deviation Change in Local Conditions

VARIABLE	−1 S.D.	+1 S.D.
Tax rate	26.5	30.2
True value	24.7	28.9
Allocational expenditures	22.0	32.0
Redistributive expenditures	29.8	23.8
Developmental expenditures	28.3	25.9
Percentage renting	31.0	22.8
Growth	23.4	30.3
Percentage black	23.6	30.1
Distance	28.7	25.9
All variables	7.2	58.7

Note: Sample mean = 27.5

the concept of entrepreneurship should not be limited to a few figures with national reputations. In addition, most studies of political entrepreneurs have focused on the actions of a single entrepreneur facing a specific policy situation. Neither the motivations of entrepreneurs nor why entrepreneurs emerge has been investigated in a systematic manner. As a result, most existing studies of political entrepreneurship are not cumulative. An overarching theoretical perspective on the political entrepreneur is needed, a perspective that will allow individual studies of entrepreneurs to be tied more closely to each other and that will increase our understanding of how entrepreneurship is embedded in the systemic process of political change. We present the beginnings of a theory that synthesizes elements of an economic approach to entrepreneurship with existing work on political entrepreneurs.

It is important to emphasize that we believe a *synthesis* of political and economic theory is required. The study of political entrepreneurs will be ill served by the wholesale importation of economic theories of the private sector entrepreneur. The study of public entrepreneurship requires different emphases and focal points from the study of private sector entrepreneurs. For example, a fully developed theory of political entrepreneurship will require the definition of an expanded utility function for the entrepreneur. In particular, compared to the analysis of private sector economic entrepreneurs, the study of political entrepreneurs will require a more complex approach to profits, with an explicit concern for the role of private information and entry barriers. The study of political entrepreneurs will also require a much fuller investigation of the relationship between the strategies of the entrepreneur and problems of collective action than is the norm in economics.

While most studies of political entrepreneurs begin with an entrepreneurial politician, a more complete theory of political entrepreneurship must begin one step back: What motivates the entrepreneur? What conditions systematically affect the probability that an entrepreneur will be found? While our indicators are not as neat as the monetized concept of entrepreneurial profits of economic theory, our data show that slack public sector resources attract entrepreneurial politicians. These resources provide opportunities for the entrepreneur to garner the local public sector's equivalent of profit. Similarly, given their concern for individual behavior, economic theories of the entrepreneur are not usually concerned with collective action. But our evidence shows that problems of collective action strongly affect the emergence of local political entrepreneurs.

In short, our theoretical concern for an integrated theory of political entrepreneurship provides conceptually richer insights into the role of entrepreneurs in the policy process than do the separate theoretical traditions in economics and political science upon which we draw.

Notes

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1. We do not know the actual size of the local population of potential entrepreneurs. Following the most recent literature on leadership, we believe the population to be much larger than implied by the biographical case studies on entrepreneurs, which tend to glorify their subjects as heroic, and which almost by definition makes them uncommon.

2. For a fuller description of the local market for public goods, see Schneider 1989.

3. For a recent summary of work building on Elder and Cobb, see Milward and Laird 1990.

4. Information asymmetries and the strategic revelation of information has become a topic of interest in the study of legislatures (see, e.g., Austen-Smith and Riker 1987; Gilligan and Krehbiel 1990). The insights contained in such studies of committees and legislatures, set in a particular institutional arena, do not easily carry over to the study of the entrepreneurial process with which we are concerned.

5. We are specifically interested in the role of individual political entrepreneurs in the development of local policies. However, these individuals are just one class of entrepreneurs found in the local market for public goods. For example, building on the work of Charles Tiebout, we are exploring how mobile households and business firms can also play entrepreneurial roles in the development of local policies. Moreover, political entrepreneurs can have radically different goals. As we argue later, some are concerned with fast growth, others with slow growth, and yet others with taxes. In other work, we are exploring differences between types of entrepreneurs; here we deal with the entire set of individual entrepreneurs.

6. We began our sample construction with all incorporated suburbs with populations greater than 2,500 in 1970 located in the 100 largest metropolitan regions of the United States (but no more than three metropolitan areas per state). This produced a set of over 1,400 suburbs in 55 metropolitan regions, of which clerks in 963 communities responded to our survey. However, limits on data availability further reduced the number of cases for analysis.

7. We recognize that this operational definition is fairly broad, encompassing entrepreneurs who change local politics, alter equilibria, develop and implement new ideas or policies, and so on. However, our operational definition is virtually identical to that developed by Roberts (1991), who has presented perhaps the most recent comprehensive review of the literature on entrepreneurship from a political science perspective. In our on-going research, using a combination of follow-up surveys, objective data from various census reports, and selected case studies, we are exploring in more detail what these entrepreneurs have done (with particular concern for the theoretically central issues of the role of entry barriers and the dissemination of information), how their actions and positions actually differed from previous policies, and what effects the entrepreneurs have had on local services and taxes.

8. Since this is the first large-scale survey of suburban municipalities, it is impossible to say whether the 27% rate of entrepreneurship is high or low. However, in a comparative study of economic development strategies in close to two hundred cities, Clarke (1990) finds "innovative" programs in 5%–21% of the cities surveyed. Since her numbers are for individual programs, it is not unreasonable to assume that a joint set of programs that might define an entrepreneur will be found in the proportion of cities we report.

9. Clerks were informed that the survey was part of a large nationwide study of political entrepreneurs. The questionnaire began with a series of questions about the local

government structure and local politics. Clerks were then asked to name someone who met our definition of an entrepreneur. The clerks who "nominated" an entrepreneur were then asked a series of open- and closed-ended questions about the entrepreneur's activities. Clerks could nominate one or more entrepreneurs in the 1980s. They were also given the opportunity to name any entrepreneurs from the 1970s.

10. The tax base is measured as the per capita true value of property in the community. The tax rate is the effective tax rate. These measures are adjusted for differences in assessment practices, using state and county reports.

11. Expenditures on business-related "developmental" services are measured by combining local per capita expenditures on streets and highways, sewers, sanitation, water transport, water supply, gas, electricity, and transit. Redistributive services are defined as health, hospitals, urban renewal, and welfare. Allocational expenditures are for "housekeeping" functions (general administration, general government, financial administration, and buildings) plus public safety (police and fire).

12. Of the variables we use in the following analysis, percentage renting and percentage black are from the 1980 census of population. Population growth is the 1970–80 rate of growth. Since 1990 census data are not yet available for individual communities, these are the only data available. Moreover, Donovan and Neiman (n.d.) show that the lagged rate of growth is more important than the current rate of growth in determining local growth politics. Distance is measured as the straight line linear distance between the outer boundary of the central city and the inner boundary of the suburb.

13. The number of cases in the probit estimate is less than the full number of cases for which we have information on the emergence of a local entrepreneur. Only the cases for which we have information on every independent variable is included in the estimation. Missing data on the fiscal variables (which are gathered not centrally, but from state and county reports) account for the elimination of most cases.

14. There is a high correlation (.41) between relative allocational expenditures and relative tax base. This is not surprising—a higher tax base leads to more resources available for expenditures on the relatively more discretionary local services encapsulated in the allocational category.

15. In other words, for variables with a positive coefficient, we estimate the probability of finding an entrepreneur while setting the variable at one standard deviation below the mean. For variables with a negative coefficient, we set the variable at one standard deviation above the mean. To increase the probability of finding an entrepreneur as a function of these variables, we reverse the signs. Note that since all the variables are z-scores, the value of the variable at one standard deviation from the mean is plus or minus one.

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SIZING UP THE PEACE DIVIDEND: ECONOMIC GROWTH AND MILITARY SPENDING IN THE UNITED STATES, 1948–1996

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We examine the relationship between military expenditures and economic growth in the United States from 1948 to 1990, in order to gauge the potential peace dividend. Our main results suggest that military spending is a significant drain on the economy. We then examine the implications of a restructured international system for U.S. military expenditures and their resultant impact on economic growth in the 1990s. Simulations of Democratic and Republican proposals for cuts in defense spending suggest increases in economic output of between 2.5% and 4.5% over the period 1993–96.

> With cold-war expenditure no longer needed, our gigantic government expenditures abroad could be reduced and our international deficit would probably disappear almost overnight. With the dollar no longer overvalued, militant expansion of monetary and credit policy could stimulate capital formation, [or investment,] to utilize the resources released from armament production. . . . America's potential and actual growth rate, far from depending upon war preparations, would be markedly increased by an end to the cold war.
>
> Paul Samuelson, *Economics: An Introductory Analysis*

The recent collapse of the Soviet Union has opened the door for a reconsideration of the role and obligation of the United States in a post–cold war international system. As Paul Kennedy argued, the United States has been on the horns of a dilemma (1987, 532–35). It has extensive political commitments that "require" large resource commitments to the military. At the same time, (relative) industrial decline, coupled with the twin problems of massive external and internal debt have all suggested the economic "fall" of the United States, particularly in terms of declining competitiveness and stagnating economic growth. Furthermore, the increased levels of economic activity and economic productivity of Europe, as well as Asia, have made the commercial world increasingly competitive. The dilemma has the potential of being resolved, however, as diminished military threat poses an opportunity for restructuring U.S. military commitments.

Concurrent with the relative decline of U.S. economic health has been the elimination of the major military threat to the United States. This has brought about significant restructuring of U.S. military policy away from stability based on nuclear deterrence. What will replace the bipolar balance of terror is at present unclear, but it is clear that the total threat to the United States has diminished in remarkable ways over the last few years. This dramatically reduced threat has led to widespread agreement that a rethinking of U.S. defense policy is required. Many have called for a significant downsizing of the U.S. military. President Bush noted in the 1992 State of the Union Address: "Two years ago, I began planning cuts in military spending that reflected the changes of the new era. But now, this year, with imperial communism gone, that process can be accelerated." Paul Tsongas has summarized much current opinion in declaring, "The basic economic reality we face in this country is very simple: the Cold War is over. . . . We can make deeper defense cuts and still maintain the world's strongest military" ("The Democrats on Defense," *Boulder Daily Camera*, 3 March 1992). Everyone seems to agree that the cold war is over; just how much of a dividend a restructuring of U.S. defense posture can produce is still in question.

Defense Secretary Cheney has asserted that the only real peace dividend is peace. On the other hand, many others have put forward the notion that a peace dividend may well provide a needed stimulus to the economy and overall U.S. global economic competitiveness. In one form, this argument targets other social programs as the beneficiary from the windfall, while others suggest that economic output itself will be enhanced. Deficit reduction and the stimulation of private investment also are targets of those wishing to spend the putative peace dividend.

Joshua Goldstein (1988) maintains that every 1% of gross national product (GNP) devoted to military spending "robs" the United States of about ½% of economic growth.[1] A recent International Monetary Fund study of 125 countries from 1972–88 concluded, "While some have argued that military expenditures enhance economic growth, there is no question that more efficient alternative means of using the resources exist that would provide greater benefits to the economy" (Hewitt 1991b, 20; see also Hewitt 1991a).

Results of contemporary research on the United States, however, are not so clear concerning the relationship between military spending (and the like) and economic activity. A number of important studies have addressed the trade-off issue and found evidence supporting the contention that the United States has paid an economic price for its military spending. Russett (1969, 1970) found evidence indi-

cating that defense expenditures displace investment, personal consumption, and state and local government expenditures; Russett's conclusions were based on the results of analysis between defense spending as a proportion of GNP and nongovernment spending proportions, rather than within the context of a macroeconomic model. Russett's later (1970) study found additional evidence supporting the argument that there was a strong bivariate trade-off between defense and welfare spending ratios within the United States; subsequently, he reexamined the trade-off hypothesis using a more sophisticated research technique and data from a longer time period and found no evidence of systematic trade-offs between defense spending and health and education expenditures (1982).

Mintz and Huang (1990, 1991) have studied the relationship between economic growth and defense expenditures over the period 1953–87. Recently, testing for externality effects, they found no statistically significant impact of defense spending on economic growth in the United States (Huang and Mintz, 1991). In their earlier studies, using a two-equation model of growth and investment, they found no direct relationship between military spending and economic growth; but they uncovered an "indirect" relationship between military spending and investment five periods subsequent (at the .10 significance level). Military spending in 1990, for example, would drive down investment levels in 1995. They concluded: "The findings reported in this study reveal the existence of an indirect, delayed effect of military spending on growth. To be more specific, we found that in the long run, lower military spending encourages investment, which in turn promotes economic growth. This effect is not immediate, however. Our analysis shows that it takes about five years for such an indirect tradeoff to begin to become manifest" (Mintz and Huang 1991, 1291).

In another study of defense spending and economic growth in the United States, Atesoglu and Mueller (1990) employed a similar neoclassical growth equation, focusing on elasticities. In this study, the economy is divided into a military and a civilian section, the latter including nonmilitary, governmental spending programs. Atesoglu and Mueller found that the elasticity of GNP vis-à-vis defense spending is inelastic and positive. This implied that growth in defense spending leads *directly* to increases in output.

We reexamine the question of the trade-off in the United States, using data from 1948 through 1990. Like others, we look at the defense–growth issue within a neoclassical economic growth model. However, we focus on the separate size and externality effects of military and nonmilitary expenditures. In so doing, we examine the multiplier effects of military and nonmilitary components of governmental spending to discern their separate impacts on the macroeconomic environment. In this way, we hope to provide a more accurate gauge of the economic impacts of defense reductions in the coming years.

A MODEL OF DEFENSE–ECONOMY TRADE-OFFS

Ram (1986) and many others since have used Feder's (1982) initial model to study the role of government spending in the economy. This is done by assuming that instead of an export and a nonexport sector developed by Feder, the macroeconomy is divided into a governmental and nongovernmental (or private) sector. Following this idea, we assume that there are two sectors contributing to national economic output, P and S, representing the *private* and *state* sectors, respectively. Capital, K, and labor, L, are the main inputs to each sector, although output of the state sector S, is also an input to the private sector.

$$P = F(K_p, L_p, S)$$

$$S = G(K_s, L_s)$$

where F and G are real valued functions; subscripts refer to sectors.

Biswas and Ram (1986) were the first to use this set of assumptions to study the impact of military spending on economic growth by substituting a military and a nonmilitary sector for Feder's original export and import sectors. Ram (1986) was the first to apply these ideas to a state and a private sector. Building on Ram's application, Mintz and Huang decomposed state activity into military spending M and nonmilitary state spending N (1990, 1991; Huang and Mintz 1990). (For a full derivation of the model, see Ward et al. 1991.) The neoclassical growth equation to be empirically estimated is

$$\frac{\Delta Y}{Y_{-1}} = \alpha_0 + \alpha \frac{I}{Y_{-1}} + \beta \frac{\Delta L}{L_{-1}} + \left(\frac{\delta_M}{1+\delta_M} - \theta_M\right)\frac{\Delta M}{Y_{-1}} + \left(\frac{\delta_N}{1+\delta_N} - \theta_N\right)\frac{\Delta N}{Y_{-1}} + \theta_M \frac{\Delta M}{S_{-1}} + \theta_N \frac{\Delta N}{S_{-1}} + \epsilon. \quad (1)$$

National income is represented by Y, investment by I, and labor by L. This equation assumes that the contribution of M and N to growth via the externality (θ) and the factor productivity differential (δ) are not necessarily equivalent and associates unique δs and θs with respective components of M and N, an assumption that can be tested empirically.

The basic idea of the model is that changes in factor inputs (i.e., investment, labor and the various types of government spending) contribute directly to the growth rate of economic output, or national product. This neoclassical assumption is further modified by separating out the size effects of spending patterns from their externality effects. *Size effects* represent the contribution to national product of the overall spending levels. Government purchases, in effect, contribute to national productivity. *Externality effects* refer to the spillover contributions. Government spending on roads, for example, contributes indirectly to the efficiency and productivity of nongovernmental sectors. In general, if government activities developed out-

side of the market economy contribute to productive capacity they are said to represent an externality effect. (A broader discussion of these effects may be found in Cornes and Sandler 1986; Huang and Mintz 1991 explores the externality effects of defense spending on the U.S. economy using a three-sector production function approach.

We apply this model to the United States because we believe that externalities of defense spending are important in understanding the defense–economy trade-off and that the linkage between the military and the aggregate economy is sufficiently strong to be evident directly. This model also permits us to discern the separate effects of military and nonmilitary spending on economic growth.

A number of theoretical expectations flow from this model. The United States benefited from contributions to industrial infrastructure over the past decades. Accordingly, we expect that the impact of investment on changes in aggregate output should be significantly positive: $\mathcal{H}_1: \alpha > 0$. Furthermore, we posit that gains in labor inputs will be translated into output gains, that is $\mathcal{H}_2: \beta \geq 0$.

Ram (1986) found that the size effects of government spending are generally positive. However, military spending is often thought to be inherently nonproductive and a drain on productive resources in mature economies. Accordingly, we posit that military spending will have negative size effects on output while nonmilitary spending programs will generally have positive size effects. Thus (for m, n = military/nonmilitary governmental spending),

$$\mathcal{H}_3: \frac{\delta_m}{1+\delta_m} - \theta_m < 0$$

and

$$\mathcal{H}_4: \frac{\delta_n}{1+\delta_n} - \theta_n > 0.$$

Military spending programs are also frequently thought to bring spinoff effects as technologies and production processes initially developed for defense applications are brought easily and without separate research and development costs into the private sector. On the basis of this argument, we posit that the military spending of the United States will have positive externalities. Nonmilitary state spending programs are also thought to have positive externalities in some regards, since much state spending is aimed at providing infrastructure (e.g., roads, schools, national defense, and other public goods) that benefit commerce. For i = military/nonmilitary state spending, we hypothesize that $\mathcal{H}_5: \theta_i > 0$.

Before turning to a description of the data employed and our empirical results, it is important to catalogue, however briefly, some weaknesses of this approach. In adopting a production function approach that assumes an equilibrium between supply and demand, one necessarily glosses over the ability of the economy to function in disequilibrium. We ignore the major mechanisms through which the macroeconomy attempts to deal with disequilibrium: prices, the labor market, the responsiveness of investment to changes in output, and financial markets. A more complete model would provide a description of demand, as well as supply. Additionally, by adopting an economywide description of the United States, we ignore the existence of linkages that may be present in one sector (such as high technology) and absent in another (such as textiles). We also ignore regional differences, differences that may be stronger in some parts of the country and absent, weak, or opposite in the other locales. Available models of open economies are considerably more fine-grained, sophisticated, and advanced than the model presented here. However, the model employed herein is a simple and straightforward tool for evaluating the impact of military expenditures on economic growth, while permitting us to tease out the economic implications of various policy options for reducing defense expenditures under consideration.

DATA

Data measuring GNP, military outlays, government spending, and investment were all taken from the 1986 U.S. Department of Commerce publication, *The National Income and Product Account of the United States; 1929–1982*.[2] These measures were updated through 1990 from the *Survey of Current Business* (Dec. 1989 and 1990, July 1991, and Jan. 1992). Data measuring employed civilian labor force were taken from the U.S. Department of Labor (1989, 1991). All fiscal data were deflated into constant 1982 U.S. dollars using the GNP deflator and are given in calendar years.

EMPIRICAL RESULTS

Table 1 presents the results of the ordinary least squares estimates of equation 1. Studies by Mintz and Huang (1990, 1991; Huang and Mintz 1990) and Atesoglu and Mueller (1990) have argued that multicollinearity is a severe problem in formulations of the sources of growth equation. Accordingly, Atesoglu and Mueller drop consideration of the externality effects from their analysis. Mintz and Huang employ a *k-class* ridge estimator to circumvent collinearity problems and report results for ridge as well as for ordinary least squares estimators. The value of the ridge approach to avoiding problems associated with multicollinearity is, however, widely contested. (See Kennedy 1986 for a discussion of the salient points.) We do not adopt the ridge approach in this study because all of the ordinary least squares parameter estimates are unbiased and highly significant. Increasing their significance by introducing bias would provide no leverage in this instance.

Turning first to the diagnostic statistics, it is clear that the model demonstrates a high degree of fit.

TABLE 1

Military Spending and the Sources of Growth in the United States, 1948–90

COEFFICIENTS	ESTIMATOR: ORDINARY LEAST SQUARES			
	VALUE	ST.E.	T-STATISTIC	PROBABILITY
Constant	−.1053	.0479	−2.1981	.0344
Investment	.6940	.3148	2.2045	.0340
Labor	.6387	.3104	2.0575	.0469
Size effects of				
Military spending	−2.9773	1.2390	−2.4029	.0215
Nonmilitary spending	6.9957	1.4895	4.6966	.0000
Externality effects of				
Military spending	.7369	.2462	2.9934	.0050
Nonmilitary spending	−1.2712	.2888	−4.4017	.0001

Note: Lagrange multiplier tests are serial correlation, $\chi^2 = .464$, $p = .496$; functional form, $\chi^2 = .258$, $p = .612$; normality, $\chi^2 = 1.133$, $p = .568$; heteroscedasticity, $\chi^2 = .508$, $p = .476$; stability, $F(7, 9) = 2.723$, $p = .027$. Diagnostic statistics are $\bar{R}^2 = .7529$; $F(6, 36) = 22.3308$; log likelihood = 128.6642.

Over three-quarters of the variance in the rates of economic growth are explained by the model as derived. Moreover, tests of the structural specification of the model support the specification of the model, which shows no evidence of serial correlation, functional misspecification, nonnormality of residuals, or heteroscedasticity. These statistics increase our confidence in the model. Graphic presentation of the fit in Figure 1 also shows that the ups and downs in U.S. economic growth are well captured by this simple neoclassical model.

Turning to the specific coefficients, each estimated coefficient is statistically significant at the .05 level or better. (Robust standard errors are reported.) Most of the coefficients have the predicted sign. We hypothesized that the impact of investment on changes in output would be significantly positive. The estimated coefficient ($\alpha = .694$) suggests that investment has a positive short-term impact on increasing gross national product. Labor inputs also have a positive coefficient, $\beta = .64$.

We posited that military spending has negative size effects on GNP, while nomilitary spending has positive size effects. These speculations are upheld by the empirical analysis:

$$\frac{\delta_m}{1+\delta_m} - \theta_m = -2.98$$

and

$$\frac{\delta_n}{1+\delta_n} - \theta_n = 7.00.$$

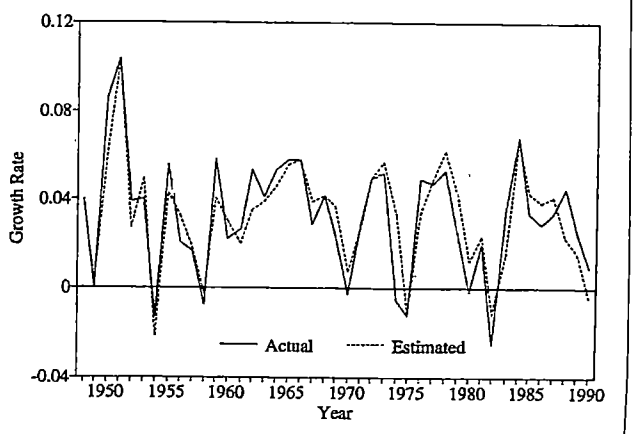
As military spending goes up, economic output will decline; as nonmilitary spending programs are augmented, one may expect positive effects on gross national product. Since military spending is generally smaller than nonmilitary spending, the overall size effect of government spending will be positive; but this hides the fact that military spending not only apparently reduces economic growth rates but may drain resources from nonmilitary spending programs that are likely to have positive size effects on the civilian economy.

Rather than observing positive externalities for both military and nonmilitary governmental spending, we find that the military has a positive externality as predicted ($\theta_M = .74$), while the externality for nonmilitary spending programs is negative ($\theta_N = -1.27$). Thus, it appears that military spending does generate positive spin-offs or externalities for the civilian economy.

Factor productivity of sector i is greater than that of the private sector whenever δ_i is positive. Using estimated values from Table 1 to solve for these parameters, we find that $\delta_N = -1.21$ for nonmilitary government spending and $\delta_M = -.69$ for military spending. Taken together, these coefficients suggest that on average, over the entire time span, *government* sectors have lower factor productivity in the United States than civilian sectors.

FIGURE 1

Actual and Fitted Values of Growth Rates in U.S. Gross National Product, 1948–90



SIZING UP THE PEACE DIVIDEND

Having developed and tested a model of the impact of both military and nonmilitary spending on the economy, we are in a position to forecast the impact that changes in U.S. government spending may have on the economy. During 1991 and 1992 there has been intense debate in the defense policymaking community about the appropriate future trajectory of U.S. defense spending.

One major proposal is that of the Bush administration, seen in terms of the revised Five Year Defense Plan (FYDP) put forward by Secretary Cheney in late 1991 and early 1992. This plan calls for defense spending to be reduced by a total of about fifty billion (current) dollars over the period 1992–96, although the level of defense spending in 1996 would be approximately ten billion dollars lower than current levels. This is widely referred to in the press as the "fifty billion" plan, and represents the view of the Bush administration as exemplified in the 1992 administration budget proposals that while some cuts in defense spending may be undertaken, the international environment remains threatening and uncertain, so that widespread cuts would be imprudent.

William W. Kaufmann and John D. Steinbruner have offered a timely criticism of this point of view in *Decisions for Defense: Prospects for a New Order* (1991). This volume offers a detailed analysis of the force structure and financial requirements of various strategic and operational postures that might guide U.S. defense policy in the years ahead. Kaufmann and Steinbruner offer an alternative conception, called *cooperative security,* that served as the basis for a different set of projections of defense spending, projections considerably lower than those put forward by the Secretary of Defense. The basic idea behind the Kaufmann and Steinbruner plan is to rethink and restructure U.S. defense planning on the basis of a collective arrangement among many national governments around the globe, rather than trying to use the defense budget to perpetuate U.S. military preponderance unilaterally. In much the same way that European Ministries of Defense are beginning to discuss a European Defense Union (Fontanel and Smith 1990), this plan would imply a subordination of U.S. traditional objectives of containment and deterrence to a cooperative strategy of engagement that would necessarily involve other important global powers.

According to Kaufmann and Steinbruner (in subsequent testimony to the U.S. Senate Appropriations Committee, 19 February 1992), the basic plan is premised on the notion that "participating military organizations are all on the same side, are all defensively configured, and are all primarily committed to providing mutual reassurance. This requires them to systematically limit offensive capabilities that might support ground invasions or might undertake long-range bombardment to achieve some political objective" (Kaufmann and Steinbruner 1991, 70–71). This type of arrangement would include the notion of an inhibition of firepower concentrations—that is, conventional arms limitations; a geographical restriction of the location of force concentrations; tactical air restrictions; full disclosure of research activities affecting weapons technologies; maintainence of small "residual" nuclear forces; and widespread and consolidated weapons export controls. Such an arrangement would require the participation of the United States, the Commonwealth of Independent States (especially Russia), the United Kingdom, France, Germany, Japan, and China at a minimum. Cooperative security permits Kaufmann and Steinbruner to tease out the force implications of such a strategy and from there to derive the budgetary implications of such a strategic restructuring over the next five to ten years.

These two different visions of the strategic future have been in considerable conflict among the Washington decision makers. The Pentagon's 18 February draft of the *Defense Planning Guidance for the Fiscal Years 1994–1999* criticizes the cooperative security approach and offers the strategic vision behind the administration's then-current FYDP. The guiding principal is "to maintain the U.S. position as the only military superpower in the world."[3] In addition to preventing the emergence of a major power dominating Western Europe, the territory of the former Soviet Union, East Asia, or Southwest Asia, this new plan promotes a military balance that will convince "potential competitors that they need not aspire to a greater role or pursue a more aggressive posture to protect their legitimate interests"; this will putatively assure that no rival superpower emerges in the military realm: "We will retain the pre-eminent responsibility for addressing selectively those wrongs which threaten not only our interests, but those of our allies or friends, or which could seriously unsettle international relations."[4] In short, this policy would reject the ideas of Kaufmann and Steinbruner and pose a distinct alternative not based on a collective or cooperative internationalism as the foundation for U.S. defense policy. In so doing, the required force structure would continue to be substantial and the potential for reduced military spending would be modest.

In these two basic plans, we see not only different strategic postures, but also budgetary implications that are quite distinct. In large measure, the administration plan has been backed by Republicans, and the Kaufmann–Steinbruner plan (or a version of it) has been backed by Democrats. Table 2 presents the specific top-line proposals in each of these two plans for annual defense spending 1992–96. It seems useful to tease out potential economic implications of these two different scenarios. To do so, we used estimated coefficients for equation 1 to structure the simulation. Since the formulation is dynamic, it produces growth rates and GNP for future periods, given exogenous data on investment, government spending, and labor. We used recent historical growth rates for these variables (the decade from 1980–89) but supplemented these by actual data for 1991 where available.

TABLE 2

Comparison of Five Year Defense Plan with Cooperative Security, 1992–96

YEAR	FYDP	COOPERATIVE SECURITY III
1992	295.2	295.2
1993	285.9	275.5
1994	282.0	256.8
1995	285.8	239.0
1996	289.1	222.4

Note: These data are for outlays, given in billions of current dollars. The Bush FYDP figures are from the Office of Management and Budget estimates of Total 050 expenditures. The Cooperative Security III scenario data appear in Steinbruner 1992. These data are a slightly accelerated version of the cooperative security option described in Kaufmann and Steinbruner 1991, 74ff).

(Complete details on the data and the simulations are available on request.) We used actual data on investment for 1991, from the *Economic Report of the President* Bush (1992), as well as official projections of government spending for the period 1991–93 in the same source. During this period, investment grew by 1.4% per annum, while the labor force grew by 1.7% per annum. During the same period, the growth rate of government spending was 2.9%.

Our simulation compares three defense spending scenarios. The first of these is a benchmark using the 1991 spending levels for each year until 1996. Our second defense spending scenario is that presented in the Bush–Cheney FYDP as of February 1992 as discussed in congressional hearings and summarized by the Office of Management and Budget. Our third military spending scenario is the plan known as Accelerated Cooperative Security III, a plan proposed by Kaufmann and Steinbruner in February 1992.

We also examine three basic options of what to do with the putative "peace dividend." First, we explore what would happen to the economy if savings from the defense reductions could be spent by other government programs. Second, we explore the implications of the so-called "Firewalls Bill" to prohibit savings in military spending from being used by other government programs; rather, the savings would simply disappear from the government budget and go toward deficit reduction. Our third scenario would take the savings from defense down-sizing and put them directly into private investment. This last option is based on the widely held notion that defense spending "crowds out" private investment. (Mintz and Huang 1990 offer a summary of this argument; see also Paul Samuelson 1964, 785). Private investment has been dropping in constant terms over the period from 1989 to the present. This scenario attempts to gauge the impact of a stimulation of investment as a result of defense downsizing. We note that the 1992 budget is the same in each scenario; it is only in 1993 that the implications of any of the proposed budgets will be felt upon the U.S. economy.

Table 3 presents the projected level of annual output (GNP) based on each of the three defense budgets in each of the three savings options over the period 1993–96. These projects are given in constant billions of 1982 dollars. Several things are striking about these results. First, it should be apparent that without a reduction of U.S. defense spending, real economic growth will be miniscule in the next few years, even assuming a recovery of investment to historical growth levels and an expanding job market. Projected 1996 GNP (in 1982 dollars) is only 1.5% higher than projected 1993 gross national product, under the benchmark scenario of no change in defense spending over the 1991 program targets. We note that actual national income has grown by an average of less than 1% per annum every year since 1989 (Bush 1992). Second, by using savings in defense spending to reduce the deficit, one can begin to see substantial effects on economic output, even in the short run, with either reduction plan, bringing about greater levels of economic growth. Thus,

TABLE 3

Growth Implications of a Peace Dividend, 1993–96: Projected Gross National Product in Billions of 1982 Dollars

USE OF PEACE DIVIDEND	1993	1994	1995	1996
Civilian government programs				
No change over 1991	4210.04	4198.14	4234.10	4275.54
FYDP	4218.87	4228.11	4274.39	4319.54
Cooperative Security III	4223.16	4245.02	4305.54	4357.62
Deficit reduction				
No change over 1991	4210.04	4198.14	4234.10	4275.54
FYDP	4218.87	4224.23	4264.06	4303.94
Cooperative Security III	4223.16	4242.66	4293.15	4341.99
Investment				
No change over 1991	4210.04	4198.14	4234.10	4275.54
FYDP	4222.50	4241.94	4299.63	4358.42
Cooperative Security III	4223.16	4248.81	4320.49	4400.81

whether a Democrat–sponsored cooperative security strategy is adopted or whether a down-sized military sculpted after Pentagon plans for the maintainence of U.S. military preponderance is adopted, the U.S. economy stands to benefit in the short run, with short-term growth rates higher for each reduction scenario. If defense savings can be used to increase investment (following the crowding-out thesis), substantial increases over current trends in output can be expected. However, even these drastic changes in the trajectories of defense spending and investment only suggest that real growth in GNP is going to be less than 2% a year over the foreseeable future. Finally, it is clear that the accelerated cooperative security option consistently has a greater stimulative effect on the U.S. economy regardless of what, if anything, is done with the savings. By 1996, U.S. economic growth under the cooperative security option will be close to 2% per annum, while the 1995–96 growth rate for the Bush–Cheney FYDP is projected to be 1.4% and under the no-change scenario, it would be about 1%.

SUMMARY

This research provides a number of interesting implications for the literature on the defense–growth trade-offs in the United States and for the current debate about the peace dividend.

First and foremost, consonant with theory and speculation suggesting the nonproductive nature of military spending in industrial societies, we find strong evidence that military spending detracts from economic growth. Recent studies have pointed away from a negative link between military spending and economic growth in the United States (Atesoglu and Mueller 1990; Congressional Budget Office 1990; Huang and Mintz 1990; and Mintz and Huang 1990 and 1991). Our results suggest that such a linkage not only exists but is composed of two distinct parts. On the one hand, we find that the size effect of military spending on the economy is negative. On the other, it seems clear that the military spending programs are able to generate spin-off benefits for private sectors of the economy (i.e., have positive externalities) independently of the size of military spending vis-à-vis total output. Furthermore, we have demonstrated that both military and nonmilitary governmental spending have lower factor productivities than the private sector of the economy, indicating that both aspects of government spending are less efficient than the private sector. Finally, we demonstrate that under each of the policy scenarios of reduced military spending, it is reasonable to expect a small but nontrivial boost to the level of gross national economic product by 1996 compared to a scenario of no reduction. This is consistent with the finding by Mintz and Huang (1990 and 1991) that the linkage between investment and growth may be a powerful conduit of the relationship between military spending and growth; our simulations suggest that by stimulating investment with the peace dividend one gains the largest macroeconomic benefit.

With respect to the current political debate over appropriate levels of military expenditures in the near-term future and the "peace dividend," these scenarios indicate that the consequences of the resolution of this debate have important implications for economic growth, especially for how it is allocated. For instance, the Democratic proposal would result in a GNP 1.6% higher by 1996 if the peace dividend is spent on deficit reduction, 2.1% higher if it is channeled to other aspects of government expenditures, and 2.9% if it is directed into investment. Corresponding numbers for the Bush–Cheney proposal are .6%, 1.0%, and 1.9%, indicating that the overall magnitude of the cut is also critical, supporting the assertions of Kennedy (1987) that maintaining a hegemonic military position carries with it economic consequences.

It turns out that cutting the defense budget is a question that is political *in the extreme.* Not only does the current discussion come in the throes of a presidential campaign, but also the current downsizing has local, as well as national and international, implications. Gordon Adams notes that "Congress is caught between a rock and a hard place. The rock is the desire for a peace dividend. The hard place is jobs in your district."[5] It is clear that local dislocations will attend to specific defense cuts. For example, the local economy around Monterey, California, will not be the same after Fort Ord is shut down. However, the results of our analyses suggest that on balance the U.S. economy will be stronger with defense cuts than without them. Larger cuts appear to help more, whether employed to offset the deficit, provide resources for civilian governmental programs, or stimulate capital formation. While it is politically challenging to support cuts that will affect local jobs, it does seem clear to all concerned that the military threat faced by the United States is significantly diminished. According to widespread accounts, the economic challenges faced by the United States continue to multiply both in number and complexity; these are made clear in the most recent *Economic Report of the President* (Bush 1992). Given this conjuncture of decreased military threat and increased economic threat, the downsizing of the military that seems to make strategic sense may well carry with it economic benefits that are substantial. However, a strong political will also seems necessary to obtain the peace dividend.

Notes

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1. See also Joshua S. Goldstein, "How Military Might Robs an Economy," *New York Times*, 16 October 1988.
2. We thank Alex Mintz for making the National Income and Product Account data available to us.
3. "Excerpts From Pentagon's Plan: Prevent the Re-Emergence of a New Rival." *New York Times*, 8 March 1992.
4. Ibid.
5. Quoted by Helen Dewar, "With the Cold War Won, Jobs Are Being Lost," *Washington Post*, 14 February 1992.

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CONTROVERSY

EXPLAINING SELF-DEFEATING FOREIGN POLICY DECISIONS: INTERPRETING SOVIET ARMS FOR EGYPT IN 1973 THROUGH PROCESS OR DOMESTIC BARGAINING MODELS?

How should we explain why a state sometimes adopts a foreign policy in one region that interferes with its concurrent policies elsewhere? In their article in the March 1989 issue of this Review, *Stewart, Hermann and Hermann proposed a three-level process model of foreign policy to explain such Soviet behavior towards Egypt in 1973. The analysis has continuing interest because it interprets the puzzling behavior as a manifestation of general problems of information processing in making foreign policy choices. Richard Anderson suggests that a two-level model of domestic bargaining better accounts for the causal sequence in Soviet-Egyptian relations and is in general more parsimonious. Margaret and Charles Hermann defend their substantive analysis and argue in any case for the complementarity of process and bargaining approaches.*

COMMENT

Why does a state sometimes adopt a foreign policy toward one country or world region that impairs the prospects for success of policies concurrently followed by the same state toward other countries or world regions? Stewart, Hermann, and Hermann (1989) offer a process model of foreign policy to explain one case of this puzzling behavior—the Soviet decision in January 1973 finally to grant Egypt's requests for offensive arms to launch the October War against Israel. This decision remains puzzling because Egyptian use of Soviet arms to attack an ally of the United States could be expected to, and ultimately did, jeopardize the détente that the Politburo was concurrently pursuing.

Even though the Soviet Union is now defunct, both the question raised by the authors and their proposed answer remain worthwhile. Many states adopt self-defeating foreign policies (Snyder 1991). The general model proposed by the authors conditions the observed Soviet behavior not on any peculiar institutional or ideological characteristics of the Soviet Union but on general problems of information processing that might distort foreign policy choices by any national leadership. The authors' model has the particular merit of linking levels of analysis by treating foreign policy as contingent on international events, not determined by them. They explain the Soviet decision by an empirical investigation of two processes: (1) the individual-level process by which each of six Soviet policymakers adjusts his attitude toward the issue in response to new information (in this case, Egyptian president Anwar Sadat's July 1972 decision to expel Soviet military personnel from Egypt) and (2) the group-level process by which policy responds to change in individual attitudes toward Sadat's request for arms. In the authors' model, the Politburo approves delivery of arms to Egypt, despite this decision's incompatibility with détente, because Sadat's expulsion order triggers reconsideration of Soviet Mideast policy without simultaneously motivating a reevaluation of East–West policy.

While flaws in information processing have often drawn attention as candidates to explain self-defeating organizational behaviors, another possibility is domestic bargaining. Belonging to the family of "two-level" or "nested" games (Putnam 1988, Tsebelis 1990), a model of domestic bargaining explains self-frustrating behavior in the international arena as a consequence of interactions among the decision makers in the domestic arena. A domestic bargaining model differs from a process model in analytic focus. While the process model draws attention to the processing of salient international events, the bargaining model draws attention to national leaders' purposive behavior.

One way to evaluate the relative merits of process models and domestic bargaining models is to compare their handling of the evidence in particular cases. In the case of Soviet arms for Egypt, the process model, drawing the authors' attention to Sadat's July expulsion order, induces them inadvertently to commit a causal inversion by attributing an event to an observably posterior cause. I begin with a discussion of the causal inversion. Then I present the alternative domestic bargaining model and lay out its historical reconstruction of the case. I conclude with a discussion of the relative merits of the two models.

THE CAUSAL INVERSION

To explain the Soviet policy reversal in granting Egyptian requests for offensive arms, the authors build a causal chain. They examine change over time in the attitudes expressed toward the Egyptian requests by four Politburo "oligarchs" (Brezhnev, Kosygin, Podgorny, Suslov) and two key advisers (Defense Minister Grechko and party foreign affairs specialist Ponomarev). Sadat's expulsion of Soviet military personnel in early July 1972 causes a shift in the declaratory stand taken by Kosygin in the third

quarter of 1972. The resulting disagreement over policy toward the Mideast causes Suslov to realign his public stance with Kosygin's. Realignment among the oligarchs produces approval of the arms deliveries in January 1973.

To detect changes in the oligarchs' and their advisers' expressed attitudes toward the Egyptian demands for arms, the authors rely on a quantitative content analysis of all "speeches and writings" by the six Soviet officials during 1970–72. They measure the ratio of "references" deemed "supportive" or "resistive" to Sadat's demand for weapons deliveries. A reference to advances in the Third World is considered supportive, while a reference to East–West détente is considered resistive.

One may question whether the ratio of references carries any significance. Any speech or article is not a concatenation of isolated references but a coherent text. A feature especially pronounced in Soviet political discourse was the tendency for speakers to communicate differing policy preferences not only by varying the number of references to agreed policies but also by varying the manner in which these references were combined (Ploss 1971, 120). For example, in each of two speeches given by the oligarchs Kosygin and Podgorny two days apart in October 1971, references to advances in the Third World considerably outnumber references to East–West détente. But Podgorny says that advances in the Third World will make détente feasible, while Kosygin says that détente is necessary for advances in the Third World. Because meaning can depend on overall organization of a speech, the ratio of references to one policy or another might vary without any change in the attitude expressed by the speaker.

Rather than relying on quantitative content analysis, the authors might examine Politburo members' explicit comments on the Arab–Israeli conflict. They would discover that the Politburo's specialist on Mideast issues during 1970–72 was Podgorny, whose role they (following Spechler 1986) overlook entirely. Although the authors score him as paying zero attention to the Mideast, in fact, 10 of his 70 speeches concern the Mideast almost exclusively; and he addressed it in other speeches. He commented on the Mideast much more than any other Soviet leader. In opposition to Sadat's demands for arms, Podgorny advocated a "political settlement" to be achieved not by direct U.S.–Soviet or Israeli–Arab negotiations but by building a diplomatic coalition that would isolate Israel and the United States and compel unilateral concessions.[1]

Until 9 December 1971, Brezhnev and Kosygin echoed Podgorny's calls for a political settlement. But at summits with Sadat in *February and April 1972*, from which Podgorny was excluded, Brezhnev and Kosygin agreed to communiqués that shifted Soviet declaratory policy in favor of Sadat's view that offensive war was a justifiable solution to the Arab–Israeli conflict (Glassman 1975, 94; Rubinstein 1977, 179; Porter 1984, 122, n. 19). Their speeches began to call for "liberation" of occupied Arab territories without repeating Podgorny's strictures against military means.[2] Podgorny, meanwhile, continued to advocate a political settlement in the Mideast; but he also lost his 1971 predominance in the public exposition of Middle East policy.[3]

Thus, the shift in Kosygin's declaratory stand on the Mideast conflict—the first link in the authors' causal chain—occurs at least five months before the event, Sadat's expulsion order, which the authors say causes his attitude to shift.[4]

A DOMESTIC BARGAINING MODEL

To explain the Soviet decision to arm Egypt, one might turn to a simple domestic bargaining model of foreign policy. The bargaining model posits that each national leader seeks to control policy on as many issues as possible. (It lets stand the question *why* leaders want control of policy—perhaps because they are ambitious, perhaps because they think their policy preferences are optimal for their state.) Each national leader is assumed to have two choices of action: (1) to insist on his or her own policy proposal and (2) accept some other policy proposal. If some leaders accept some other policy proposal than their own when one leader insists on his or her own, the "other" policy is assumed to be the insistent leader's own proposal; if all leaders accept some proposal other than their own, the policy is assumed to be some intermediate compromise. This set of assumptions produces a game matrix, shown in Figure 1 for the two-leader case.

If the assumption that leaders want to control policy is interpreted to mean that they would rather disagree than surrender policy to another leader's control but prefer a mutually acceptable policy to disagreement, this game is the familiar prisoner's dilemma. It is iterated over indefinitely many policy decisions over time. This particular prisoner's dilemma has the interesting feature of uncertainty about who plays last. For example, under the rules in 1972, Politburo members could die in office or fall ill and retire or the Central Committee could remove them from office. Consequently, this indefinitely iterated prisoner's dilemma is not subject to the usual logic of backward induction, and there are many possible equilibria (Tsebelis 1990). Among these equilibria is an "alternating sucker" pattern in which each leader accepts an insistent leader's proposal on one issue in return for obtaining acceptance of his or her insistence on some other issue (Hardin 1982; for a spatial formalization, see Austen-Smith and Banks 1990).

In order for policy to change in response to world events, the model further assumes the existence of an informal norm among national leaders. Any leader who proposes a policy must justify it by setting forth a public agenda of goals that the leader promises the policy will accomplish. According to the posited norm, information that the policy is not accomplish-

FIGURE 1

A Foreign Policy Bargaining Game

		Leader 2	
		Accept other proposal	Insist on own proposal
Leader 1	Accept other proposal	Mutually acceptable policy	Leader 2's policy
	Insist on own proposal	Leader 1's policy	Disagreement: no policy

ing this agenda eventually requires the proponent leader to stop insisting on the proposal.

HISTORICAL RECONSTRUCTION

This model can explain the Soviet reversal of policy toward Egypt. Podgorny disagreed with Brezhnev and Kosygin about both Mideast policy and East–West détente. They wanted to negotiate with the United States on both issues. He wanted to negotiate only with friendly regional states: the Arabs in the Mideast, and France, Italy, and the small states in Europe. During 1970, these policy preferences imposed no particular choice on this subset of Soviet leaders, since Podgorny's desire for separate negotiations with West European countries could be accommodated in a general program of exploring détente through bilateral diplomacy with a variety of Western countries. Beginning in early 1971, Israeli recalcitrance and Henry Kissinger's duplicity combined to frustrate Brezhnev and Kosygin's efforts for superpower cooperation in sponsoring a negotiated settlement in the Mideast (Breslauer 1983), while the European NATO members' unwillingness to negotiate security issues separately from the United States frustrated Podgorny's program in Europe. Consequently, both Podgorny and the Brezhnev–Kosygin pair could invoke the informal norm; a compromise would combine Podgorny's policy in the Mideast with Brezhnev and Kosygin's policy on East–West issues. (The other three officials examined by the authors—Grechko, Suslov, and Ponomarev—were inactive on the Mideast; and Brezhnev and Kosygin used concessions on other issues to buy off their opposition to détente.)[5]

During 1971, Podgorny's speeches advocated his political solution to the Israeli–Egyptian dispute. Sadat, having declared 1971 the "year of decision," immediately complained about the gradualist aspects of Soviet policy but was willing in the short run to accept proposals for efforts through the United Nations to achieve an Israeli withdrawal from Sinai. However, as 1971 wore on without results, Sadat began to argue that Israeli rejection of the UN initiatives justified his demands that the Soviets provide offensive arms for his preferred military solution. He also sought a settlement through direct bilateral contacts with Washington (Golan 1990, 76–79). His spokesman Mohammed Haykal (1975, 1978) accused the Soviets of perpetuating a condition of "no war, no peace" for their own benefit.

By the end of 1971, Sadat's arguments, his contacts with Washington, and Haykal's accusations constituted information that Podgorny's coalition for a gradualist policy was falling apart. In fact, in a speech for the visiting Sadat in October 1971, Podgorny tried to rebut such concerns. Consequently, at this stage Brezhnev and Kosygin could invoke the informal norm again. They would prefer to return to a policy of negotiating a Middle East settlement in tandem with the United States. However, Kissinger's unwillingness to enter serious negotiations on the Mideast, combined with his efforts to lure Sadat away from the Soviet alliance, made their preferred policy impractical. As Kissinger himself writes, "[Foreign Minister] Gromyko was experienced enough to know what I was doing" (quoted in George 1993). Consequently, Kosygin and Brezhnev proposed a policy of maintaining the Egyptian alliance (and frustrating Kissinger's plans in the Mideast) by conceding to Sadat that Egypt had the legitimate right to undertake war against Israel.

At the same time, Brezhnev and Kosygin recognized that an Egyptian attack on the Sinai would seriously interfere with their policy of U.S.–Soviet détente. To manage this tension within their policy, they took advantage of Egypt's known lack of hard currency by insisting that Egypt pay cash for any new arms deliveries (Central Asian Research Centre vol. 1, p. 208 and vol. 2, pp. 93, 246). This demand prevented arms deliveries during 1972. Resorting to

the only leverage he could imagine, Sadat ordered the expulsion of the Soviet military personnel in July 1972 (Sadat 1977). His move did not affect the Politburo bargaining; for he did not withdraw from the Soviet alliance, and keeping troops in Egypt was not part of Brezhnev and Kosygin's stated agenda. They willingly "overcomplied" with the expulsion order, withdrawing more troops and weapons than Sadat had asked (Golan 1990, 79; Roi 1975).

In January 1973, however, Saudi Arabia suddenly granted Egypt five hundred million dollars for the purchase of Soviet arms (Golan 1990, 84; Rubinstein 1977, 242). Brezhnev and Kosygin did not foresee this development; for their image of Arab countries distinguished "progressives" like Egypt from "reactionaries" like Saudi Arabia, said to be tacitly allied with Israel. Within days an Egyptian delegation left for Moscow to buy arms. If, as per my assumption, Brezhnev and Kosygin's purpose was to control policy on as many issues as possible, they would have acceded to the Egyptian demand for arms deliveries because this move would have enabled their policy proposal to retain control of the Mideast issue. Otherwise, they would have to admit that their policy had failed to maintain the Egyptian alliance and agree to some policy sponsored by someone else. Podgorny held them to their bargain with Sadat because he expected the arms deliveries to interfere with the détente that he opposed.

In short, the Soviet leaders chose a policy of delivering arms to Egypt that later interfered with U.S.–Soviet détente because this suboptimal policy in the international game was optimal in a domestic bargaining game.

CONCLUSION

Both process models and domestic bargaining models offer general explanations why states often adopt self-frustrating foreign policies. Process models attribute these policies to deficiencies in processing information about new events. In domestic bargaining models, leaders may be uninformed about circumstances and surprised by events; but they adjust their strategies reliably to new information. According to the domestic bargaining model, states adopt self-frustrating policies whenever these policies win in the domestic game, because a win in the domestic game is prerequisite for any policy to advance to the international game. In the case of Soviet arms for Egypt, the process model's focus on the impact of salient international events leads the authors to attribute the change in the Soviet leaders' attitude toward Mideast policy to an event, Sadat's expulsion order, that *followed* the observed change by five months. A domestic bargaining model avoids this causal inversion.

Bargaining models and process models are alternatives, not complements, because their explanatory logics make incommensurable assumptions about decision makers' use of information. The domestic bargaining model has the advantage of being much more compact than the authors' very elaborate process model. At the same time, the bargaining model preserves all the desirable features of the process model. Individual-level variables (preferences, perceptions of the international situation, information) combine with group-level variables (bargaining, norms) and state-level variables (rules affecting choice of equilibrium, number and identities of bargainers) to produce policy choices that react to international events but are contingent in the sense that national leaders can choose any of a wide variety of possible equilibria to the game. While the explanation of Soviet policy is consistent with evidence of oligarchic decision making by the Politburo, the model is well known to be general to policy choice, domestic and foreign, in other states.

This last point deserves emphasis. The propensity for strategic interaction within collectivities to produce behaviors suboptimal for the collectivity in larger social and natural environments is familiar from virtually every subfield of political science. Failures of information processing are inherently plausible as explanations for suboptimal behaviors, but they are inordinately difficult to detect against the background of the general disarray introduced into policy by collective choice.

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RESPONSE

Anderson, in critiquing the process model we employ to understand one puzzling case of a self-defeating foreign policy, claims (1) that the indicators we use do not adequately assess aspects of the process model, (2) that we have engaged in a causal inversion, and (3) that the domestic bargaining model is more parsimonious and better fitted to explaining the Soviet shift in early 1973 to granting Egypt's request for offensive arms. We would like to rephrase Anderson's criticisms into a set of questions and compare and contrast the process and domestic bargaining models in light of these queries: (1) Do self-defeating foreign policies reflect compartmentalized or competing attitudes among the leadership? (2) How do we know when attitudes change? and (3) Is the domestic bargaining model *too* parsimonious?

COMPARTMENTALIZED OR CONFLICTING ATTITUDES?

The differences between Anderson's and our approaches to assessing leaders' commitment to their preferences are at the heart of a debate among scholars studying the Brezhnev regime and lie at the heart of the questions that arise around self-defeating foreign policies. Two schools of thought have developed

to account for the Soviet government's simultaneous pursuit of essentially contradictory policy lines during the 1970s. One—like Anderson—sees a consensus among the Soviet leaders about their image of the Soviet Union as a global and imperial power that included within it the complementary elements of military growth, Third World activism, and détente (e.g., Glassman 1975; Head 1982; Rigby 1970). As Head has noted, the leadership practiced "separatism"—the Soviet Union had "the right to seek cooperation with the West in some matters, while challenging it in others" (1982, 41). Differences among Politburo members focused on how these various elements should be combined—how each might condition, or be conditioned by, the others—rather than on which should be the center of attention.

The other school—the one we have followed—proposes that the sharp differences in Soviet policy reflected similar differences among the leaders about the nature of Soviet objectives and appropriate methods of achieving them. Which objective should be the number one priority was at issue. Politburo members held different images of the Soviet role in international relations. Consequently, Soviet foreign policy could shift dramatically as the coalitions supporting different objectives aligned themselves in different ways. The data from studies by Spechler (1986) and Stewart, Warhola, and Blough (1984) persuaded us of the appropriateness of following the conflict theory approach and developing a ratio measure of commitment to preference (see also Dawisha 1979 and Kass 1978).

Spechler (1986) identifies two dominant images toward the United States among Politburo members during the years 1967–73. These images, she argues, had implications for the Arab–Israeli conflict and the decision to provide Egypt with offensive, strategic weapons in 1973. She labels the two the "cooperative" and "antagonistic" images. Members with the cooperative image "were impressed by the fragility of détente. . . . They did not want events in the Middle East (or in any other region) to cause or deepen mistrust and hostility between the U.S. and the U.S.S.R." (Spechler, 1986, 448). Those with the antagonistic view argued, "The U.S. must be actively countered and restrained, both militarily and politically. Highest priority must therefore be given to expansion of Soviet military power and political influence on a global scale. The Soviet Union must acquire friends and military facilities and maintain loyal allies wherever these would help to combat, contain, or undermine American power" (p. 450). In Spechler's study of the Soviet decision to give arms to Egypt, Politburo members held one or the other of these images, *not* both. In effect, the images were inverse, not conditional.

In support of Spechler's argument for the conflict between these two images in Soviet foreign policy during this time period, we note the content analysis of themes surrounding East–West relations done by Stewart, Warhola, and Blough (1984). References to détente and Soviet military power were the most prevalent in the 1970s when compared to all other themes (43% and 28% respectively), reflecting the importance of these two issues in Soviet foreign policy discourse. An R-factor analysis of all the themes across Politburo members indicated that these two foci were in competition with one another. Factor scores for individual Politburo members showed that the spokesmen for détente were not the spokesmen for increased military power, and vice versa.

The emphasis on either compartmentalized or conflicting attitudes has led Anderson and us to employ different techniques to measure commitment to preference. Anderson has used a more qualitative content analysis procedure in developing leaders' positions. He is as interested in noting when phrases describing policies and approaches change as in keeping track of how often a particular theme is repeated. He focuses on how the Politburo members link the various streams of Soviet foreign policy to deal with what appear on the surface as contradictory ideas. One result of this focus is his interest in conditional policies. We, on the other hand, have used a more quantitative content-analytic approach that looks for how much Politburo members emphasized certain themes relative to other themes. We have paid particular attention to which leaders have focused on which issues to facilitate our understanding of the positions each is likely to advocate in a decision-making process where there are basic disagreements over objectives.

HOW DO WE KNOW WHEN ATTITUDES CHANGE?

One of the important concerns of both Anderson and us is to determine when Brezhnev and Kosygin began to be influenced by the environment to change their attitudes regarding supplying Egypt with offensive, strategic weapons. Because these two leaders are classified as caretakers by our process model, we are interested in how contextual information shaped their positions. What environmental cues triggered a change has implications not only for the positions of these two leaders but for what happened in the group process as the Politburo worked to solve the problem. Ascertaining such environmental impacts is important for Anderson, as well, in determining when Brezhnev and Kosygin's views began to diverge from Podgorny's and, thus, when the bargaining equilibrium started to shift from Podgorny. Anderson argues that Brezhnev and Kosygin changed their positions in December 1971, or at least by February 1972, when they stopped advocating a political settlement. We focus on Sadat's two dramatic steps in the summer and fall of 1972 (expelling Soviet military personnel and returning "hat in hand" to seek Soviet support when overtures to the West were rebuffed) as catalytic agents for shaping Brezhnev and Kosygin's positions. In effect, Anderson believes that the change occurred from five to seven months earlier.

For Anderson, it is a change in the expressed attitude that took place by February 1972. Brezhnev and Kosygin stopped advocating a political settlement of the Arab–Israeli dispute and began shifting toward Sadat's position, indicating that means other than diplomacy could be used in dealing with Israel. Anderson refers to a shift in "Soviet declaratory policy." He argues that Brezhnev and Kosygin could shift their rhetoric and not interfere with their policy of détente with the West because they added the caveat that Egypt would have to pay hard currency for any new arms deliveries. They knew Egypt did not have sufficient hard currency to buy the weapons. Their bluff was called in January 1973 when Saudi Arabia provided Egypt with the hard currency for Soviet weapons; and the Soviets sold offensive, strategic arms to Egypt.

As Anderson describes the scenario, his choice of words for Brezhnev and Kosygin's positions seems quite appropriate—a change in Soviet "declaratory policy." The two Soviet leaders changed their public posture, but did they change their attitude before their bluff is called? Indeed, this change in public posture without a coterminous change in Soviet foreign policy behavior led to Sadat's frustration and his expulsion of Soviet military personnel in July 1972. The Soviet leadership appeared hypocritical—saying one thing and doing another. In a description of the communiqués issued from the series of meetings between Soviet and Egyptian leaders between February 1972 and July 1972, Rubinstein notes the increased tension and disagreement occurring as Sadat heard Brezhnev and Kosygin's words encouraging him to use other means than a political settlement to deal with the Israelis but saw no action in providing him with the military support he needed (1977, app. 4). As Rubinstein notes for the meeting on 27–29 April 1972, "Moscow expresses 'full support' for Arab efforts, though, 'the sides found it necessary to study again in a spirit of fraternal cooperation measures' for increasing the military potential of Egypt" (p. 362). At the meeting on 13–14 July 1972—just three days before Sadat's expulsion of Soviet military personnel, Rubinstein observes, "the brevity of Sidqi's visit suggested tension and disagreement" and "the mention of economic cooperation serves to highlight the absence of any mention of military cooperation or of Soviet efforts to increase Egypt's defense capability" (p. 363). In effect, as Anderson has recorded the events, the Soviet leaders' attitudes may never have changed. By providing hard currency to Egypt, the Saudis helped Sadat to call the Soviet bluff. As a result, Brezhnev and Kosygin were forced by the strategies they had used to maintain their position to act contrary to their preferences.

In our application of the process model to this case, we present a table with commitment-to-preference ratios for the Soviet leaders whose positions counted in this decision for each of five quarters beginning with the first quarter of 1972 (Stewart, Hermann, and Hermann 1989, Table 3). *Only* in the first quarter of 1973 are the commitment-to-preference ratios for three of the leaders more than one standard deviation above the mean for the Politburo and in the direction of supporting sending offensive weapons to Egypt (over .50). Two of them (Kosygin and Suslov) are leaders in the oligarchy at this point in time; that is, by the first quarter of 1973—*after* Sadat's two dramatic moves—half of the oligarchs whose positions counted have become advocates for supplying Egypt with such weapons. Throughout the period, Grechko, the defense minister, has scores one standard deviation above the mean—the lone advocate for supporting Egypt militarily. Interestingly, Kosygin's commitment-to-preference ratio increases dramatically in the third quarter of 1972 during the period when Sadat expelled Soviet military personnel (going over .50) and again when the decision to provide weapons is made. He appears to have been affected by what was happening to Soviet influence in the Middle East as a result of Sadat's behavior.

Brezhnev's scores remain an anomaly, staying close to the mean of the Politburo across the five quarters. We hypothesize that as a cuetaker Brezhnev may have been more interested in what was going on in the Politburo than what was happening in the international environment. He retained his position based on support among his colleagues in this body. Thus, when there was disagreement among members of the Politburo, we would argue, Brezhnev made a conscious effort to determine where everyone stood, wanting to reflect the consensus among the members and not be too different until the position of the group was fairly well defined. There is some support for this proposition in Valenta's (1979) study of the Politburo's decision to intervene in Czechoslovakia in 1968. Brezhnev sat on the fence between the arguing factions until a definite consensus began to emerge.

Thus, our data suggests that the change in Politburo members' attitudes, particularly Kosygin's and Suslov's, occurred in the early part of 1973 after Sadat's moves away from, and then back toward, the Soviet Union. The data also indicate that Kosygin's reaction to the expulsion of Soviet military personnel may have helped to begin a reevaluation of the situation among Politburo members. In any case, there appears to have been a growing consensus in the first quarter of 1973 to supply Egypt with offensive, strategic weapons. The knowledge of a change in the consensus among Politburo members probably led Brezhnev to acquiesce and agree to providing Egypt with the weapons Sadat wanted.

IS THE DOMESTIC BARGAINING MODEL TOO PARSIMONIOUS?

The process model asks three questions: (1) Whose positions count in making a decision on a particular issue ? (2) What are their positions on the issue under consideration? and (3) How are disagreements handled? As a consequence of these three questions, the process model examines decision making using three

levels of analysis: the individual, the group, and the state. In some interesting ways, the process model is a more inclusive model that provides the input information required by the domestic bargaining model. For example, Anderson does not question the part of the process model that indicates whose positions count—which Politburo members had influence and had to be included or consulted before a decision was made. He assumes this part of our model, as he concludes that the Brezhnev regime during the period 1970–72 was an oligarchy. The domestic bargaining model does not provide a mechanism for deciding whom to focus on as the bargainers in a decision-making body. They must be stipulated by the researcher. The process model includes a way to determine whose positions count.

Data from the process model are also useful in assessing which of the two positions the bargainers are likely to take in the domestic bargaining model (accept another proposal or insist on one's own proposal; see Figure 1). The advocate in the process model is equivalent to the bargainer who insists on his own proposal; the cuetaker is equivalent to the bargainer who is willing to accept another proposal. In the process model, using personality and organizational information, we are able to identify different types of advocates and what cues are likely to be valued by the cuetakers. Thus, there are advocates who push their positions *only* when the timing is right or their position is congruent with that of their organization, as well as advocates who persistently push their positions or their organization's agenda *regardless* of the situation. Cuetakers likewise differ on where they look for cues on appropriate behavior. Some focus on the particular situation, others on their patrons or clients, and still others on their organizations. Again, the process model provides the means for determining the positions needed—but not established—by the bargaining model.

Assuming Anderson's description of Podgorny to be correct, we probably have in this leader an advocate who was interested in pushing his position, perceiving only the information from the environment that supports what he believed was right. There was little flexibility when he was strongly committed to something. As we have already noted, the data in our study indicate that both Brezhnev and Kosygin were cuetakers who, though focusing on different kinds of cues, used situation-specific data in making up their minds, ever taking stock of context factors before assuming a position and cautious in jumping to conclusions without knowing what was going on around them at a particular point in time. In terms of the domestic bargaining model, then, using information from the process model, we would expect Podgorny to have insisted on his proposal in a competitive and aggressive manner and Brezhnev and Kosygin to have been willing to accept another's proposal and, most likely, seek a mutually acceptable policy. The data Brezhnev and Kosygin would use in seeking a mutually acceptable policy, however, would be different. Brezhnev would want to know what his colleagues were thinking; Kosygin would be interested in what was happening in the international environment regarding the particular issue under debate.

An examination of Anderson's historical reconstruction suggests the accuracy of these profiles. Throughout his description of what happened in the bargaining process during 1971–73, Anderson notes how events were driving Brezhnev and Kosygin while Podgorny continued to hold the position he began with.[6] Podgorny held to his original point of view, while Brezhnev and Kosygin responded to what was going on in the Politburo, as well as to Israeli recalcitrance, Sadat's growing frustration, and Saudi grants of hard currency to Egypt, in fashioning a policy that tried to respond to the situation that they perceived was facing them at the time.

The domestic bargaining model and our process model are most similar in stipulating rules for how Politburo members resolve differences and disagreements among themselves. In both, the models are focused on the dynamics in the group as members try to reach a decision. When those who count in the decision-making process disagree and come into conflict, how they go about resolving this conflict is at issue. Interestingly, the outcomes of the two models mirror one another. In the process model, "adopt oligarch's position" is similar to the case in the domestic bargaining model where Leader 1's or Leader 2's policies are selected. In the process model, deadlock can occur just as in the domestic bargaining model. And compromise is possible in the process model, matching "mutually acceptable policy" in the domestic bargaining model.

In actuality, the process model helps to spell out some of the activities in the group that underlie the domestic bargaining model. Whereas the domestic bargaining model searches for equilibria and builds on the rules of nested games, the process model arises out of the group dynamics and organization literatures where the emphases are, among others, on understanding majority–minority influences, building cohesion, achieving compromise and consensus, preventing deadlock, and developing decision norms. The process model offers ways to explain why certain equilibria are possible, given the configuration of roles and positions in a decision unit.

CONCLUSION

If our aim is to understand and explain how foreign policy decisions are made, it seems important to use a variety of models and methods to study self-defeating policies such as this Soviet decision. Only if we engage in these endeavors, can we begin to see how the models might interrelate, might explain different aspects of the decision process, might be relevant under different conditions, or might hold in different contexts. Moreover, only by taking different perspectives on the problem and making different assumptions can we gain a multifaceted view of

leaders' preferences, positions, and roles. The tendency to see models and methods as always in competition with one another with only one supposed to win and supersede the others does not facilitate such an exploration. We may learn more by comparing and contrasting the models, as we have done in this point–counterpoint analysis, than by focusing on one model to the exclusion of all others.

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Notes

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1. Factual assertions concerning the contents of Soviet leaders' speeches during 1970–72 are taken from my dissertation (Anderson 1989).

2. For Kosygin, see *Pravda*, 12 February, 10 April, 4 July, and 3 and 17 October 1972. For Brezhnev, see *Pravda*, 9 December 1971 and 19 March, 6 and 28 June, and 14 November 1972. Kosygin's speech published on 17 October 1972 coupled the call for liberation with a call for a political settlement as an alternative if possible; his speech on 25 October defined a political settlement as equal to an Israeli withdrawal from all Arab territory. Brezhnev's speech on 22 December adopted this same position.

3. For Podgorny's continuing advocacy of a political settlement, see *Pravda*, 13 October and 8, 9, and 14 December 1971 and 13 April, 7 July, and 15 September 1972. Note that Podgorny's three references to a political settlement during December 1971 coincide with Brezhnev's abandonment of this phrase.

4. The literature disagrees on the importance of the expulsion to the Soviet decision. Spechler 1986 and Porter 1984 concur with the authors; Dawisha 1981, Freedman 1978, Glassman 1975, Golan 1977 and 1990, and Roi 1974 and 1975 all attach much less importance to Sadat's action.

5. Grechko possibly preferred to grant Sadat's requests for arms, but the Politburo had an arrangement with the high command that the generals could have autonomy in military policy if they accepted the Politburo's right to decide larger foreign policy issues (Colton 1979; Colton and Gustafson 1990; Rice 1987). Suslov and Ponomarev concentrated on relations with world communist parties. Because communist parties were inactive in Egypt and Syria, the Mideast issue played little role in world communist politics.

6. See n. 3

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN DECEMBER, 1992

The following articles, research notes, and controversies have been tentatively scheduled for publication in the December, 1992 issue:

Paul Allen Beck, Lawrence Baum, Aage Clausen, and Charles E. Smith, Jr. "Patterns and Sources of Ticket-Splitting in Subpresidential Voting."

Christopher Colmo. "Theory and Practice: Alfarabi's *Plato* Revisited."

Ada W. Finifter and Ellen Mickiewicz. "Redefining the Political System of the USSR: Mass Support for Political Change."

Thomas Hartley and Bruce Russett. "Public Opinion and the Common Defense: Who Governs Military Spending in the United States?"

Ken Kollman, John H. Miller, and Scott E. Page. "Adaptive Parties in Spatial Elections."

Eileen L. McDonagh. "Representative Democracy and State Building in the Progressive Era."

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In the Abstract of the controversy "Question-Wording and Macropartisanship" in the June 1992 issue, p. 475, lines 6–7, the commenting authors should have been identified as MacKuen, Erikson and Stimpson.

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POLITICAL THEORY

We the People: Foundations. By Bruce Ackerman. Cambridge: Harvard University Press, 1991. 369p. $24.95.

This book of essays is an outline of, and introduction to, a larger work that will eventually include two more volumes. The guiding and very persuasively argued thesis set out here is that the United States is a "dual democracy" in which "We the People" assert ourselves occasionally in acts of revolutionary constitutional politics, while mostly it is governed by the normal politics of its elected representatives. The Supreme Court acts as the agent of "the People" during the long calm periods by preserving the Constitution against the "erosion" of normal politics (p. 87). As such, it is an eminently democratic institution. This argument is not entirely new. I first heard a version of it in the early 1950s in the lectures of Robert McCloskey, who used to say that the American people wanted both change and continuity and that judicial review guaranteed the latter. It was even suggested that this was a realization of Rousseauian democracy in which the will of all, or at least of the majority, prevailed most of the time, to be checked when necessary by the higher general will of the people.

Ackerman would probably reject any such introduction of European philosophy into the interpretation of American constitutional history. He, in fact, begins with an outburst of truly Jacksonian populism, denouncing just such theoretical European distortions of the native experience, and especially of the majesty of "We the People." In truth, his own version owes more than a little to Hegel. In what he calls his "story," institutions have a historical logic and dialectic of their own, quite apart from the preferences and beliefs of the individual agents who at any moment administer them. This is, moreover, especially true of the courts, because Ackerman is wholly convinced that theirs is an autonomous history within America's culture and that bench and bar form a discrete community that has historically followed its own path (p. 39). Unfortunately, he merely asserts this conventional common lawyer's ideology without offering any arguments in its favor—a great pity, because it simply imposes an excessively narrow framework upon his otherwise ambitious project. As it is, Ackerman is only "gesturing" toward the social sciences and history (p. 60). He is certainly not writing for political scientists, since in the second half of the book he openly addresses readers who have never read, much less thought about, *The Federalist Papers* (p. 200).

In spite of these oddities and of far too many rhetorical flourishes, the first part of this book is extraordinarily interesting, intelligent, and lucid; and it is sure to raise the intellectual level of the whole field of constitutional theory. In Ackerman's new "story," American constitutional history is neither a seamless web nor a succession of ups and downs. Instead, it has been run on two tracks, one revolutionary and the other adaptive. There have, he argues, been three acts of radical adjustments between generations: the ratification of the great document in 1787, the three Reconstruction Amendments, and the New Deal. The latter differs from the first two in that it was not preceded by violent warfare and that it amended the Constitution not by following the procedures of Article 5, but wholly by judicial interpretation, after the "switch in time." It was a genuine revolution, nevertheless, because it created a new activist state and, with its acceptance, a renewed and aggressive judicial emphasis on the Bill of Rights. Jacksonian democracy, on the other hand, does not qualify as a revolution, because it was "not sweeping enough", failed to promote the nationalization of politics, and its tribunal presidency did not last (pp. 77–80). Judicial doctrines did not really shift, either. One might, of course, consider that the adoption of universal white manhood suffrage was a major democratic achievement, as was the recasting of democratic ideology, both of which set America forever apart from Europe. This was, however, played out in the states, where most of the political action was, in any case—which is why it can have no part in Ackerman's exclusively national "story."

Beginning, as one must, with the Marshall Court, Ackerman shows how it preserved the essence of the new Constitution against the localizing tendencies of normal politics. It sustained the centralizing political thrust of the Constitution, as well as its guarantees of rights, in the memorable *McCulloch* and *Dartmouth* decisions respectively. Above all, Marshall set the Court its permanent task—the protection of the revolutionary new governmental order against the demands of regular politics. This was to be the function of the Court when it was confronted by the three Reconstruction Amendments, as well. While, at first, it treated the new Constitution as applying only to African-American exslaves, it soon extended "the equal protection of the laws" to cover the contractual and property rights of white citizens, as well. The market was its paradigm, and for that time and place it was a genuine work of integration as the new rights were absorbed into the Federalist doctrines of the first era of interpretation. For the Court understood that the people had acted to create national citizenship and an energized laissez-faire economy, free of the impediments that slavery, sectionalism, and their attendant political inhibitions had imposed on the pre-Civil War order. In that view, *Lochner* was quite appropriate in 1905, though it was no longer acceptable in 1932. With the New Deal, the Court learned to accept the regulatory state and the finally institutionalized "energetic" presidency that Hamilton and Jackson had promoted in vain. Above all, beginning with Chief Justice Stone's celebrated footnote in *Carolene Products*, the Court restored the Bill of Rights to primacy in order to protect personal and minority rights within the now enormously expanded powers of the state. The break with its immediate past was thus not really complete, since the Court saw to it that the new state was still limited in its powers. Moreover, the Warren Court was no more "prophetic" than its predecessors (pp. 140–42). Even *Brown* was only a part of the New Deal's comprehensive state activity, extended to public education with due regard to the tradition of "the equal protection of the laws." It was not a return to the Marshall Court, as some claimed, but an adaptation of its doctrines to wholly new circumstances and a measured response to the more immediate past. This story has at least two important implications. First, it reveals the Court as the

guardian of those few truly revolutionary amendments when "the People" act directly and not through their "stand-ins." Its does so ultimately by integrating these radical changes into accumulated interpretive practices. Second, Ackerman also neatly overcomes the old question so often put to liberals, "Why are personal rights more important than property rights?" If one accepts, as he does, that our rights are to be found only in the Constitution that "the People" have made and remade, then one simply recognizes three successive episodes in which rights were given new meanings and applied to new claimants. In this scenario, all rights have the same standing and source—and all undergo the same fate—as history imposes its limits on every successful revolution.

"We the People" are only the politically active citizenry in Ackerman's story. Americans are normally "private citizens" who become politically engaged only sporadically. He does not particularly regret their preference for civil society. The Constitution was made for such people, and it does not require Jacobin, virtuous, full-time citizens. An occasional Ralph Nader is quite enough (pp. 232–33). Nevertheless, Ackerman does think that the amending process should be made more like a referendum and offer a greater opportunity for presidential initiative. This would evidently make the mobilization of "the People" for revolutionary change easier and more frequent. Actually, Jefferson proposed something of the sort only to be soundly refuted by Madison in *Federalist* 49. Ackerman does not mention this, perhaps because he seems so eager to deny his own perfectly obvious Jeffersonian roots. He is none the worse for them; for though he is not as insouciant about bloodshed as Jefferson was (Who in our century could be?), he, too, advocates transforming popular action, in a modified version of Hannah Arendt's idea of a political revolution with some social issues added. Her definition, moreover, justifies Ackerman's applying the term *revolution* to his three transforming moments of constitutional history. It is an encouraging thought, as well, meant to inspire "the People" to assert their sovereignty again. The Supreme Court could presumably use a new amendment to divert it in time from its current course. In the meantime, we can look forward to the continuation of Bruce Ackerman's interpretation of our constitutional history and hope that he will expand his story to include people and events that lie beyond the confines of a hermetically sealed legal history.

Harvard University JUDITH N. SHKLAR

The Prevalence of Deceit. By F. G. Bailey. Ithaca: Cornell University Press, 1991. 143p. $24.95 cloth, $7.95 paper.

Positivism has been displaced, or so we assume. The program of making everything knowable through the supposedly impersonal norms and procedures of "science" has been radically questioned. The hope of constructing a "grand narrative," either intellectual or political, that will give us the ultimate truth and will lead us to freedom has been shattered in many ways. Reality, it seems, is a text subject to multiple interpretations and multiple uses. The implications of this for research and politics in our supposedly "postmodern" age are more than a little interesting.

There are few words more often used—and abused—in discussions of contemporary culture and politics than postmodernism. It is hard, in fact, to find an area of study where it has *not* had an impact. In general terms, the form postmodernism takes is that of the self-conscious, self-contradictory, self-undermining statement. It wants to provide a thoroughgoing "denaturalizing" critique, to "dedoxify" our cultural representations. All of our discourses are politically "uninnocent." They occur within a dynamic social context in which the existence of multiple sets of power relations are inevitable. Hence, discourse and politics, knowledge and power, seem to be part of an indissoluble couplet.

All of this is provocative. Yet while much of the postmodern project is insightful and serves as an important corrective to what even progressive scholars take for granted, there are still parts of postmodernism that should and do make one pause. Its capacity for overstatement, its own rhetorical flourishes, its use by all too many people to make often simple points that we sometimes already knew from other traditions—all of this detracts from some of its strengths.

Bailey avows parts of the deconstructionist program while offering substantive (and occasionally simply rhetorical) criticisms of it. Even when I was not always in full agreement with his reproofs, constructing responses to them was quite an enlightening enterprise.

Bailey himself is a reconstructed positivist, one who wants to inquire into the conditions in which we test "truth" in a world of contested values. Because of this, *The Prevalence of Deceit* is true to its title. Social life is organized around "basic lies" and smaller lies. Truth is a contest, it is not simply there. The language of truth and deceit has multiple uses. It provides the cement of social interaction and is malleable depending on its historical and cultural context. Truth and untruth are the very instruments of politics. Bailey's discussion of this is based on multiple examples, often taken from his own earlier anthropological fieldwork in India.

In the process, he offers comments on how the issue of truth and lying has been dealt with in previous theories, how we are to understand the process through which dominant groups gain hegemony, how negotiations over "the truth" involving differential power relations go on, the place of deceit in ethnographic research, and the status of truth claims in such research. The volume is really a wide, varying essay, more a "reflections on" than a detailed treatment of its subject. To prove his case that in political and cultural life, truth is "mostly illusory" and that the "life we share with others must include untruth" in a variety of forms (p. xxi), Bailey would have had to expand his examples and arguments considerably. While this gives some of its content a rather stipulative feeling, what he does present is nicely complemented by an engaging and often bitingly humorous style.

The volume is not without its conceptual and stylistic problems, however. There are times when, even given Bailey's elegance and wit, the reader is not at all certain where the book is going. Further, I was surprised that Bailey did not draw upon Murray Edelman's work on political language or James Scott's discussions of public "performances" that hide acts of resistance. This might have lent even more strength to some of his claims about relations between dominant and dominated groups.

Finally, Bailey at times does not always fully represent the more sophisticated arguments already made either defending or criticizing interpretive/hermeneutic an-

thropological theories and postmodern approaches. Although I, too, have strong reservations about aspects of interpretive and postmodern theories, especially their tacitly apolitical tendencies behind their rhetoric, these theories are not composed of simple arguments. They require detailed exposition of their *best* case; then one can set about doing a more powerful critique.

All this said, I do recommend *The Practice of Deceit*. Few books actually force us, by both their style and their content, to engage in a good debate. Bailey has offered one such book.

University of Wisconsin, Madison MICHAEL W. APPLE

Identity\Difference: Democratic Negotiations of Political Paradox. By William E. Connolly. Ithaca: Cornell University Press, 1991. 244p. $27.50.

In a letter to his brother Theo, Vincent Van Gogh cautioned, "We must not judge God from this world, it's just a study that didn't come off. It's only a master who can make such a blunder." The statement expresses awe at worldly existence without expressing resentment of its shortcomings. Yet both here and in his art Van Gogh conveys that for all its wonders, there is much pain in life and that we decidedly do not live in the best of all possible worlds. One might say that William Connolly's *Identity\Difference* offers a secular and political translation of these sentiments.

For inspiration, Connolly primarily looks to Friedrich Nietzsche, who did with prose what Van Gogh did with paint. The purpose of Connolly's book is to investigate the psychology of resentment, so trenchantly described by Nietzsche, that stems from our unwillingness to accept the contingent and ungrounded aspect of the human condition. Connolly also looks to Michel Foucault, who offers historical analyses that illustrate the means by which resentment finds its social and political expression in disciplinary regimes, normalizing procedures, and processes of exclusion. Following Nietzsche, Foucault has called these investigations "genealogies" because they illustrate the pedigree of the identities we take for granted. As an academic strategy, Connolly strongly advocates for them. Departing from Nietzsche, Foucault promotes an agonistic and *democratic* politics that seeks to avoid, or at least severely limit, the imposition of identity and the penalizing or pathologizing of difference. As a political program, Connolly strongly advocates for this. Such democratic practice, Connolly admits, will not eliminate but will at least "expose and redress the politics of resentment" (p. 192).

Identity\Difference is, to my mind, the best-written of Connolly's books. It is stylistically original and captivating. Formally, its major problem is its repetitiveness. The reader is frequently reintroduced to the book's central theses, which get dressed up in different attire for different events but generally fail to make new impressions. The gist of his 34-page "Letter to Augustine," for instance, already elaborated in earlier chapters, could have been nicely conveyed by Nietzsche in a few aphorisms. Notwithstanding such formal criticism, I consider *Identity\Difference* to be the best work to date that theorizes politically about the effects of existential resentment. In the spirit of Connolly's desire agonistically to "negotiate" with his readers about the political paradoxes this theorizing begets, I shall illustrate Connolly's impressive achievement by questioning a few of his tactics.

Connolly highlights the effects of resentment by focusing on the "problem of evil." He examines Saint Augustine and the "discoverers" of America as examples of those who persecute or conquer others because they are incapable of celebrating difference. Evil appears to exist, for the world is imperfect. Unwilling to ascribe the root of evil to their God or to themselves, these powerful men indignantly thrust its burden on those different from them. The Manicheans, Pelagians, and Donatists suffered for their otherness at the hands of Augustine. Similarly, the native dwellers of America suffered at the hands of Columbus and Cortez, who attempted to conquer them, and the priest Sepulveda, intent on their conversion to Christianity. Connolly extends this analysis to modern "territorial democracy," wherein national solidarity is bolstered through the creation of external threats. As the old quip goes, a nation is a group of people with nothing in common except an enemy. A democratic politics that questions national boundaries, celebrates difference, and struggles against existential resentment, Connolly suggests, would offer us an alternative to the age-old alleviation of our existential insecurities by the facile imposition of evil identities on other individuals and nations. The goal is certainly admirable. Connolly's means of promoting it, I would suggest, require rethinking on at least two counts. First, despite Connolly's globalist aspirations, his own analyses, resources, and discussion remain fundamentally Western (not to say, American) in orientation and scope. Despite his call for us to escape the territorial state, Connolly's own work seldom leaves its own shores to encounter the other. Second, Connolly challenges us to eschew the attempt to secure the purity of our own identities by the simplistic and dangerous imposition of evil identities on others. Yet his own attack on the critics of postmodernism and other assorted opponents often fails to escape this type of behavior. In Connolly's hands, "[Kenneth] Waltz becomes the Sepulveda of his time" (p. 51); "[Robert] Keohane is the Columbus of modernity discovering the postmodern" (p. 55); and we find "[Charles] Taylor and [Jean-Paul] Sartre . . . embodying modernizations, respectively, of Augustinianism and of a heresy (Pelagianism) it spawned" (p. 113). These characterizations are stimulating. But they do not exemplify the ambiguous nature of identity advocated by Connolly.

Identity\Difference "reverses the emphasis" of his previous works, Connolly claims, by focusing more on the effects of resentment than the "social injustices" that may foster it (p. 34). But, and this is my third substantive criticism, the reader of *Identity\Difference* is given *no* reason, theoretical or empirical, to believe that social injustices, however defined, foster existential resentment. We are offered only the weak claim that "general access to economic, educational, and cultural opportunities *seems* to be one of the preconditions of more widespread engagement with the contingency of identity" (pp. 211–12; emphasis mine). In his magisterial investigation of the political effects of existential resentment, Connolly neglects to examine its social causes and therefore leaves us with no basis for understanding the social preconditions required for agonal democracy.

We are introduced to Connolly's text by an epigraph of Foucault's that celebrates curiosity as "a lack of

respect for the traditional hierarchies of what is important and fundamental." It seems a fitting orientation for Connolly's postmodern project. Yet Connolly fails to notice that Heidegger (who, with Nietzsche and Foucault, forms the tripod supporting Connolly's endeavors) had a different view of curiosity. Heidegger often spoke of curiosity as an attempt to escape thinking about the fundamental ground of existence by way of a constant and eventually jaded search for novelty and the stimulus of innovation. As such, curiosity feeds the modern technological attempt to escape the limitations of our world and the burden of our being through the continuous fabrication of new things. My final criticism is that Connolly generally ignores the threat posed to sustainable democracy by technological ways of life and thought, as well as the potential charge that postmodernism often reflects just this attempt to flee the human condition and the hard choices of politics by means of literary fabrication coupled with a flippant iconoclasm and rootlessness.

Connolly is an unapologetic postmodernist. He has no pretensions that the world we inhabit and the lives we live are metaphysically grounded, ideally constructed, or even rationally coherent. For the most part life is a mystery whose tangles only further enmesh us the more we struggle to undo them. Nearly two-and-a-half millennia ago, Socrates seemed to say the same thing. He claimed to know only that he knew nothing. But then Plato, in Book III of the *Republic* and with political purpose in mind, has Socrates tell a lie; the truth of life's mystery does not allow for an orderly politics. So Socrates spins the myth of the metals, encouraging the belief that the gods have mixed gold, silver, iron, or bronze in people's veins at birth so that their endowments and destinies are predetermined. Unambiguous identities are established. Political order is achieved. Difference is tolerated only to the extent that it plays its prefigured role within the austere and restrictive structures of the city.

Identity\Difference is a courageous attempt to sketch a city in speech that rejects the need for noble lies. Like Plato, Connolly is necessarily ironic. That is, he is telling stories that make no claim to transcendent truth. Unlike Plato, Connolly makes no pretense that the stories he tells are noble ones. That is, he eschews that heroism investing Plato and Nietzsche (Foucault is explicitly invoked as an antidote to Nietzschean heroism on p. 10) that has one striving to raise one's program above all possible contenders. There is no attempt by Connolly to justify his theory as the means by which the polity at large, or even one's own soul, *must* be grounded. Thus, Connolly rejects any form of "command ethics" and opts for an "ethic of cultivation" that promotes, but does not ground, a caring for difference. He inserts his into the tumult of voices seeking our academic attention and political cooperation in the late modern world, not to "provide a single, exclusive interpretation but to place another candidate onto the field of discussion" (p. 171). Connolly's only firm conviction is of the partial (i.e., neither impartial nor totalistic) nature of the picture he paints. In short, Connolly's irony distinguishes him from the modernist, who "perpetually asserts . . . *as if* each time were at once the first and the last time it will be needed to refute the opponent" (p. 59). What separates Connolly from the ancients, and from Nietzsche, is his rejection of the need for nobility, which, as Nietzsche knew, is the oldest collective identity invoked by some that they might impose yet a different identity upon others.

University of Florida LESLIE PAUL THIELE

The Noble Savage: Jean-Jacques Rousseau, 1754–1762. By Maurice Cranston. Chicago: University of Chicago Press, 1991. 399p. $32.50.

The admiring readers of the previous volume of this biography (*Jean-Jacques Rousseau: The Early Life and Work, 1712–1754* (now available in paperback) have looked forward to this second volume; and, with confirmed enthusiasm, will be looking forward to the third. This volume begins with the *Discourse on Inequality* on the point of publication. It covers the years during which Rousseau was writing and getting into print *Julie*, the most successful novel of the eighteenth century; *Emile*, a hardly less considerable founding contribution to the philosophy of education; and *The Social Contract*, which would have sufficed by itself to make his name immortal. The volume ends with *The Social Contract* banned in France, and with Rousseau fleeing France, under indictment for provocation to recrudescent Catholicism offered in *Emile*, just banned, recalled, shredded, and burned.

He was leaving behind the best (and best-placed) friends that any original and versatile genius could have hoped for—the Marechal and Marechale de Luxembourg, whose patience, affection, magnanimity, and kindness for Rousseau resound through this book down the centuries. They were not the only kind friends that Rousseau had in these years nor the only ones who had to exercise patience to keep friendship up. Yet, touchy and often suspicious as he was, he was not at this time incapable of revising away his suspicions; and the touchiness reflected, in part, physical suffering and in part, a continual and, by Cranston's account, reasonably successful effort to maintain his exemplary independence. We find him again and again making difficulties about accepting gifts from any of his rich and noble friends and treating the Luxembourgs (grand and grandly helpful though they were) as friends, rather than patrons, all the while denouncing established society with undiminished vigor. Where affectionate relations did end, it was in one instance disappointment in love (for Sophie d'Houdetot, who fully returned his love for a time and long continued friendly and kind even after she had to break off) and in another a quarrel with Diderot after he had shabbily betrayed Rousseau to Sophie's established life companion.

Are these details of any importance to understanding Rousseau's thought? (The people just mentioned are only a few in the long procession that Cranston brings before us, keeping the whole cast of characters under such control that the information about them does not for a moment impede the forward motion of the narrative.) Cranston's synopses of Rousseau's works as they come up are excellent; but in them, the book gives us nothing new except grace in expression, polish, and economy. The story is engaging as the story of an interesting eighteenth-century life; but it might be engaging without having anything of importance to imply about the content of the hero's thought.

That is not the case here. There is something to learn about the depth of Rousseau's convictions from the aims

and character of his attempt to set an example for his age. There are points to gather, from his correspondence with friends (more often than not, pastors) in Geneva, about his complex and discriminating attitude toward that city, its government, and its culture. It is of considerable importance to students of his work to observe his growing intellectual distance from the circle of the Encyclopedists, where he first gained intellectual credit. Cranston quotes liberally from Rousseau's letters in all these connections and thus usefully supplements the body of Rousseau's writings standardly read.

Yet the most important implications of the story told here are to be found in the sympathy and respect for Rousseau that its details tend to elicit. He was painfully and incurably ill with a defective urethra and with stone, yet he not only kept at work but enjoyed many happy moments and gave happiness to many others. He continued to be mixed up about sex, but he hardly fell outside our own normal range of confusion. He was not wildly irrational, to be looked to more for preromantic effusions than for orderly thinking. On the contrary, we see him carrying on a pointed and penetrating correspondence with his publishers. We see him judiciously discarding works that did not meet the standards that he had set for greatness, namely, *Les Solitaires*, a sequel to *Emile* in which both Emile and Sophie belie their promise and succumb to the corrupt temptations of the metropolis, and a sequel to *Julie* in which Milord Edonard disports himself at some length with women in Italy. The persecutions of which he became a target were under way, but we see Rousseau for the time being making no more of them than was warranted. He had given Voltaire provocation, and one can see that their ideas were irreconcilable; but it is Voltaire, not Rousseau, who displays malice and vindictiveness. We see Rousseau, in the midst of writing his greatest works, sincere, balanced, and clear-headed; and we should bear in mind that many charming and civilized people found him lovable.

University of Texas, Austin DAVID BRAYBROOKE

Configurations of Masculinity: A Feminist Perspective on Modern Political Theory. By Christine Di Stefano. Ithaca: Cornell University Press, 1991. 256p. $29.95 cloth, $10.95 paper.

"Gentlemen: this book's for you." It is not new to suggest that, despite their explicit address to a generically human condition, the canonical texts of western political theory may implicitly carry rather more partial dedicatory inscriptions. At least since Wollstonecraft—which is to say, well before the term *feminist* entered the English lexicon—writers of the female persuasion have repeatedly taken exception to the masculine undertones of that tradition. Focusing on the work of Hobbes, Marx, and Mill, Christine Di Stefano reiterates this criticism even as she significantly shifts and complicates the position from which it is voiced. At once a feminist intervention in political theory and a political intervention in feminist theory, *Configurations of Masculinity* is a provocative contribution to both.

Di Stefano draws upon object relations and literary theory to consider "the plausibility of a demonstrable connection between modern masculine gender and the political theories of Hobbes, Marx, and Mill" (p. 11). The connection suggested, however, as well as the approach used to elaborate its features in each case, distinguish this work from what should by now be familiar criticisms of the sexism of social and political theory. At issue in *Configurations* is not particular authors' neglect or exclusion of women nor even how the internal coherence of a particular theory is structurally guaranteed by the subordination of conventionally "female" activities. Instead, Di Stefano argues, masculinity functions in the form of culturally and historically specific "cognitive proclivities" structuring perception "such that the social world is interpreted, and hence engaged, contested, and reproduced, within the horizon of those perceptual commitments" (p. 55). More specifically, as formulated by object relations theory, masculinity is a deep cognitive orientation generated by the processes of gender differentiation in the modern nuclear family—in particular, by the distinctive tensions and ambivalences experienced by boys during the pre-Oedipal stage of separation and individuation from the mother. Characterized by rigid ego boundaries and an exaggerated emphasis upon separation and autonomy, it is at once supported and threatened by its differentiation from a female maternal "other"—which Di Stefano refers to as the "(m)other."

Mothers, to be sure, receive scant notice in modern political theory, but this is not Di Stefano's focus. In the book's most theoretically innovative move, she shifts beyond the question of explicit representations of women or femininity. Embracing the hermeneutic principle that reading *generates*, rather than finds, meaning, she adapts Teresa de Lauretis' use of the *space-off* (*Technologies of Gender*, 1987)—a concept in film theory designating that which is implied by the cinematic apparatus yet not visible within a film's explicit frame of reference—to situate her own project of rereading. Locating the "(m)other" of object relations accounts in this space beyond overt representation, Di Stefano theorizes maternal origins as that which is at once necessary to, and elided by, each of her selected theorists' productions of a generic humanity.

Di Stefano thus limns the political face of modern masculinity as a more general construction of the political across otherwise disparate theoretical frames. It is not, however, offered as a seamless or unitary epochal characteristic. Separate chapters devoted to Hobbes, Marx, and Mill identify "heroic," "productive," and "disciplinary" masculinity (respectively) as distinct variations on a common frame of reference. Each configuration, Di Stefano argues, offers up substantive and thematic tensions between images of autonomous self-creation, longings for the solace of social unity, and fears of engulfment or annihilation—tensions that resonate strongly with the object relations account of masculine identity as a precarious achievement—hence Leviathan's taming of "heroic masculinity," communism's emancipation of "productive masculinity," and Mill's dual quest for individuality and social progress through civilized self-discipline. Each theory, on the account offered here, sustains the illusion of generically human self-production by effacing its own gendered specificity. Simultaneously forgetting maternal origins and appropriating maternal power, each thus falls short of its own claim to a comprehensive account of the human condition—not by its treatment of women but, more profoundly, by its obliviousness to the masculinity of its own perceptual frame. In Di Stefano's trenchant phrase,

"the 'forgotten self' of these political theories is the modern masculine self" (p. 16).

Configurations of Masculinity is a work of complex intelligence and subtle ambition, a work likely to challenge, rather than satisfy, its own multiple audiences of address. Three sorts of potential criticism bear mention. Those unconvinced that gender constitutes a distinctive politics may question the book's inattention to what are conventionally taken to be significant political stakes in the differences among Hobbes's sovereignty, Marx's class analysis, and Mill's liberalism. Similarly, feminists and fellow-travelers sympathetic to gender-based criticism of the canon may wonder whether such variations within modern masculinity may structure different prospects for reconfiguring gender relations. While Di Stefano's account does not foreclose either of these lines of questioning, neither does it address their respective concerns. Finally, readers invested in feminist debates over sexual difference may find themselves divided over the book's reliance upon the object relations account of gender differentiation. Arguably, this emphasis upon the pre-Oedipal roots of gender identity not only sustains the masculine–feminine opposition but does so at the expense of contemporary interrogations of sexuality. In effect, the identification of the (m)other as the off-screen presence of traditional theory may inscribe a different avoidance; for what occupies the "space-off," as it were, of Di Stefano's own account may be the sexual "other" elided by the constitutive heterosexuality of (re)producing mothers, an "other" now beginning to receive specific articulation in the emergent field of lesbian and gay theory.

Such criticisms, however, are beside Di Stefano's point—not in the sense of being irrelevant but, rather, in the sense of being parallel or proximate to the interpretations she advances. If she proffers the (m)other as a privileged site for feminist readings of canonical texts, she lays no claim to have discovered the truth of such texts' indebtedness to gender. Similarly, if important political differences among modern political theories appear to dissolve into diverse substrata of modern masculinity, the book is an invitation not to abandon such differentiations but to re-view them in light of their variegated imbrications with the problematic of gender. As emphatic in its insistence upon feminism's engagement with the canon as it is upon political theory's engagement with feminist perspectives, *Configurations of Masculinity* thus accomplishes the difficult task of addressing diverse constituencies with both clarity and grace. And this is no small achievement.

Johns Hopkins University K. M. McCLURE

Rights Talk: The Impoverishment of Political Discourse. By Mary Ann Glendon. New York: Free Press, 1991. 218p. $22.95.

The title and subtitle of this book nicely indicate its content. Mary Ann Glendon argues that Americans have become fluent in discussing and claiming rights but dangerously inarticulate in speaking of responsibilities, of limits on rights, and of the complex and fragile social setting in which rights have to be realized. Among the results is a tendency to formulate all social and political issues in terms of rights and to phrase rights claims in stark and uncompromising terms. Hence, political discourse tends to be reduced to confrontations about rights, with the nuances of the situations being debated left out of consideration and the interests of the competing parties formulated as absolutes. This makes it difficult either to understand political issues adequately or to negotiate their resolution.

Glendon is particularly concerned with how our preoccupation with rights leads us to neglect the social ecology on which our lives depend for their human substance. We envision the rights bearer as isolated and self-sufficient. Hence, in public deliberation, we neglect the network of families, neighborhoods, churches, and other groups below the level of government without which freedom is empty. Judges, like the rest of us, are habituated to thinking and speaking in terms of rights and are consequently inept when it comes to the threat that certain presumed rights, like that of a business corporation to shift one of its plants to another city, may pose to traditional ways of life that cannot readily be defended in terms of counterrights. Phenomena such as urbanization, bureaucracy, mobility, and mass culture tend to strip individuals to human atoms whose relations with one another are controlled wholly by the market and the state. In such circumstances, rights talk loses the social roots necessary to its moral authenticity. It comes to be less about human dignity than about "insistent, unending desires" (p. 171).

To appreciate Glendon's argument, it is essential to understand that she is not arguing anything as banal as the necessity of balancing rights with responsibilities. She is concerned with what happens to our understanding of rights when we forget about responsibilities. Ironically, if we can think only of rights, our ability to understand and defend those rights is threatened. We become preoccupied with our own rights and lose our capacity for respecting the rights of others and for understanding the social circumstances in which rights are realized. The purpose of the book, accordingly, is not that of curtailing rights talk but of refining and deepening it.

In a sense, *Rights Talk* is about law; and it contains informative discussions of court cases in this and other countries. But when the author is discussing law, she obviously has in mind also morals, politics, and language, and one of the major virtues of her book is the way it illuminates the role of law in the overall life of society. It shows, for example, how legal and moral norms interact and also how litigation shapes our language and thus affects the way we talk about, and act upon, our social environment. A particularly interesting example is her discussion of "silences" in the law. Given the heterogeneity of contemporary society, it is often hard to identify values that we all have in common. This gives law a particular moral weight; people tend to assume that an obligation not prescribed in statutes and upheld in the courts is not an obligation at all. Thus, the lack of a legal obligation, such as that of coming to the assistance of someone in peril (even the police in America being under no such obligation), encourages people to infer the lack of a moral obligation.

This is not an easy book to criticize. The author's argument is continuously interesting and is upheld with a wealth of information from this and other countries. The main themes advanced seem both true and important. And almost everything that is said is said felicitously and exactly. In the most comprehensive sense of the term, it is an intelligent book.

One might, however, take issue with Glendon's suggestion that there exists in the United States "a significant constituency . . . for candor, moderation, and complexity" and that what is in doubt is only "whether Americans in positions of leadership have the will, ability, courage, and imagination necessary to respond to and mobilize such a constituency" (p. 183). One can hardly deny that American leaders on the whole are both simplistic and timid in their public utterances. Nevertheless, they have gained office by being elected and one of the main reasons they do not speak more boldly is that they have learned better. For example, any candidate with even the modest respect for complexity necessary for saying that perhaps taxes need to be raised rather than lowered or that there may be legal ways of recognizing the evils of abortion without interfering intolerably in the private lives of women will, unless the audiences for such suggestions have been very carefully chosen, quickly be taught the perils of candor and moderation.

But this is perhaps only a quibble. What mainly needs saying is that this is a book that specialists in political theory and public law—and indeed all branches of political science—are certain to find provocative and instructive.

University of Massachusetts, Boston GLENN TINDER

Political Theory Today. Edited by David Held. Stanford: Stanford University Press, 1991. 360p. $39.50 cloth, $12.95 paper.

This well-edited volume offers 13 chapters of high quality essays ranging over numerous issues in contemporary political theory. Its most distinctive strength is a set of five essays on international politics: David Held's on democracy, the nation–state, and the global system; Charles Beitz's on sovereignty and morality in international affairs; Antonio Cassese's on violence, war, and the rule of law; Onora O'Neill's on transnational justice; and Samir Amin's on the state and development. To those acquainted with these authors' previous work, there will be little that is dramatically new; but each manages to restate key theses in a lucid and organized manner. Four essays represent sophisticated versions of conceptual analysis conducted with sensitivity to history and empirical political science: John Dunn's on political obligation; Steven Lukes's on equality and liberty; Andrew Reeve's on property; and Agnes Heller's on the concept of the political. Writing from a rational choice perspective, Iain McLean contributes an essay on forms of representation and systems of voting, while Jon Elster offers cogent if not exactly unfamiliar reflections on the limits of rational politics. Susan Moller Okin analyzes the public/private distinction from the perspective of gender, while Clauss Offe and Ulrich Preuss explore how democracy can be justified, limited, and extended.

Two other strengths of this volume should be noted. First, most of the chapters are written with admirable breadth and grasp of the relevant literature. Reeve's and O'Neill's contributions are exemplary in this regard, amounting to concise review essays that scholars will wish to consult early on. Second, taken as a whole, the chapters represent a welcome engagement of theory with the real world of politics, along the lines long recommended by Brian Barry, John Dunn and others. That the terms of this engagement are so manifestly uncertain and contested is only an argument for proceeding further.

The volume begins with a useful and balanced editor's introduction exploring the contemporary revival of political theory under the rubrics of its "object" and "nature." With regard to objects, Held argues persuasively that the traditional focus on the public institutions of the nation–state must be broadened both *internally*, to cover a wider range of economic, social, and familial activities, and *externally*, to address not only changing relations among nation–states but also new issues and institutions that cut across their boundaries and powers. With regard to the nature of contemporary political theory, Held delineates seven principal approaches: history of political thought, conceptual analysis, systematic theories of political value, concrete problem- or institution-oriented explorations, discussions concerning foundationalism and postmodernism, systematic model building, and theory directed toward empirical political inquiry.

It is of some interest to look at Held's volume through the prism of his own typology. The foundationalist/antifoundationalist, communitarian particularist/liberal universalist, and modern/postmodern debates go unrepresented. Also conspicuous by its near absence is systematic value theory. There are, for example, no sustained discussions of domestic distributive justice or of deontological liberal theory: John Rawls is far less central than in other easily imaginable volumes on contemporary theory and, to judge from this volume, utilitarianism is all but dead. There is also surprisingly little on the theory of the welfare state, about which (as David Miller argues) there has been remarkable ferment in Britain and rising concern in the United States as well. Nor is there much about the kinds of social issues—multiculturalism, difference, affirmative action, dependency, and others—that have so dominated recent public debate and (increasingly) political theorizing in the United States. Nor (with the partial though honorable exception of the Offe and Preuss chapter) is there sustained attention to the individual moral prerequisites of citizenship or, conversely, the moral consequences for individuals of participating in various kinds of institutions.

This enumeration, which could be lengthened, is not intended as a criticism of the volume under review, which represents a serious effort, stretching over 360 pages of dense type, to achieve breadth of coverage. It is to say, however, that a volume fully representing "political theory today" would have to be much longer and far more diverse. In the face of the proliferation of political theory over the past 20 years, perhaps no single volume can any longer aspire to such representativeness.

There is another reason why this may well be the case. In many respects, political theory is politics carried on by other means. One would expect, therefore, that political theory would tend to mirror the full range of contemporary public debate. *Political Theory Today* presents the portion of that range running (roughly speaking) from liberal egalitarianism to quasi Marxism. I had thought that over the past decade in the United States and Britain—indeed, in Europe from the Atlantic to the Urals—other political tendencies were finding public

voice and systematic expression. They, too, deserve to be considered part of "political theory today."

University of Maryland, College Park WILLIAM A. GALSTON

Politics/Sense/Experience: A Pragmatic Inquiry into the Promise of Democracy. By Timothy V. Kaufman-Osborn. Ithaca: Cornell University Press, 1991. 327p. $21.91.

John Dewey and American Democracy. By Robert B. Westbrook. Ithaca: Cornell University Press, 1991. 570p. $29.95.

Something of a "Dewey revival" is under way. These two books come on the heels of Richard Rorty's decade-long campaign to get American scholars to take Dewey as seriously as they have long taken Wittgenstein, Heidegger, and Nietzsche, while they coincide with Steven Rockefeller's study of Dewey's religious thought (*John Dewey: Religious Faith and Democratic Humanism*, 1991) and Robert Bellah and his colleagues' call for a revival of Deweyan "public philosophy" in *The Good Society* (1991).

Dewey occupies a slightly odd place in American intellectual life. When he died in 1952, he was an intellectual dinosaur. He had been a philosopher in the grand style, ready to write about art, religion, morals, politics, and the nature of science, and sometimes about all of them at once. He died when the analytical movement's reaction against all this was at its fiercest; and although he was honored as America's greatest philosopher, he exerted little influence on the subject for the next 30 years. The revival in his reputation even now owes less to the discovery of his merits by orthodox academic philosophy (though there is some of that) than to the sense that the analytical enterprise has lost its way.

John Dewey and American Democracy offers the reader very much what the title suggests. Westbrook takes us through Dewey's career step by step, tackling his views on democracy and education, industrial democracy, the religious sensibility of a democratic society, the role of art in a democracy, and more. The author writes with the enthusiasm of a convert. A note confesses that once he shared Christopher Lasch's view that the main function of pragmatism was to teach young Americans how to be well-adapted citizens of the corporate state; but now he sees Dewey as an inveterate radical, a critic of democratic practice in the light of democratic ideals, a social democrat, and an enthusiast for workers' control. His enthusiasm for his subject greatly enlivens this substantial book and ought to persuade any reader that there is much in Dewey still worth reading.

Politics/Sense/Experience is not so much "about" Dewey's views on democracy as a Deweyan meditation on the domination of our lives by a narrowly instrumental rationalism and the contrast between this and the politics of a truly democratic society. Because what Kaufman-Osborn values most highly in Dewey is the latter's ability to subvert or displace the conventional meanings of terms, it is not quite exact to say that *Politics/Sense/Experience* criticizes contemporary politics in the light of Deweyan ideals. Rather, the deficiencies of contemporary politics are seen as an outgrowth of a narrow conception of how rationally to manage our affairs. The defects of this conception are illuminated by an appeal to Dewey's own view of the way *experience*, with its connotations of doing and suffering, had been denigrated in favor of *reason*, understood in essentially spectatorial, detached, unengaged terms. "Political things" lose their meaning when detached from experience, and we are suffering from just that meaninglessness.

Westbrook's book is an admirable piece of work. Now that we have Dewey's *Collected Works* in 37 volumes, it is easier to appreciate the sheer hard digging that Westbrook engaged in before the Dewey Center at Southern Illinois University had finished its work. He seems to have read everything by and about Dewey and most things by and about Dewey's contemporaries. Particularly valuable is his way of moving in and out of Dewey's wider philosophical concerns, as well as his politics (though that is a slightly misleading way of putting it, since Dewey insisted that the kind of cultural criticism that philosophy had become in the twentieth century was a seamless web).

It is only by following Dewey through the whole enterprise that one can understand why he enjoyed the esteem he did. His claim that there need be no conflict between faith and science, high culture and democratic politics, art and democracy was just what his readers wished to be told. Westbrook usefully conducts us through Dewey's account of religion, with its startling conclusion that in the modern world, the task of the churches is to vanish, leaving behind a common faith in democracy and a conviction that its pursuit is overwhelmingly valuable. Again, it is not quite clear either how Dewey's relations with the eccentric Albert Barnes resulted in *Art and Experience* or how Dewey's belief that in a democracy all experience must have an aesthetic quality relates to Barnes's hatred of art experts and his enthusiasm for teaching art history to ordinary folk; but it is clear that Dewey thought a democracy could be more, rather than less, appreciative of high art.

Admirable as *John Dewey and American Democracy* is, its governing theme is not very persuasive. Westbrook writes of Dewey as a participatory democrat and an enemy of corporate liberalism, quite as if Dewey was the patron saint of Students for a Democratic Society. This must be multiply misguided, especially in its suggestion that Dewey was hostile to liberalism. Certainly, he thought capitalism doomed during the 1930s; certainly, he thought the rise of the large capitalist corporation had made the economy ungovernable. Certainly, he wanted some form of industrial democracy, vague though he was about just what it was. But he never renounced his own liberalism. Nor was his conception of the participatory society on all fours with the blend of Marx and Rousseau that we were fond of 20 years ago; quite what it was is hard to tell, but it certainly was not to be found in the Port Huron Manifesto.

Anyone with a certain amount of philosophical training will also wonder why Westbrook is so uncritical of his subject. Dewey's conviction that democracy and education are mutually reinforcing, for instance, is on the face of it a sleight of hand, achieved by emphasizing the "organic" quality of understanding and the "organic" quality of democratic life. Westbrook never stops to wonder whether Dewey's reconciliations rest on anything firmer than definitional fiat. Again, Westbrook uncritically accepts Dewey's attempts to preserve the "religious" attitude while turning his back on "religion"

but is much too quick to dismiss Reinhold Neibuhr's complaint that in throwing out institutional Christianity and the doctrine of original sin, Dewey ignored the radical imperfection of human nature and our need for constant moral discipline.

Dewey used to get particularly irritable whenever Bertrand Russell criticised his residual Hegelianism, and Westbrook is still irritable on Dewey's behalf. But Russell was quite right to ask why Dewey was so confident that "experience" revealed a world permeated by meaning and by the values to which Dewey subscribed. Dewey had been a Congregationalist, and then an Idealist, before adopting his own kind of naturalism. It was proper to ask what in naturalism substituted for God and the absolute, and *nothing* was hardly an answer. Still, Westbrook writes with such lucidity that readers will have no trouble seeing when Dewey is skating on thin ice.

Kaufman-Osborn is more elusive than Westbrook. Readers who share his horror of Weber's "iron cage" and believe that we are subject to the tyranny of instrumental rationality will find him persuasive; skeptics who think we are more alarmingly subject to the whims and follies of our rulers and in need of a lot more instrumental rationality will not. Those who share his view (held by the young Marx and many other distinguished thinkers) that "politics" has been stolen from the people will find him persuasive; skeptics who have always wanted to know what we would do with politics when we got it back and have found no answer in Marx, Arendt, Sheldon Wolin, or Jürgen Habermas may complain that they find no answer here. I find my heart with the sympathizers and my head with the skeptics. It was at least somewhat unwise of Kaufman-Osborn to couch his meditation on these difficult issues in a Deweyan voice. It adds needless complexity to what is already vertiginously difficult, and Kaufman-Osborn's own view of our situation is quite interesting enough for him to risk giving it to the reader straight.

Princeton University ALAN RYAN

Legal Theory, Political Theory, and Deconstruction: Against Rhadamanthus. By Matthew H. Kramer. Bloomington: Indiana University Press, 1991. 352p. $35.00.

Kramer's book proceeds on two levels. The larger and more successful part of the book is devoted to textual analysis of some of the key writings of G. A. Cohen, H. L. A. Hart, David Hume, and members of the Critical Legal Studies movement. A smaller and less fully realized portion of the book is concerned with negotiating the transition between theory and practice—how its textual strategies might enable us to achieve a bolder, and less frustrating, radical politics.

In his introduction, Kramer defines as the book's principal aim "to make clear the relevance and power of deconstruction for analytic philosophers" (p. 37). In this he succeeds superbly. One of the great virtues of the book is its systematization of the multifarious writings of Jacques Derrida and their distillation into a critical canon that is imaginatively and illuminatingly extended to some standard texts in legal and political theory. Central to this canon is a relentless working out of the implications of dilemmas surrounding self-referentialism: "If Epimenides avows that all Cretans never make truthful statements, and if he is himself a Cretan (and if all Cretans thitherto have indeed been chronic liars), then his pronouncement will be true only when false and false only when true" (p. 20). This perspective immediately exposes the vulnerability of extreme skepticism and relativism in philosophy. If extreme skepticism is true, then it is false. The same conundrum is applicable to extreme relativism as well.

The issue of self-referentialism also helps to structure a number of other antinomies relating both to our sense of the world and our use of language (p. 34). One of these has to do with how events can be individuated only in relation to a particular structure; and yet the structure as a whole can only be individuated in relation to the events that compose it. Analogously, the framing of events or texts through interpretive statements both constitutes an attempt at closure and attests to the impossibility of ever achieving closure. Every interpretive statement needs, in turn, to be interpreted and thus everlastingly forestalls what it presumes. Also, "because signifiers are always subject to recontextualization," the prospect of an assymetry—even a clash—between the "codes of intentionality" and the "codes of construability" always remains open and undermines the certitude of assigning consistent truth values even to tautologies (p. 29). Derrida extends the instabilities disclosed by paradoxes of self-referentialism into the citadel of language itself. With his famous notion of *différance*, Derrida calls attention to how the play of signifiers is grouped to yield significant meaning only in relation to receding contexts of what is other than itself, so that irresolvable problems of regress and circularity that were diagnosed in relation to the event/structure and text/interpretation dichotomies resurface with regard to the dualism of signifier/signified. There is an irremediable opacity in our very use of language: "All positions—be they metaphysical or political—will perforce dismantle themselves as the condition of their being elaborated" (p. 148). Kramer has a veritable field day transposing Derridean deconstructive moves to texts in legal and political theory. Thus, he shows how the distinction between *materiality* and *sociality*, pivotal in Cohen's interpretation of Marx, is undermined by the fact that *materiality* is "always already social" (p. 87). What is classified as the material domain is mediated through a set of linguistic and cultural categories that is socially ordained. In his analysis of Hart, Kramer shows how an infinite regress derails Hart's search for normativity. Hart's vaunted "rule of recognition," supposed to be internal to every legal system, can confer legitimacy on the rules that it pinpoints but cannot endow itself with binding force without erupting into an infinite regress (p. 143). Hume's philosophy unhinges itself in ways that resonate directly with Derrida's concept of *différance*, because the sensory particulars that are the building blocks of Hume's empiricism get their individuation endlessly deferred by the unceasing play of difference between what is and what is not themselves (p. 203).

Kramer fares less successfully with the second, more subordinate, part of his project, namely to evolve a more coherent radical politics from what is currently available, on the basis of Derridean deconstructive principles. He is very sensitive and sophisticated about the difficulties of effecting transitions between metaphysics and politics (pp. 246–47). Nevertheless, in his long, concluding chapter, he engages in an uphill battle to achieve a

fruitful political "clearing" in deconstructionist theorizing. Here, I feel, Kramer is less successful than in the rest of the book and is joined by the company of the vast majority of postmodernist theorists who are unable to coherently and fully articulate the connections between their metaphysical positions and their politics. Kramer is writing against Rhadamanthus—against objectivity. The deconstructionist approach he pursues, with its continual disclosure of *aporias* and paradoxes in both sides of traditional dichotomies such as skepticism/knowledge and inner/outer, ends up resurrecting a version of objectivity—a structure of dynamic "thereness" that remains unimpugned by the persisting recuperative turnings of deconstruction. The structure of the real is diagnosed as a field of dialectically countervailing forces, but it is still the "structure of the real." Deconstructionism discounts all discountings (including those of a Freudian and pragmatist sort), so that the remainder of its argument is what was there before—the brutely given, the sheerly and everlastingly unfolding other, the real. For all its sophistication, therefore, deconstructionism appears to reconcile us to what is. In the end, it is a philosophy of acquiescence—of dealienated resignation before the world (no matter how subjectively/objectively defined). Perhaps Kramer needs to take a cue from a critical comment he himself makes at the beginning of the book in talking about liberalism: "Those who hope for assurance that a tyranny cannot ever arise will have to plummet into an infinite regress, wherein each ground for encouragement has reproduced all the insecurities that evoked it. Each layer of supervision will provide assurance about everything but itself. Literal theories of politics hence turn out to be structured by incompleteness, since they enact the impossibility of achieving full and rigorous closure" (p. 22). Perhaps, then, the refusal of theory—unmoored from requirements of justification—becomes the most stalwart and clear-headed "theoretical" stance to take with regard to political matters.

Temple University ARYEH BOTWINICK

Equality and Partiality. By Thomas Nagel. New York: Oxford University Press, 1991. 186p. $17.95.

Very early in this difficult and illuminating book, Thomas Nagel remarks, "We really do not know how to live together." By this he does not mean only, or even primarily, that we live in a world of fractious disagreement. The deeper point is that we have not yet discovered social and political forms capable of ensuring the satisfaction of the claims we are justified in pressing upon each other without simultaneously imposing burdens on ourselves that we could reasonably resist. Nagel takes the reconciliation of this conflict between the perspectives of the collectivity and of the individual (the "equality" and "partiality" of the title) to be the main problem confronting political theory. It is also a main problem for political practice; and the very low probability of its being adequately resolved in practice anytime soon makes the theoretical enterprise a gloomy affair indeed.

The contrast of perspectives is a reflection in ethical thought of an inherent duality in human nature—the capacity to regard the world, in turns, from a particular position within it and from a position detached from it. Looking at things from the particular point of view of a determinate self, the success of one's own life will appear to be of very great importance. Looking at things from the detached point of view of one self among many, it will seem that the success of each life matters equally. Each point of view can give rise to reasons for action, and these reasons may conflict: impartiality may require more than people can give without risking the success of their own lives. When such a conflict exists, there may be no comfortable resolution, because it can be reasonable to be motivated by the requirements arising from both perspectives.

This complexity in practical reason will be familiar to readers of Nagel's earlier works, in which it is treated as a central problem of ethical theory. Here, it emerges as a problem for political theory, as well. In fact, it is part of several problems, of which I shall mention three that preoccupy him in most of this book.

First, and perhaps most fundamentally, it is a problem for the theory of legitimacy. The task of political theory is to discover the nature of legitimate institutions. Nagel's conception of legitimacy reflects the Kantian notion that morally acceptable institutions should be unanimously acceptable to those who live within them. To use the formulation Nagel borrows from T. M. Scanlon, institutions should conform to principles which no reasonable person could reject. The chief interest of Nagel's discussion of this difficult subject is his reinterpretation of the Kantian test in light of the two standpoints. His position is, perhaps, more controversial than he allows; but even those who disagree with this interpretation of legitimacy will find it a fruitful development in contractualist thought.

Second, the contrast of standpoints sets a problem for our conception of the role of political institutions in individual life. In recent years, political theorists influenced by John Rawls's *A Theory of Justice* have concentrated on matters of distributive justice in the broadest sense, that is, on how social institutions distribute the benefits and burdens of social cooperation. Although Nagel pays considerable attention to such questions here, his main contribution is to shift the focus to the interplay of legitimate institutions and the moral lives of individuals. Even in a world in which the requirements of distributive justice had been fully realized, the two standpoints could come into conflict. Institutions should help reconcile this conflict by satisfying the requirements of impartiality through collective action, so that individuals are left free to live their own lives in relative freedom from the direct demands of others. By doing their share in a moral division of labor, legitimate institutions become essential conditions of each individual's moral life.

The third problem involves moral motivation. Nagel believes that most of the inequalities of wealth and status characteristic of modern societies (to say nothing of those characteristic of the world at large) are unacceptable when these societies are regarded from an impartial point of view. Radical changes in institutions would be necessary to reduce social inequalities to acceptable proportions. As if the motivational obstacles confronting the transition were not enough, Nagel argues that maintaining egalitarian institutions once they were established would require large and perhaps inconceivable changes in human motivation, as much in the political sphere as in the economic. Largely for this reason, he claims that an ideally egalitarian society is

difficult to imagine and, in any case, practically out of reach. The best we can hope for is something like progressive social democracy. Nagel regards this alternative as distinctly second best; but even so, achieving and sustaining it will be a daunting challenge to the motivational capacities of people in most industrial societies.

There is much in this short and unsettling book to stimulate serious thought about a wide range of issues in contemporary political theory. Nagel's style is deceptively simple, masking an unusual depth and complexity of view. This is not a book for philosophical amateurs. For those who have read widely in the recent literature of political philosophy, it is a rare treat.

Bowdoin College CHARLES R. BEITZ

Democracy and Moral Development. By David L. Norton. Berkeley: University of California Press, 1991. 198p. $29.95.

Since its inception, liberal political theory has been criticized for positing moral ideals that have not been fully realized in practice. In recent years this line of argument, most closely associated with demands for social reform, has been overshadowed by criticism of the ideals themselves. So-called communitarians have decried liberal theory's removal of the individual from a meaningful social context and consequent postulation of an overly abstract conception of the right, regarded as prior to the good. Other critics have appealed to the virtues of the ancient Greeks and their belief in an objective human good, to the realization of which political life should be devoted.

David L. Norton has feet planted firmly in both of these camps. He describes liberal theory as an "ethics of rules" and criticizes it for moral minimalism in two senses. It regards only certain subjects as moral, and prescribes rules in regard to these that hold generally and so cannot require special or developed moral characteristics of their upholders (p. 21). Norton's eudaimonistic moral view, which he calls an "ethics of character," is based on the Aristotelian view that the good life consists of the full development of one's moral and intellectual faculties (intro.). Like J. S. Mill, Norton believes that individual development depends on making one's own choices, discovering one's true nature for oneself. In opposition to liberal individualism, which opposes the individual's self-interest to the interests of other people, Norton argues for an expanded conception of self-interest, which encompasses the interests of others.

Throughout his book, Norton draws from an impressive array of sources, including Plato's Socrates, Mill, Dewey, Emerson, Tocqueville, Maslow, and Oakeshott. His discussion is most valuable in providing a coherent framework for a range of important issues, both theoretical and practical, and in proposing concrete steps for implementing his ideas. Like Mill and John Dewey, Norton believes that active self-government should play an important role in self-development and criticizes "equilibrium democracy" upheld by many contemporary theorists (chap. 2). In the spirit of Dewey, he recommends restructuring education (e.g., by alternating academic subjects and practical experience) and supports youth public service (chap. 3). As one can imagine, Norton advocates meaningful—as opposed to alienated—work, through which, once again, one's potential could unfold (chap. 4). He takes a broadly communitarian view of the individual's relationship to society (chap. 6) and argues that government, along the lines of Plato's *Republic*, should concentrate on helping individuals to realize their potential (chap. 7). Norton's most sustained philosophical discussion concerns a view of rights based on responsibilities, rather than welfare (chap. 5). Rights should be founded upon what one requires in order to develop properly. Viewed from this perpective, rights, too, blunt the adversarial thrust of traditional liberal theory.

The main problem with all this is that Norton says little about the ideals we are to realize. He does declare that all individuals possess all the virtues *in potentia* (p. 83). Under the proper conditions, with their potential made actual, all would lead meaningful lives, in a society devoid of conflict. Virtually nothing is said about competitive market economies or the economic consequences of proposed educational and vocational reforms. Norton realizes the need to abandon the "closed teleology" of Plato and Aristotle for "a liberal open-ended teleology whose ends require to be discovered through exploration" (pp. 6, 163). But the only definite value he upholds is moral autonomy. In regard to this, one must ask, Autonomy to what end? Without a clear account of the good, Norton's depiction of worlds of meaningful education, meaningful work, and meaningful community are cloying in tone and largely devoid of substance. Indicative of the remorseless hopefulness that suffuses his book is Norton's interpretation of Plato's *Republic* as upholding a political system in which the rulers' power depends upon the consent of the governed (p. 160).

Norton is correct that contemporary liberal theory pays more attention to the public than to the private, to rules of justice than to desirable traits of character, to the right than to the good. But the reason for this is the need to provide a stable framework within which people of diverse moral, religious, and political views can live together in harmony. Norton's open-ended eudaimonism would do little to obviate the need for this task. In emphasizing responsibilities over rights, the place of others' happiness in our own self-interest, and the importance of developing our latent powers, Norton has something important to say. But without a clear account of the good (or goods) that all citizens should pursue, his views provide a supplement, rather than an alternative, to existing liberal theory.

University of Virginia GEORGE KLOSKO

The State: Its Nature, Development, and Prospects. By Gianfranco Poggi. Stanford: Stanford University Press, 1991. 214p. $29.50 cloth, $9.95 paper.

Gianfranco Poggi is an experienced, learned, and sophisticated political sociologist who has authored several valuable works on political theory. *The Development of the Modern State* (1978) was an impressive short history of the rise of the modern European state. Not only was it interesting for scholars because it summarized so much European, particularly German scholarship, but it was also extremely valuable as a classroom text. Undergraduates found it difficult; but it introduced them to

such concepts as feudalism, the *Ständestaat*, absolutism, and liberalism in a neatly comprehensible way.

This new work is meant to add to the earlier, successful volume. It is a more difficult, and also a more rambling book because it grapples with Poggi's rather mixed assessment of the direction of modern politics in Europe and North America. Perhaps it tries to cover too much by providing introductory material, a review of several European lines of theoretical thinking about the state, and a good bit of personal opinion.

Poggi begins by reviewing the history of the rise of the European state. He places insufficient stress on the survival of the *Ständestaat*—particularly in England, but in the Netherlands, as well, as the precursor of the liberal state. Continental absolutism, whether of the French, Prussian, Habsburg, or Russian type all proved to be dead ends, though it took two centuries (1789–1989), three bloody world wars (the Napoleonic Wars and World Wars I and II), and the intervention of England's heir (the United States) to save the liberal state. It is true, as he points out, that the modern liberal state is a vastly more powerful entity, and controls a much larger portion of our lives, than it did in the eighteenth century. But it is an error to see the modern liberal state as derived from European absolutism. Not only Montesquieu but much of the aristocratic opposition to absolutism (and it was aristocratic, not bourgeois, opposition, that began the French Revolution) looked to England precisely because it had preserved an important balancing role for its elite estates. How the United States managed to preserve some of this through its complex constitutional arrangements, at least until quite recently, is the part of the story that most European theorists tend to miss, though Americans (e.g., Samuel Huntington) know it well. The more modern, efficient, centralized bureaucratic systems of continental Europe, combined with the rise of mass participation in politics and of an egalitarian ethos produced totalitarianism, not the modern liberal state. Only the Anglo–American victory in 1945 and the subsequent defensive wall erected against communism in Europe kept continental Europe liberal. And all totalitarian systems have proved to be too inflexible to adapt well to the demands of rapidly changing technologies.

Poggi then goes on to describe the development of the modern state and to stress the disjunction between increasing democracy and the growth of specialized bureaucracies. Again, his summary and review of important works on this topic are elegant and useful. But in the end, he cannot explain why, in the past decade, there has been a revolt throughout Europe and North America against this old trend of bureaucratization and reliance on specialized civil servants to run the state. This has gone much further in the United States than in Europe, probably because the United States never went through an absolutist royal stage. Here, for reasons that are not very clear, the civil service has lost its legitimacy. It remains large and powerful but also underfunded and despised by most of the population. This has produced a kind of vicious circle in which the quality of high civil servants (those whom some Europeans like to call "mandarins") has declined, the efficiency of key portions of the bureaucracy has degenerated, and contempt for it has increased.

It is sobering to read Poggi's chapter on Gorbachev and the Soviet Union, in which he explicates the failed Soviet system as an alternative to the liberal state. Since he wrote this section, the centralized Soviet bureaucracy has virtually disintegrated. Ironically, the extreme expression of European absolutism—the Stalinist state—has failed at the same time that its arch enemy, the United States, the great bastion of liberalism, faces the incipient collapse of large parts of the central bureaucracy. That shows the advantage of preserving old-fashioned local and private centers of economic status and political power. In a liberal society, even one as degraded as the United States, the failure of the central bureaucracy leaves much of the rest of the society functioning tolerably well. In an absolutist state, the collapse of the bureaucracy leaves only chaos. In his next book, Poggi may take on these large questions, which he introduces, but does not fully elaborate in this volume. There are few comparative political sociologists more able to accomplish this task than he.

University of Washington, Seattle DANIEL CHIROT

Value-Free Science? Purity and Power in Modern Knowledge. By Robert N. Proctor. Cambridge: Harvard University Press, 1991. 331p. $34.95.

The question whether science can or should be "value-free" is one of those perennial topics on which it is difficult to say anything that is both new and sensible. A primary cause of the failure to be sensible is the tendency in such discussions to reify the critical terms, especially *science* and *value*. We blithely assume that there are two distinct entities, science and value, and ask how they are related to each other. But the real issue is always how science and value and other related concepts (e.g., fact, objectivity) are understood or constructed. What is at stake in these discussions, as Proctor clearly shows, varies with the intellectual, social, and political context in which they occur. In the ancient world, in early modern thought, in the great *Werturteilsstreit* of German social theory at the turn of the century, and in post-1945 American social science, different meanings were attached to the key terms, and different issues were at stake. Thus, members of the Royal Society in the seventeenth century may have been concerned to defend their investigations against religious censorship, while postwar American social scientists reacted against Nazi efforts to claim scientific warrant for their monstrous projects. Because standard accounts of the relationship between values and science are conducted on too philosophical and too abstract a plane, Proctor argues, they fail to realize the complexity and the variability of these issues, and an historical approach is required. By explicating the arguments of the proponents of value-freedom, Proctor clearly demonstrates how this set of issues has changed, reflecting the intellectual, moral, and institutional pressures and concerns of different times.

Many of the individual chapters addressing particular thinkers and their contexts are illuminating and reflect a sure grasp of the relevant scholarly literature. But, perhaps because it covers such a wide range of cases, the work is sometimes an unsure guide to the territory. In some cases, for example, the argument becomes tendentious, to the point of deliberately misreading his sources. Citing Tönnies, he argues that neutrality "was proposed

in opposition to socialism, the expansion of women's rights, and the mixing of races"; yet he quotes Tönnies as saying, "As sociologists, we are neither for nor against *socialism*, neither for nor against the *expansion of women's rights*, neither for nor against the *mixing of races*" and adds that sociology's task is not to further or obstruct particular ideas or movements (p. 92). Similarly, on the basis of his examination of the views of prominent defenders of the ideal of value freedom (Sombart, Simmel, and Weber), Proctor argues that the commitment to value-freedom functioned to exclude women from science. But it is far from clear that their beliefs about value freedom (as opposed to beliefs about women's alleged capacities) had anything whatever to do with their attitudes toward women is engaging in scholarship. Indeed, they reached significantly different conclusions in spite of the similarities in their commitments to value freedom.

In other cases, important errors creep into the argument. In the chapter devoted to Weber, for example, he fails to recognize that Weber's view of value-freedom is qualified by his recognition that "ideal-types" are constructed with an eye to their "cultural significance": different investigators, working from different points of view, will select different traits in constructing "an ideal-typical view of a particular culture" (*The Methodology of the Social Sciences*, 1949, p. 91). Proctor also appears to confuse Weber's ideal-types with the "idealizations" of the physical sciences (p. 146). In a similar vein, we are also told that "the positivists" believed that "methodology must be closely linked with subject matter" (p. 146), though we later learn (this time closer to the mark) that positivism is committed to "an assumption of the unity of science" (p. 159).

More seriously, however, the work loses its way in forgetting its primary lesson, that the call for value-freedom in science is not one thing but means different things and that it is advocated for different reasons in different contexts. Although granting that the ideal of neutrality was once "progressive," it no longer is so, Proctor asserts, because "science has become a power in itself" (p. 270). Warming to his theme, he argues that "all science is politics, or ethics," because "science *should* respond to practical problems of human need and suffering" (p. 270). But if this work teaches anything, it is that *science* is not a unified agent to which power and responsibility can be ascribed. Proctor dismisses the ideal of neutrality on the grounds that "the priorities of science are shaped by larger social priorities" (p. 267). But few advocates of value-freedom would disagree with that observation. The problem is to organize the practice of, and to set priorities for, research without compromising the objectivity—or, to use an even more old-fashioned term, the truth—of the results; it is to guard against the kinds of abuses epitomized by Lysenkoism and Aryan physics. Theory, as Proctor argues, can "shape the world" (p. 230), in part because research can lead us to alter what we believe and how we act in unexpected ways. And precisely because of that, the course and outcome of research can never be fully determined or controlled. Whatever else they advocated, the proponents of value-free science understood that point and, with it, the limits of Proctor's claim that "science is politics."

Wesleyan University J. Donald Moon

Post-Modernism and the Social Sciences: Insights, Inroads, and Intrusions. By Pauline Marie Rosenau. Princeton: Princeton University Press, 1992. 229p. $35.00 cloth, $12.95 paper.

Political Theory and Postmodernism. By Stephen K. White. New York: Cambridge University Press, 1991. 180p. $39.50 cloth, $12.95 paper.

At a decade or so past midcentury, with the dissemination of the writings of John Austin and Ludwig Wittgenstein on language, the once secure epistemological status of statements was called into question. Apart from the impact on philosophical discourse, the resulting blurring of the line between descriptions and other linguistic acts and the evocation of a vast domain of normative conventions intervening between statements and their referents, acts, and effects put tremendous pressure on many of the conceptual props of political theory and the social sciences in general.

There were many reactions, but here I refer to two very prominent and polar ones. One kind of response took the challenges seriously and attempted to rethink such domains as the history of political theory, the status of normative versus empirical judgments, the epistemology of the social sciences, and so on. The second was a defensive posture that regarded the texts with shallowness and hostility and reproached Austin and Wittgenstein's conceits as dangerous attacks on morality and "the real." These attacks tended to be obtuse to the problem in that their reproaches made use of the very distinctions that Austin and Wittgenstein's analyses had called into question.

Now, as we approach the end of the century, a variety of philosophical, historical, literary, political, and critical/social analyses called postmodernism have offered a new set of challenges. It is important to note that the challenge has gone beyond the Austinian–Wittgenstinian destabilizing of the warrants for treating statements and has destabilized the grounds for evaluating "discourses"—the collections of narrative, rhetorical, grammatical orientations (sets of statements in various enunciative modes) through which fields of practice are organized and the subjects and objects of discourse are situated. The implications for political theory, political science, and the social sciences, among other domains of inquiry, have been far-reaching.

Stephen K. White's *Political Thought and Postmodernism* represents the first kind of reaction. It is an intelligent book; and although it is an assembly of chapters whose prototypes appeared separately, it is a coherent piece of analysis. It is held together by two of White's compelling interests: (1) his Habermasian inclination to save a model of intersubjectivity within which community can be negotiated rationally (he chides postmodernists for "cavalierly dismissing Habermas") and (2) his Heideggerian hope for a frame that would unite the critical interpretive interest in disclosure with a political–communitarian interest in norms for responsible action.

Convinced that Heidegger has left a legacy for postmodernism, White argues that Heidegger's concept of *Gelassenheit* overcomes a sharp dichotomy between passivity and activity and offers hope for moving from the critical process of disclosure to the normative legitimation for action. These strong positions give White's book both its strengths and weaknesses. On the positive side, White's determination to find in Heidegger a chance to bridge criticism and action and thereby vindicate a

Habermasian version of enlightenment reason is consistently argued against various postmodernist challenges that he poses to his hopes and views. In chapters on Heidegger and various postmodernist thinkers and in more thematic treatments of "difference feminism" and the problem of justice, White's treatments of postmodernist positions versus his own are nuanced, fair-minded, and scholarly.

The book's weaknesses, however, stem from its strengths. Because it is primarily a work of exogenous criticism, postmodernist thought is neither well elaborated nor critically assessed on its own ground. Postmodernist thinkers are not so much analyzed as judged on the basis of White's interests. As he puts it baldly, he is after an "acceptable approach to ethics and politics." What emerges as acceptable comes out of an attempt to maintain the ontological and moral conceptions that come from his hermeneutic–critical orientation, edified by postmodernist criticism. As a result, his position is never confronted by a strong version of postmodernism, for example, a postmodernist questioning of whether morality and "truth" (even an intersubjectively negotiated version of it) are compatible.

In his attempt to incorporate some of postmodernism's critical postures into an ecumenical synthesis with his Habermasian and Heideggerian interests, White involves himself in an interesting, engaging, and edifying struggle, even though he fails to allow postmodernist thought the space to articulate fully its cautions about such neoenlightenment projects and the different problematics it would substitute.

Many postmodernist positions do not dismiss the idea of a normative ground for action. But they try to exercise "patience" (as Foucault put it) and consistently disclose what is lost by fixing interpretations that permit the legitimation of action. This is not a rejection of legitimation; it is an enactment, rather, of the recognition that legitimation involves a never-ending negotiation that requires the dissolution of certainties. It is thus negotiation all the way down. It is not a joint search for communality or grounds, nor can it count on a transcendent narrative—a temporally unfolding ontology (destiny) that will attract agreement.

However, whatever position one might take on the success of White's attempt at theoretical reconciliation, it does raise the level of debate among Habermasian critical theory, Heideggerian hermeneutics, and postmodernist inquiry. Both those familiar and those unfamiliar with postmodernist thought will find White's book challenging. It is an important contribution to contemporary political theory.

Pauline Marie Rosenau's treatment in *Post-modernism and the Social Sciences* belongs to the more obtuse genre of reaction I have noted. Its scholarship is thin (and often secondary) and its treatment of positions often uninformed and confused. Postmodernist thought, which has a variety of different and challenging implications for the social sciences, is unfortunately often known and characterized on the basis of rumor. One hears that it is a celebration of irrationality, that it is a wholly skeptical and depoliticizing attack on the very possibility of rational political and moral action, that it rejects any conception of history or truth, and so on. Rosenau's book, despite occasional suggestions that there are more reasonable (what she refers to as "affirmative") postmodernists, is a sustained attempt to perpetuate these rumors. Although the advertising blurb on the back of the book portrays the author as neither hostile to, nor apologetic for, postmodernism, the text tends to represent postmodernism as a series of perverse attitudes with very little philosophical depth.

On its face, the chapter-by-chapter list of topics is promising and comprehensive, treating issues of the subject, authorship, history, truth, and a broad range of other epistemological issues central to the social sciences. But, alas, almost every issue is seriously misrepresented. A frequent topic sentence is "Post-modernists argue, . . ." typically followed by a confusing collage of positions that represent the views of no particular thinker and often tend to conflate widely conflicting positions. We are told, for example, that a postmodernist reader has complete license "to attribute meaning to the text without consequence or responsibility" (p. 25). Apart from the fact that most postmodern thinkers are text-oriented, not reader-oriented, it is hard to imagine whose position this might be. We learn also that postmodernism is anti-subject. It is the case, rather, that postmodernist thinkers tend to treat subjectivity as an unstable, interpretive imposition, rather than a fact about consciousness. Accordingly, they often undertake inquiries into the histories of different modes of imposed subjectivity along with analyses of the institutional and other correlates of those modes. Rosenau offers no information or analysis on these undertakings.

Confusions abound. One of the most eggregiously uninformed treatments is that of the issue of "presence" as it is encountered in the writings of Jacques Derrida. Even a slight familiarity with Derrida's texts would make it evident that for Derrida "presence," the idea that the meaning of something is present to the speaker or writer is to be juxtaposed to "representation"; for his argument is that the world of things and relationships is always mediated through representations. The condition of possibility for apprehension is a script—a system of meaning—that precedes and preorganizes any individual's consciousness.

Whatever one might think about the validity of this argument, which is sustained throughout Derrida's early writings, the idea of presence is not to be simply juxtaposed to absence—as Rosenau does, for example, in this vexed passage: "As we have seen, post-modernists argue presence is never absolute: the absent is always present to some degree, and the present is always absent" (p. 79). Adding to the confusion, Rosenau characterizes postmodernists as generally hostile to representation (a position that could be better applied to Marx's attack on the commodity form).

In perhaps her most polemical section, Rosenau states that "almost all postmodernists reject truth even as a goal or ideal because it is the very epitome of modernity." (Here she cites Michel Foucault and Richard Ashley). Apart from the fact that answering the question about what modernity is was central to Foucault's work, this is a puzzling thing to say about a thinker whose historical analyses were organized around discerning the "will to truth." Certainly, Foucault impugned the traditional empiricist view of truth as correspondence (as has, indeed, even empiricist philosophy of science for several decades now) and criticized the hermeneutic view of truth as a recovery of depth, which has supported expressivist theories of meaning. Intensely concerned with truth, Foucault posed the question of how "truths" understood as unchallenged interpretations

have emerged and the correlative question about what forms of power they have enabled.

Here, again, Rosenau makes no philosophical defense of a more traditional view. Implying that "truth" (in the form of an apparently unproblematic presence) is both a fact and a moral necessity, she treats postmodernist alternatives as morally offensive. She does attempt to moderate, however, her characterization by again summoning her "affirmative postmodernists," who are less immoral, because they accept some (albeit relative) truths. Here, again, she has missed what is at issue for the thinkers whose work she trivializes. Although it is not the most familiar form of investigation in the social sciences, one of the most significant postmodernist contributions has been its treatment of the historical process by which some interpretations succeed and others fail, contextualized within a concern for both the political implications of the successes and failures and the losses associated with the imposition of historical narratives that gloss over those historical struggles.

In short, readers will not learn very much about postmodernist thought or its implications for the social sciences in Rosenau's treatment; for she has reduced a complex set of philosophical positions, interpretive orientations, and practices of inquiry to simplistic and misleading slogans.

University of Hawaii MICHAEL J. SHAPIRO

What's Left? The Ecole Normale Supérieure and the Right. By Diane Rubenstein. Madison: University of Wisconsin Press, 1990. 215p. $40.00 cloth, $19.95 paper.

This book raises a question that has perplexed political theorists and philosophers interested in the phenomenon of fascism in the twentieth century: How do we explain the complicity of intellectuals with the heinousness of Nazism? Diane Rubenstein's deconstructive reading of the Ecole Normale Supérieure (ENS) cleverly reveals the danger of this question, namely, that posing the question might reveal the rather remarkable fact that a single intellectual discourse and institutional logic may be capable of housing *both* resistors and collaborators. The urgency with which the choice of fascism must be explained, expressed in works such as Sartre's *Anti-Semite and Jew* (1964), Merleau-Ponty's *Sense and Nonsense* (1964), and Simone de Beauvoir's *Force of Circumstance* (1965), is felt still in Robert Soucy's confession that what motivated his writing *Fascist Intellectual: Drieu La Rochelle* (1979) was the nagging question, "How could such a sensitive, intelligent, cultivated intellectual. . . become a fascist?" (quoted p. 76). Rubenstein's strategy in *What's Left* is to turn the question back on the poser, implicitly rewriting it to read, How could someone like me become a fascist?" Her work engages not with the explanatory power of different theories of fascism, but with the power of intellectual discourse to remain blind to what motivates its own efforts to distance itself from mistaken choices by defining them as aberrations or as thought gone wrong: "The 'riddle' of the ENS is *not* how a few rightist *normaliens* can be explained away, but rather, how both right and left can be recuperated within the institutional logic and discourse of The Ecole Normale" (p. 13).

Rubenstein's strategic reversal of the question of fascist intellectuals leads her to resituate "the right within the ENS political tradition" (p. 16). Her decentering of the hold of "leftist stereotypes of the ENS" (p. 12) leads her to "reinscribe writing within politics, ideology within institutions, and at the same time, develop a methodology (or principles of a reading) for the study of intellectuals" (p. 16). Refusing to accept the evidence that because the ENS produced "numerically more famous 'leftist' normaliens" (p. 27), its political affiliation is automatically established, Rubenstein embarks on a systematic (although somewhat tedious) debunking of the representation of the ENS as a left-wing breeding ground.

In Rubenstein's convoluted narrative of the production of the *normalien* from the initial selection of candidates through rigorous examinations to the internalization of an elaborate *normalien* culture (which drew very heavily on elitist symbols, including a pompous language canonized as the signal of belles lettres and worked to create familylike ties among *normaliens*), infamous ENS offspring of the interwar years (e.g., Robert Brasillach, Jean Luchaire, Claude Jamet, and Thierry Maulnier), who penned collaborationist propaganda for a network of journals, move to center stage. Rather than seeing them as exceptional, Rubenstein prefers to make them and their fascism paradigmatic of the ENS sensibility: "(T)here would appear to be a fundamental compatability of notions of cult, athleticism, physical ordeals, eccentricity, dandyism, self-conscious elitism, and distrust for the *moyenne*, evidenced in the *normalien*'s self-perception of its rite of institution. . . with those themes of fascist ideology" (p. 68). That this same school could have produced Jean Paul Sartre, Jean Hyppolite, or Simone Weil becomes, in Rubenstein's reading, what needs explanation.

There is an unarticulated connection between "writing" and the "right" that ought to be taken seriously, Rubenstein contends, not only in order to understand the ease with which fascist support was found among *normaliens* but to appreciate more fully the significance of the postwar trials of French intellectuals such as Brasillach and Jamet, who were accused and ultimately condemned for collaborating with the enemy. These trials animated French intellectuals of the period, leading some writers associated with the resistance to circulate petitions for clemency. Rubenstein's book makes an important contribution to the literature on what Otto Kircheimer called "political trials." But instead of reading these trials only as one way for a successor regime to distance itself from its tainted past by symbolically purging—one easily delineated segment of the population" (p. 158), Rubenstein proposes to interpret the trials "as both a judgment on and protection of an educational system that ritualizes speech" (p. 139): "What made the French purge trials noteworthy . . . was the insistence that words were deeds and should be so tried" (p. 139). These trials of "purely literary collaboration" (p. 138) led to the damning of intellectuals for their words and their signatures. Brassilach and others were condemned to death.

The strength of Rubenstein's interpretation of the ENS as a likely producer of fascist collaborators rests on her articulation of the hidden circuit between politics and writing and her insistence on the "tie between the principles of linguistic and political authority" (p. 14). Ironically, this linkage may exceed the boundaries of Rubenstein's own construction of it. She notes in pass-

ing that many of the codes for establishing the *normalien* discourse as an authoritative discourse—linguistic codes such as neologisms, a language of hyperbole, and "poetic devices such as repetition, alliteration, rhyme, etc."—are not wholly unique to this elite" (p. 102). These codes, however, may be more pervasive than Rubenstein intends. Her work draws heavily on "current poststructuralist thought, much of it produced by former students of the ENS: Derrida (promotion 1952), Bourdieu (promotion 1951), Foucault (promotion 1946), and Debray (promotion 1960)" (p. 16). Despite its insistence to the contrary, the discourse of poststructuralism can be read as trafficking in some of the same codes that Rubenstein contends led to the articulation of "notions of hierarchy, difference, and exclusion implicitly, in a manner similar to that of the right" (p. 102). After all, Rubenstein relies on Derrida's and Foucault's own deconstruction of the concepts of agency and subjectivity as disciplinary practices when she concludes that "we should view the author as a victim of his writing which is the social product of an institutional practice—in this case the Ecole Normale" (p. 138). If we can no longer read a text as the property of its author or search for the author's intention as the key to decoding, then the same deconstructivist practice is permissible with regard to poststructuralist texts as well.

Since the political allegiances of one of poststructuralism's most celebrated figures, Paul de Man, have been widely debated, it is strange that he remains unmentioned (except for one reference) throughout Rubenstein's work. Despite the fact that this book is about French intellectuals in the twenties and thirties, the ghost of de Man haunts these pages. The unintended contribution of this densely argued book is to invite us to reverse the question and its focus yet again and ask whether, if we are to see deconstructivist readings and writings as "active interventions . . . that transform contexts" (p. xv), we will not have to ask, What's Left? again.

San Diego State University KATHLEEN B. JONES

Discourses on Society: The Shaping of the Social Science Disciplines. Edited by Peter Wagner, Bjorn Wittrock, and Richard Whitley. Boston: Kluwer Academic, 1991. 386p. $136.00.

In bringing this fifteenth *Sociology of Science* yearbook to a close, the editors claim that the sociology of science is "in a sense the very reflective core of social science." This claim would have made little sense not too many years ago when the sociology of science was just as causal, structural, functional, and nonreflective as sociology itself. The apparent truthfulness of the claim today depends on the sociology of science's having taken a historical turn, made alliances with disciplinary historians and philosophers outside of sociology, and joined the collective effort to undertake detailed investigations of the diverse discourses, practices, and political involvements of flesh-and-blood social scientists in different disciplines, contexts, and times. Thankfully, this sort of reflection is impressively on view in this volume by twelve European and North American scholars in political science, economics, philosophy, and history, as well as sociology.

The 13 essays focus on the shaping of the social science disciplines, particularly in connection with their institutionalization and affiliations to the state. In moving away from history, moral philosophy, and Enlightenment reformism, the social sciences were institutionalized mainly during the late nineteenth century with the formation of different academic associations and theoretical discourses. As a collection, the essays indicate that this process was uneven and differentiated, however, in various national contexts, especially the United States, France, Italy, and Germany. Moreover, the institutionalization was not always at research universities but at training centers for administrative elites (as in French political science) or at commercial colleges and mechanics' institutes (as in English economics). The state played similarly diverse roles in these processes, from actively providing directives about the education of experts to passively playing the constitutive object of grand theorizing. Desrosieres writes, "As the very etymology of the word shows, statistical activity [was even] in league with the State". Yet the intellectual fields established in the late nineteenth century did not generally persist, either in the case of German(-inspired) theories of the state, or even in the case of the so-called classical sociology of Weber, Durkheim, and Pareto. In British political science and Swedish sociology, a clearly articulated social "science" did not emerge until after World War II when "the American model" of behavioral and empirical studies completed its domination of the European imagination. Today's social sciences largely conform to this model and play, according to the editors of the present book, "the correlate of a full-blown modern interventionist welfare state".

The principal casualty of this collective and differentiated portrait, as Anthony Giddens points out in the preface, is the neopositivist view (held by hardly anyone these days anyway) that the social sciences progressively accumulate knowledge in strict autonomy from the rest of social and political life. The methodological consequences of this casualty are not spelled out, unfortunately, save for the editors' fleeting reference to an alternative "critical realism." This philosophical stance, is not shared by all the contributors and would make little if any difference to social scientific practice even if it could be verified.

More engaging methodological issues, however, are raised (or at least displayed) in connection with the historical and sociological interpretations provided by the contributors themselves. Gunnell advocates a "disciplinary archaeology" that peruses the textual monuments of the past and acknowledges only the "congenial ambience" (not the explanatory power) that contexts provide for understanding their meaning. (The editors should have glossed this as a "contextually sensitive textualism" as opposed to a "textually sensitive contextualism"; and much more remains to be done under that label in the case of the history of American political science). Vout would turn our methodological attentions to the "idioms" of social science and its precursors; and he does this, in part, by composing an imaginative "pastiche of inaugural addresses" by Oxbridge chairs of politics between 1920 and 1950. Heilbron rightly calls attention to the importance of "predisciplinary history" for establishing the discourses of incipient disciplines. Desrosieres hails a "social history of cognitive forms" that would connect conceptual schemas to political cat-

egories, especially those about, or provided by, the state. Tribe insists that we should understand the public discourse of the social sciences in institutional, not theoretical, terms and proceeds to attend to the syllabi, class rosters, and fiscal endowments of a variety of educational sites (rightly calling attention to economic historians' "sheer ignorance of what went on outside Cambridge and London"). These various methodological strategies are not all in league; but they prove by their substantive fruits the merits of methodological pluralism in the history of the social sciences if not, indeed, in the social sciences themselves.

Given the strengths of the volume, one complaint must be lodged. If, (as the contributors imply) the separation of academic from public discourse that resulted from institutionalization is regretiable, then what, if anything, might repair that relationship, and can we be sure the academic history of social science does not reinforce that very separation?

University of Minnesota JAMES FARR

AMERICAN POLITICS

Politics and Economics in the Eighties. Edited by Alberto Alesina and Geoffrey Carliner. Chicago: Chicago University Press, 1991. 296p. $45.00 cloth, $17.95 paper.

Shifting the Burden: The Struggle over Growth and Corporate Taxation. By Cathie J. Martin. Chicago: University of Chicago Press, 1991. 251p. $43.00 cloth, $15.95 paper.

It would be hard to find two books on American economic policymaking that were more different than these two. Alesina and Carliner's broadly focused book is an edited volume of articles by major figures in the public choice tradition writing on a wide variety of economic policy areas. It is primarily concerned with the question of whether events in the Reagan–Bush period represented a new relation between economics and politics or simply confirmed existing relationships. Cathie Martin's narrowly focused work is a first book by someone in the quasi-Marxist "state politics" tradition, primarily concerned with the underlying changes in political economy since the 1950s that led to alterations in the corporate income tax. The writers in the Alesina and Carliner volume try to trace everything to personal interest and have little room for the role of beliefs. Martin thinks economic ideology is a prime driver of American economic outcomes. Martin looks at qualitative historical data to determine how policy was made. The Alesina and Carliner writers prefer to look at quantitative indicators where possible. The reader is particularly struck by the fact that the literature most cited in each book is largely ignored in the other. Nonetheless, both are useful and interesting studies that widen our knowledge of economic policymaking.

While neither book will take your breath away, both have something to offer. Martin's book is divided into three parts. In the first two chapters she outlines her theory. In the bulk of the book, she examines five case studies of corporate tax bills. In the short final chapter, she talks of implications for the state autonomy and business power literature.

She wishes to make the case that changes in corporate taxation were predominantly due to a series of alliances between the presidency and various parts of business aimed at promoting economic growth. Although she calls this a coalition, she clearly sees business as the junior partner, organized by the executive branch in pursuit of its growth strategies. Because these growth strategies change over time, the executive allies itself with different sections of business at different times, generally choosing the section that gains the most or loses the least from the proposed changes. She identifies four different periods in the postwar era: a neoclassical period, a Fordian–Keynsian period, a hyperaccumulationist (or supply side) period, and a high technology period with different ideologies on growth. She thinks that the executive linked itself with different sections of business in each period and that major changes in corporate taxation resulted.

Having laid out this theory, she then presents a series of five case studies of corporate tax change since 1960. I found these case studies to be well researched, well laid out, and interesting. She made extensive use of material from presidential libraries for the early period and of interviews with participants for the later case studies. The case studies reveal interesting details about splits within administrations and splits between different sections of business. They also provide new material on the organization of business coalitions by the executive branch in pursuit of corporate tax changes. They are, however, somewhat unbalanced. She gives too little attention to what happens in Congress, to Congress's role in initiating legislation, and to coalitions between Congress and business. In the later cases, I also thought that she was sometimes stretching thin material to try and prove her thesis. For example, an executive–business coalition where only 20 of the top five-hundred corporations sign on is not one that overwhelms the reader with its power. However, one cannot carp too much. Precisely this focus on one element of the political spectrum produces most of what is new and fresh in the book.

Do the case studies support the theory? As is so often the case, the answer is *yes* and *no*. She makes a case that both political and economic views on what causes economic growth have changed over time and that these have sometimes affected corporate tax changes, especially in the short term. Her focus on the business–executive relationship also brings into prominence an element of the political process that has not been much emphasized in other literature. She also clearly demonstrates that the executive branch has often tried to organize a supportive business grouping. Thus, what she stresses is true as far as it goes.

But what she omits is as important as what she includes. There is little discussion of distributive motives, the role of parties and party ideology is shortchanged, and there is only cursory consideration of electoral motives and the interaction between changes in corporate taxes and other tax changes. She also downplays the role of Congress as an initiator of policy (especially in 1981) and fails to look seriously at the alliances between Congress and business. And, despite her mention of Ronald King's work on redistributive taxation, she did not look very seriously at the possibility that growth arguments are a way of disguising tax changes that have unpopular distributive consequences. To be fair, many of these points are mentioned in one or another of the case studies; but they are then downplayed in the summaries and theoretical sections.

A few other points bear mentioning. I found her grasp of the literature in the area somewhat selective. There is no mention, for example, of Pierce's earlier study of the Kennedy–Johnson tax changes nor of relevant work by Hibbs, Tufte, Gordon, Buchanan, Sundquist, Page, or Wildavsky. I found the part of her theory arguing that we have had four paradigm shifts in 30 years unconvincing. I read her case studies as showing that business acted mainly in its short-term interests, rather than as part of an alliance. I also have a bias toward testing one's theories against plausible alternatives. There was little attempt to do so in this book. The reader will get from this book the first serious look at the politics of corporate taxation, some interesting case studies, and an attempt

to create a theory that is interesting, if not always clear or supported by the data.

The Alesina and Carliner book is an attempt to apply the insights of public choice theory to economic events under the Reagan administration. There is a conscious attempt on the part of most (but not all) of these writers to eschew their usual mathematical models and quantitative tests and to make what they have to say more accessible to the mathematically impaired. There are two obvious questions. One is, What does the book have to say about the political roots of the economic changes under Reagan? The second is, When shorn of the equations, what insights does the public choice perspective give into economic policy making?

The articles in the book deal with most of the major macro policy areas. They discuss political business cycles, monetary policy, tax policy, welfare expenditures, the deficit, trade policy, the savings-and-loan crisis, and minimum wage legislation. Most of the articles contain as part of their substance a brief summary of developments in each area in the 1980s. With the exception of the article on the deficit (which I found misleading), these minihistories are well executed and informative, going beyond mere description to provide insights into the motivations of key policymakers and how external events impinged on them. I. M. Destler's account of trade policy developments is especially well done. The articles also contain excellent summaries of the state of the art in the public choice literature. Anyone wanting to find out what the public choice theorists have been doing in this and related areas in the last five years could not find a better place to start. Fiorina's summary of the political business cycle literature is especially masterful.

But what does this approach teach us that we did not know before? The answer is *not a whole lot*. We learn that presidents do better when the economy is strong; that regulatory agencies get captured by members of congress, who themselves get captured by active constituents; that the vote of members of congress is affected by their party affiliation and their ideology; that when the two parts of Congress are in the hands of different parties, there is more conflict and that this conflict is often resolved by increasing expenditures; and that the president's veto power is important. While these points are developed professionally and applied well to the data, they are not breakthrough insights.

The book is blessed with a number of smaller insights and elaborations on common models that add to our knowledge on the margins. But there are also one or two more provocative insights. My own favorites were the ruminations on the role of the Federal Reserve and the Office of Trade Protection in the articles by Jim Alt and I. M. Destler. The latter has an interesting variant on Kane's (1987) model of regulatory agencies as convenient scapegoats. Alt uses principal–agent theory to combine the somewhat disparate evidence on who really controls monetary policy. I also liked Fiorina's attempt to show that party affiliation was a lagged effect of economic policy. While I was unconvinced by his model specifications and found his evidence less than awe-inspiring, the idea is one that I have always thought had possibilities; and Fiorina makes a good logical case for it.

While these chapters have many virtues, they also have omissions and shortcomings. The most obvious is the excessive concentration on Congress. While Martin gives Congress too little attention, the writers in this book give it too much. Eager to combat the view that Reagan altered policy, this group of congressional specialists want to attribute most of the changes to Congress. Six of the eight papers look at little but Congress, and all downplay the role of the president. Private actors are important only insofar as they affect congressional action and are viewed through a congressional filter. This concentration often produces interesting insights into how Congress deals with economic policy, but it gives a somewhat distorted view of the total policy realm. There is also an excessive tendency to claim that extraordinary policies followed from ordinary political processes. While this might be plausible once, it is less plausible over the entire range of policies.

Finally, while many of the explanations offered were reasonable, others would often have been equally reasonable. Congressional timidity on trade issues could be due to a fear of being labeled unpatriotic when the president is combatting trade antagonists; deficits could be due to changing economic views, rather than a divided Congress; the savings-and-loan crisis could be primarily the result of Reagan's policy and high deficits, rather than congressional capture; and so on.

The two books together show that what one gets out of the process depends significantly on the models one takes into it. The Martin book comes out of traditions that emphasize the state, business power, and ideology; and she finds that corporate taxes are formed by an executive–business alliance that is affected by current growth ideology. Most of those writing in the Alesina and Carliner collection study Congress, and believe in the power of rational self-interest and institutional structure to shape outcomes; and they find that economic policy outcomes are largely determined by congressional structure and the self-interest of members of Congress. Such specialization is both the strength and the weakness of these books.

Overall, these are good books written by intelligent people in a difficult area. While the Alesina and Carliner volume is clearly the stronger of the two books, both are well worth reading, and both add to our knowledge about economic policy and our understanding of the uses and misuses of political models.

California State University, Fullerton PAUL PERETZ

Assessing the President: The Media, Elite Opinion, and Public Support. By Richard A. Brody. Stanford: Stanford University Press, 1991. 198p. $22.50.

It is difficult to break new ground in an area of research as well trod as the study of presidential popularity; yet this book succeeds admirably by offering noteworthy theoretical and empirical insights. Brody provides a worthwhile synthesis of much of his earlier work and perceptive new appraisals of the subject. Especially for people not in the field, his cogent review of the voluminous literature on honeymoons, rally events, time, and the economy is "must" reading.

The central thesis of the book is that Americans judge presidents based on their impressions of news reports about presidential policy successes and failures. According to Brody, the news is not merely a reporting of the details of an event itself. It also involves interpretations of the event by political opinion leaders (or elites). When the significance of an event is appreciated by the public, their evaluations of a president will depend primarily on

the description of the event in news stories. When the significance of an event is more ambiguous, its meaning will be supplied by the reactions and interpretations of opinion leaders, which are carried as part of the news. If these interpretations are flattering toward a president, Brody argues that the public's evaluations will be similarly buoyant. If public opinion leaders are less enthralled, so, too, is the public at large. Thus, there are two parts to the theory: the news covers elite opinions and the news and elite opinions shape public opinion.

Brody's news model of presidential approval does not preclude consideration of such factors as the honeymoon and rally events, which have been pivotal in other explanations of the rise and fall of public evaluations. Brody maintains that opinion leaders tend to mute their criticisms of a president early in the term (the honeymoon) and during dramatic international crises that pit the United States against an unrelenting foe (rally events). Brody offers the first systematic comparison of the early period of presidents' first and second terms. The results suggest that while there is a honeymoon plateau of support in the first term, there is none in the second term. Presumably by the second term, opinion leaders are comfortable criticizing a known commodity. Brody also describes elite reactions to various international crises, from the U-2 incident during the Eisenhower administration to the Iran–Contra affair of the Reagan administration. He observes that during some of these crises, when the danger seems remote and presidential folly is apparent, elite criticism, rather than kudos, mounts. In this way, the international crisis (e.g., Iran–Contra) becomes a negative event, rather than a rally event, causing presidential approval to fall.

The core of the book is an intriguing analysis of the news model, offered in two parts: Kennedy through Ford and Carter and Reagan. Unhappily, because of some lost data sets, a combined analysis from Kennedy to Reagan is not presented. The news stories are distinguished between foreign and domestic news; policy proposals and policy results; and good, bad, and neutral stories. The results show that the net difference between good news and bad news affects public approval (relative to the effects of presidential broadcasts and the honeymoon in the first period and to the honeymoon, the economy, past approval, and the hostage taking in Iran in the second).

Even with this ambitious effort, work remains to be done in three areas. First, while Brody clearly outlines the relationship between media coverage and public opinion, he leaves for another book a test of the indirect relationship posited between opinion leaders and public opinion. As currently presented, opinion leadership is only vaguely defined, and opinion leaders constitute a somewhat amorphous group. They are primarily reporters' sources, including the president, vice president, presidential candidates, some members of the White House staff, the congressional leadership, some committee chairs, and columnists. No mention is made of governors, cabinet members, business leaders, or reporters and anchors themselves. In addition, the empirical treatment of elite opinion is largely anecdotal. Future efforts can develop a classification scheme that distinguishes news coverage of opinion leaders' comments about an event from news coverage of the event itself. This would provide a systematic test of the posited effect of elite opinion on news coverage and offer a way of judging whether opinion leadership takes place.

Second, the book does not fully distinguish between presidential opinion leadership and other elites' opinion leadership. In the Kennedy–Ford period, a variable measures presidential broadcasts, but this is not repeated for the Carter–Reagan period. The book acknowledges, but does not study, White House efforts to counter elite criticisms, create elite support, and generate its own favorable publicity. Other projects can build these activities into the news model more fully. Third, as part of presidential opinion leadership, presidents often offer symbolic presentations, as well as the policy proposals and outcomes Brody discusses. Many news events are mixtures of both. It would be important to know how these symbolic components of presidential news affect public approval.

In all, this valuable book offers scholars of the presidency, the media, and public opinion an approach that should be taken seriously and that offers an opportunity for future research.

University of Arizona LYN RAGSDALE

Ballots of Tumult: A Portrait of Volatility in American Voting. By Courtney Brown. Ann Arbor: University of Michigan Press, 1991. 232p. $34.50.

Courtney Brown's book is an ambitious one. He attempts to model electoral volatility in the United States in such a way as to resolve the debate about mobilization versus conversion during the New Deal realignments; develop a general picture of the impact of third parties on the American electoral universe; explain "where all those new female Republican voters came from in 1920"; and build a model of the congressional mobilization cycle incorporating variables derived from a wide range of theories about congressional voting. Brown argues that he addresses these questions from a new perspective, namely, "the possibility of fixed dynamic structures as characteristics of volatile mass electoral behavior differentially related to social context and evident across a wide range of events has not been extensively explored. . ." (p. 11).

Ballots of Tumult contains, in addition to its theoretical chapters, a case study of the dynamics of presidential voting during the New Deal realignment (1928–36); a chapter examining the sources of third-party support (and the subsequent movement of "demobilized" third-party supporters in the instances of the Populist party, Roosevelt in 1912, La Follette in 1924, Wallace in 1968, and Anderson in 1980); a chapter on the 1920 election that examines the differential mobilization of women into the Democratic and Republican parties; and a chapter that attempts to build a new and synthetic model of voting behavior in off-year congressional elections. Brown develops systems of differential equations to investigate each of these problems. For example, in the New Deal chapter, he illuminates (in my view quite successfully) the relative impact of conversion and mobilization by proposing three equations that together constitute an interdependent system describing the over-time fluctuations of three groups in the population: Democrats, Republicans, and nonvoters. Moreover, to communicate the results of his estimations, Brown offers the reader innovative and generally appealing graphic devices (though some are more successful than others).

Brown's is clearly a "bottom-up" perspective on po-

litical change. Leaders follow the masses; therefore, it makes sense to work on developing theories of mass behavior in the relative absence of theories of elite behavior. He makes fascinating and cogent points about how organizational dynamics might tie into his model. For example, he speculates that the differential mobilization of women into the parties in 1920 was a function of the local parties' electoral requisites. Nonetheless, he often seems more comfortable talking about mass behavior as a response not to intentional behavior on the part of elites but to "social forces" or "particular social chemistries indigenous to localized milieus" (p. 187).

This said, Brown does take into account critical contextual variables. Not only are his models "conditioned" so that we can see clearly, for example, the relationships between x and y for farm areas versus urban areas, but local levels of two-party competition are central to his view of how electoral dynamics work, and these kinds of variables are built into his models. This seems to me a clear strength of the book. Another is his lucid and useful discussion of the advantages and disadvantages (or pitfalls) of both aggregate voting data and cross-sectional survey data in the analysis of electoral change. He argues persuasively that neither is to be preferred consistently over the other. Finally, independent of the book's argument, Brown has clearly accomplished a monumental and valuable task by integrating diverse data from the ICPSR into a single data set containing county-level electoral and census data.

Ballots of Tumult will serve an important heuristic function, stimulating not only attempts to develop alternative models, but micro-level historical research, as well (which the author suggests is necessary to reach a better understanding of the mobilization of women in 1920). The chapter on the New Deal realignment is perhaps the most clear and convincing. The chapter on the congressional mobilization cycle and that on women in 1920 have a certain ad hoc quality about the explanations; but both, like the book as a whole, raise all sorts of interesting questions. Any reader interested in American voting behavior will find an idea or problem in this book that will encourage the rethinking of a question, the redoing of an analysis, or the revision of a conclusion.

Syracuse University KRISTI ANDERSEN

Catholic Bishops in American Politics. By Timothy A. Byrnes. Princeton: Princeton University Press, 1991. 189p. $29.95.

The Bible and the Ballot Box: Religion and Politics in the 1988 Election. Edited by James L. Guth and John C. Green. Boulder: Westview, 1991. 250p. $34.95.

These two books, one a focused examination of a particular religious elite, the other a collection of essays and research reports about religion in a single election, illustrate well both the strength and weakness of contemporary research on religion in American politics.

In studying the political role of American Catholic bishops, Timothy Byrnes might have emphasized the *process* by which the church hierarchy formulates political policy and strategy. The fascinating story of how the bishops balance their own views with the conflicting imperatives of the Vatican, parish priests and nuns, professional staff members, Catholic interest groups, the various religious orders, and the variegated and increasingly vocal American Catholic community has yet to be fully told. Alternatively, a book devoted to the bishops as political actors might have explored their *impact*, asking how their well-publicized messages on subjects as diverse as nuclear weapons, abortion, and the American economy influenced political elites, ordinary Catholics, and the American public. Eschewing both approaches in this slim book, Byrnes's principal aim is rather to chronicle and explain the emergence of the bishops as a provocative political force in recent American politics.

The recent change in the bishops' leadership style—their increased visibility on a range of controversial issues—was associated with the social transformation of American Catholicism. When they represented an immigrant church whose members suffered poverty, discrimination, and accusations of disloyalty, the bishops' principal political role was local in scope and defensive of Catholic interests and cultural values. When Catholics came out of the ghetto in the mid–twentieth century, the bishops similarly shifted their political role to encompass a broader range of political issues and values, pursue a more national political agenda, and assert a distinctly Catholic political ethic. The increased self-confidence of American Catholics and the enhanced organizational capacity of the bishops—the most common explanations for the new involvement—are only part of the story. According to Byrnes, the bishops simply adjusted to major shifts in American political life—the nationalization of American politics, the politicization of new issues, and efforts by other political elites to capture the Catholic vote. If the bishops became major actors on behalf of the prolife movement in the 1976 and 1980 presidential campaign, that is largely because the Supreme Court transformed abortion into a national issue, numerous candidates attempted to associate themselves with the bishops' agenda, and dealignment loosened the ties binding Catholics to the Democratic party, making them an attractive target for mobilization by conservatives. To those who regard the bishops as unwelcome interlopers trying to impose Catholic values on politics, one might say the bishops were more sinned against than they were sinners.

While the 1988 election may have seemed less overtly religious than its recent predecessors, the essays collected by James Guth and John Green in *The Bible and the Ballot Box* demonstrate a considerably more complex reality. While many observers discounted the role of religion once Jackson and Robertson departed the scene, the studies show a continuing if subterranean religious presence animating elites and ordinary citizens. The chapters following Allen Hertzke's essay—essentially a cultural argument that religion supplied the populist motif in the campaign—examine the political activities of denominational leaders and organizations, the impact of religion on political activists, and surveys of religious influences on the voting of major religious groups—white Protestants, black Protestants, Catholics, and Jews. The collection contains considerable original research, utilizing surveys of Protestant clergy, campaign contributors, and black Americans, as well as elite interviews and secondary analysis of more familiar exit polls and data sources.

In their conclusion, the editors extract an important theme from the diverse studies: "The United States is moving away from the ethnocultural foundations of

political alignments, based on conflict between religious traditions, toward a more European pattern, in which conservative parties draw heavily from religious groups and leaders, while parties of the left are supported by more secular forces" (p. 207). As they recognize, this conclusion applies with more force to elites than to masses and does not fit some groups, notably Catholics, black Protestants, and Jews, as well as others. The strongest evidence for it, the disparity in religious commitment between contributors to Republican and Democratic political action committees in chapter 7, is also somewhat suspect. The lower mean score of Democratic contributors on a measure of "traditional religiosity" is potentially an artifact of the relatively large proportion of Jewish contributors in the Democratic camp—a group whose religious orientations are not well represented by an essentially Christian index including questions about biblical literalism and life after death. To appropriate Milton Yinger's dictum, Democratic contributors may be just as religious as their Republican counterparts but religious in a different way.

Both books do a good job of challenging conventional wisdom that portrays religion as a marginal, predictable, and reactionary force in American politics. Byrnes shows, for example, that the Catholic bishops are not a monolithic force but are seriously divided between those who elevate abortion above all other issues and those who regard it as one element in a package of concerns for a "consistent ethic of life." His analysis, along with the studies of religious elites in the Guth and Green volume, demonstrates that the extent of political engagement in any particular campaign depends both on internal factors and the stimulus offered by the political system. Similarly, the studies of groups as diverse as campaign contributors, convention delegates, and voters shows that religion may support a wide variety of political positions and values. After reading these volumes, there is clearly no justification for the all too common tendency to overlook or caricature religion as a political force.

But the two volumes also demonstrate a conspicuous weakness among studies of religion in electoral politics—a tendency to favor description over theorizing. Precisely because of their descriptive nature, these books are rich sources for lecture material and could profitably be incorporated in undergraduate courses. Yet it is hard to say that our efforts to build conceptual understanding of the politics of religion are much advanced by either volume. Byrnes modestly avoids calling his work a theory of elite Catholic mobilization, though there is something approaching theory lurking in his case study. I wonder, for example, whether the same forces he credits with bringing Catholic elites into the political arena might equally well explain the contemporaneous political emergence of fundamentalist Protestant clergy or even some nonreligious groups. A framework such as resource mobilization might make the results of Byrnes's research more interesting to other scholars, and using it could enable Byrnes to offer insights that transcend his case study. Similarly, the excellent studies in the Guth and Green volume tend to stick to the facts rather than connect findings to extant theories about mass political behavior. This is a pity because the studies shed valuable light on such interesting themes as group consciousness and cohesion, social identification, cultural politics, and organizational encapsulation. Unless research on religion is connected explicitly to the theoretical concerns of the larger scholarly community, it is unlikely to influence mainstream research. It would be a supreme irony if scholars of religion in American politics should build a research ghetto just as the subjects of their labor—major religious groups—have emerged to claim a place at the heart of American politics.

University of Florida KENNETH D. WALD

Collective Action and the Civil Rights Movement. By Dennis Chong. Chicago: University of Chicago Press, 1991. 261p. $42.50 cloth, $16.95 paper.

This fascinating book brings together a number of intellectual and research activities that are usually carried on by different kinds of scholars, usually in different forums and without mutual interaction. It would be a mistake for the reader to suppose that this book is a richly detailed, substantive account of the postwar Civil Rights movement in the United States. Rather, it is a successful and needed illustration of how formal theory is developed and used to explain a complex and important phenomenon, in this case, the Civil Rights movement in the United States after World War II. Although the substantive focus is the Civil Rights movement, the author often demonstrates the wider applicability of the approach and analysis to other "public-spirited" movements.

Building on the work of Russell Hardin and Mancur Olson, theorists of collective action have sought to explain the conditions under which utility-maximizing, rational, and self-interested individuals act to produce results that require their voluntary contributions of money, time, effort, and risk to personal well-being. When the object is to achieve justice, fairness, due process, and other public-spirited objectives, the usual problems associated with the efficient production of public goods are accentuated. The special difficulties associated with private provision of public goods like civil rights, women's and gay rights, and even resource preservation and historic preservation are plagued with such routine public goods traits as jointness of supply, nonexcludability, and nonrivalry in consumption.

Dennis Chong formulates a general model of collective action and then applies it to the modern Civil Rights movement. Moreover, he accomplishes this in a manner that should not disaffect those who are uninitiated in the argot of rational choice approaches. Whenever possible, the conceptual apparatus of game theory and public choice are defined in direct, conventional terms, thereby illustrating that there is no necessary reason for formal theory to be surrounded by a notational thicket of symbolic logic or mathematics. Only reflexive fear or hostility should prevent a reading and understanding of the straightforward mathematics and graphical presentation that are sometimes used; in all cases, the verbal explanations are clear and comprehensible.

Considering its conceptual apparatus, it is a relief to find that the text is logically organized. Chong begins with an introduction to his topic, generally phrased as the problem of explaining "public-spirited collective action." The overarching puzzle of collective action is nicely summarized, as are the traditional terms employed to account for it, including public goods, prisoner's dilemma (and its variants, such as an iterated

prisoner's dilemma), free-rider, and expressive benefits, among others. The outline of *Collective Action and the Civil Rights Movement* is spelled out clearly and, more importantly, implemented in subsequent chapters.

Chong's analysis is energetically eclectic. For example, early in his analysis, Chong employs the notion of an "all-or-nothing" public good to explain why boycotts and nonviolent demonstrations were effective and sustained in the early days of the postwar Civil Rights movement. He sprinkles the narrative and explanation with references to primary and secondary historical sources and concepts borrowed from the study of social psychology and communications. He ends the second chapter by pointing out that while the persistence of boycotts and protest can be accounted for in game-theoretic and public goods language, the account still does not explain why individuals with every incentive to free-ride actually participate in boycotts and nonviolent demonstrations.

In chapter 3, a variety of "social incentives and reputational concerns" are introduced to answer this question. Friendship, social standing, social networks, and other nonconventional and nonpecuniary incentives are joined with a game-theoretic application of Axelrod's iterated prisoner's dilemma to help explain individual decisions to join and sustain civil rights activity. Chong illustrates how individual behavior and expectations interact in groups so as to create social incentives and reputational concern sufficient to incite and sustain participation in civil rights activity. This chapter is especially interesting, since it focuses on perhaps the core collective choice problem and even provides implicit guidelines on how to mitigate the tendency to free-ride. Chong fails, however, to account for how one can anticipate the conditions under which particular social incentives or concerns for one's reputation will work for any individual. Indeed, there is more than a whiff of post hoc and tautological reasoning in evidence here. After all, concern for one's reputation as a reason for participation is selectively supported by the biographical or repertorial evidence cited by Chong. How to anticipate when such factors do or do not exert influence is not addressed.

The benefits and shortcomings of the overall approach are further illustrated in Chong's treatment of "narrowly rational expressive benefits." These benefits refer to the satisfaction an individual gains from the mere act of doing something, rather than from the putative goal of the participation (e.g., justice, equal opportunity, or abortion rights). A sense of revenge, a feeling of community, and a feeling of fun are presumably folded into calculations of benefits and costs and often produce unexpected levels of participation in public-spirited movements. Finally, with sufficient introduction of nonmaterial, psychic benefits and social-psychological qualifications, Chong gets us to the critical notion of "assurance games," a family of the more general coordination games. A formal model of collective action in pursuit of public-spirited objectives is eventually provided; and only enough mathematics is used to draw out inferences and generate expectations about the impact of various interacting variables on civil rights participation (e.g., the relationship between group demand making and official response, the impact of the schedule of response by officials, and the number of participants in the collective action). Ultimately, the model provides an explanation for the conditions under which participation, success, and, finally, decline in the Civil Rights movement occurred.

In the end, Chong explains that the Civil Rights movement experienced collapse because it was successful in satisfying its participants with its initial agenda and its various "material, social, and psychological benefits." The concluding portion of this book is perhaps least impressive. Chong simply suggests the disappearance of civil rights activism was the consequence of "tired radicals who [were] satisfied with their efforts or disappointed over the impact of their achievements" (p. 239). Saddled with either satisfied or frustrated former activists or cynical contemporaries who are unmotivated by the older schedule of reinforcement, the Civil Rights movement, as it traditionally manifested itself, died.

Yet even though Chong's conclusions about the Civil Rights movement are not startling, he does illustrate the value of rational model approaches to generating a framework for cumulative and replicable social inquiry. The reader should not dismiss this very useful, interesting text for the whimper of its conclusion. The bang here is in the demonstration of how formal and mathematical reason can be employed on behalf of substantive, even qualitative concerns. In this sense, Chong has provided a valuable, successful contribution.

University of California, Riverside MAX NEIMAN

The Postreform Congress. Edited by Roger H. Davidson. New York: St. Martin's, 1991. 354p. $24.00 paper.

Congressional Careers: Contours of Life in the U.S. House of Representatives. By John R. Hibbing. Chapel Hill: University of North Carolina Press, 1991. 213p. $29.95 cloth, $12.95 paper.

Although discussions of the state of congressional research regularly call for fresh data to reassess extant theories and for longitudinal studies that track and explain change, much of what we think we know about the Congress still comes from time-bound snapshots that may no longer be accurate. Congressional scholars need to be concerned with change in the behavior of individual members of the institution and change in the structure and practices of the institution as a whole. A concern with the former drives John Hibbing's careful analysis of congressional careers and a concern with institutional change motivates the essays on the "postreform" Congress that Roger Davidson has drawn together.

Typically, change in Congress has been described in institutional terms, utilizing time series data of the sort reported in *Vital Statistics on the Congress* or in over-time analyses of legislative voting patterns. Hibbing, however, is interested in change at the individual level, specifically during a member's career. He points out that although the literature is filled with findings and assertions about phases of the congressional career and about the impact of increased tenure on the behavior of legislators, the evidence in support of these conclusions is not always very strong. Some are based on little more than anecdotes; others are arrived at either by comparing the behavior of more-senior members with that of less-senior members during the same time period or by comparing cross-sectional data collected at different times. Such research designs can only suggest the change that takes place as members move through their

careers and, more importantly, cannot distinguish the change attributable to increased tenure from the "period" effects that impinge on the entire Congress.

To remedy that situation, Hibbing takes a longitudinal approach, developing and analyzing a huge data set covering the full careers of each member of the House of Representatives who entered the body from 1946. As members move through their careers, he looks at five aspects of change: in their electoral situation, in their formal positions within the Congress, in their roll-call voting behavior, in their legislative activity, and in their district activity. In order to test for change across a legislator's career, Hibbing uses standardized measures so that, for example, changes in member involvement in legislative activity attributable to tenure can be distinguished from changes attributable to revised institutional rules and norms governing participation.

These institutional changes have been especially apparent during the last two decades as the Congress moved through the reform era of the 1970s to the postreform era of the 1980s. The Congress of the earlier period was characterized by weak party leadership, strong subcommittees, highly decentralized power, and wide open participation by all members on all issues. This structure, it was argued, well served the needs of members increasingly dependent for their reelection upon their own individual efforts.

The Davidson volume demonstrates that although important elements of the 1970s system remain in place, changes did occur during the 1980s. The scholars contributing to the volume find a resurgence of partisanship in legislative voting behavior (David Rohde), an enhanced role for national political party organizations in congressional elections (Paul Herrnson), a strengthened role for party leaders in the House (Dan Palazzolo), new strategies and opportunities for party leaders (Barbara Sinclair), and new techniques that subcommittees use to defend their proposals in a more democratized House (Barry Weingast). Davidson provides a highly readable historic overview of the emergence of the postreform Congress; and Paul Quirk evaluates congressional performance, a project all too seldom undertaken by congressional scholars.

For the most part, these articles explain change in terms of institutional adaptation to external variables, particularly the altered political environment and policy agenda that arose in the 1980s as a result of changes in voting behavior, divided government, sharper ideological cleavages between the two parties, and higher levels of conflict between the president and the Congress. James Pfiffner, James Thurber, and John Gilmour provide strong discussions of how these changes have affected the manner in which Congress and the president make public policy.

As one would expect, there is some unevenness in the quality of the articles in the Davidson book. They display the wide range of approaches that always has characterized legislative research, from the purely descriptive, even impressionistic, to the more rigorous theoretical and empirical work typical of journal literature. Hibbing's book, in contrast, is a methodological tour de force. He carefully reviews the extant literature on congressional careers, identifies areas of consensus and controversy, and uses his vast data base to reassess much of what we know or think we know about careers.

In the process, some conventional wisdom is reaffirmed. Increased tenure in the House means increased legislative involvement and specialization, lower participation rates on roll calls, a more conservative and somewhat less partisan voting record, and, possibly, somewhat less attention to the district (although the data on this last point are not very strong). But some conventional wisdom also goes by the boards. Increased tenure does not mean greater electoral security. Although formal positions of power come somewhat earlier in the careers of members elected after 1969 than for those elected before, there is little evidence that power is more equitably distributed now than in the past. And Hibbing's data suggest that the widely reported death of the apprenticeship norm may well have been exaggerated. In recent years, the gap between participation rates of new and senior members is greater than it was in earlier years when the apprenticeship norm was supposedly in full flower.

Generally, Hibbing questions the prevailing view of a cyclical congressional career, discovering at several points that recent careers are characterized more by constancy than by change. Increasingly, behavior early in a legislator's career predicts behavior later in the career. The one exception to this generalization—the clear growth in legislative involvement and specialization over the legislative career—leads Hibbing to the conclusion that term limitation would be a bad idea, because it would deprive the Congress of valuable legislative expertise.

Although both of these volumes will find their way into the library of every scholar interested in the Congress, they are also written in a manner that will make them accessible to students and to those in the profession who do not specialize in the Congress literature. The Davidson book will replace a number of reform era anthologies that have long been staples of undergraduate Congress courses and will, I suspect, contribute to the updating of more than a few introductory Congress lectures. But if, as Hibbing argues, there is no longer a life-cycle for legislators, there certainly is one for Congress books; and with the possible exception of the Rohde and Weingast pieces, there are no "classics" in the Davidson collection. Rather, its strength lies in its timeliness; and as times change, the utility of this volume will decline. Because Hibbing takes the long view, because he builds explicitly on the research literature, and because he is meticulously attentive to data and its proper analysis, his book constitutes the more solid and enduring contribution to scholarship on the Congress.

DePaul University MICHAEL L. MEZEY

Why Americans Hate Politics. By E. J. Dionne, Jr. New York: Simon & Schuster, 1991. 430p. $22.95.

America's Constitutional Soul. By Harvey C. Mansfield, Jr. Baltimore: Johns Hopkins University Press, 1991. 236p. $25.95.

Why have Americans lost faith in government? How has American politics gone awry? What has led voter participation in presidential elections to decline steadily since the 1950s and divided government to persist and rendered strong presidential leadership so elusive? And, finally, what is to be done?

E. J. Dionne, Jr. and Harvey C. Mansfield, Jr. address these questions in their books. The failure of both

liberalism and interest group pluralism in the 1960s, for both authors, is at the root of the problems confronting Democrats and Republicans in the 1990s. Both also look to a revival of the tradition of civic republicanism in American political thought. But they would revive republicanism in different ways, because they draw different lessons from that tradition for constitutional politics.

Writing for a popular audience, Dionne aims to provide a kind of intellectual history for what has gone wrong in American politics. Voters are frustrated, he argues, because the Left and the Right pose "false choices"—for example, between prochoice and prolife stances on abortion. Americans remain ambivalent about abortion, however. They overwhelmingly favor allowing women the legal right to choose an abortion but deem most abortions morally wrong. Liberal Democrats have lost touch with the values of the middle class, Dionne observes, while conservative Republicans increasingly fail to represent the interests of Americans. Sharply polarized campaigns, sound-bite commercials, and attenuated presidential debates divert us from a serious quest for common ground, or a "new political center" that would revive civil responsibility and build consensus on policies for education, the environment, health care, and other problems facing the middle class and the poor (p. 338).

The liberal consensus that dominated politics from the New Deal coalition, through the Eisenhower administration, to the Great Society programs broke down in the 1960s. That was due to its successes and excesses, as well as from mounting pressure from the Left and the Right. The New Left railed against the "liberal establishment" for not moving far enough toward a more egalitarian, participatory democracy at home and for waging the Vietnam War abroad. Conservatives also distrusted "the establishment," though for different reasons: the establishment was liberal and pushed social welfare policies too far. Notably, Dionne fails to mention that the Warren Court also came together in the 1960s, revolutionizing criminal procedure and greatly expanding civil rights and liberties. Rulings of the Warren Court—and later the Burger Court—reinforced the defection of white middle-class voters, especially in the South, from the Democratic party. Already angry over the passage of the Civil Rights Act, they became increasingly bitter over affirmative action, forced integration, and much else. "Judicial activism," "crime control," busing, and quotas became powerful symbols for Republican presidential candidates from Richard M. Nixon to George Bush. In this regard, the influence of race, civil rights, and economic issues on American politics is handled much more persuasively by Thomas and Mary Edsall in their book *Chain Reaction* (1991).

Dionne follows conventional wisdom in pointing to the decline of political parties and how reforms in the nominating convention of the Democratic party opened up the process at the cost of rendering it the party of special interests. With the breakup of the old New Deal coalition and unable to integrate interest group politics anew, Democratics promised virtually every group something and delivered little, winning only one in the last six presidential elections.

From the failures of liberalism, the second half of *Why Americans Hate Politics* turns to the ascendancy of the conservative Right. After the "fusionist consensus" (p. 161) forged among champions of free-market economics and traditionalists concerned with civic virtue, Dionne surveys the rise of the right wing of the Republican party. Prominent in his story is the neoconservatives' influence and critiques of social programs that went wrong, produced unintended consequencies, and raised social expectations. Republican victories in the 1980s, Mansfield agrees, repudiated much of the 1960s counterculture. Yet they also rested on a precarious coalition of libertarians, religious fundamentalists, neoconservatives, and conservative populists. And both authors note how easily that coalition could come apart.

Government once worked reasonably well. Still, it is little wonder that Americans are losing faith. Both the New Left and the New Right preached distrust of government: "Government is not the solution, it is the problem." The performance of Democrats and Republicans rarely matched their promises. Government succeeded in its failures, rather than building on successful policies. Conflicts over race and civil rights remain, though transformed, from the "cultural wars" of the 1960s and 1980s. They are compounded, moreover, by the further deterioration of communities and inner cities; overcrowded prisons, despite a decades-old battle against crime, tough new laws, a freer hand for law enforcement, stringent federal guidelines for sentencing, and more executions; the excesses of deregulation that led to the savings-and-loan crisis; and a budget deficit that looms ever larger.

Dionne and Mansfield agree on some factors contributing to the problems of governance: the decline of a competitive two-party system and patronage, the pervasive influence of the commercial media and incumbents' preoccupation with campaign contributions, populist appeals, and overly heightened expectations about governmental programs. However, Dionne looks for a new blend of political pragmatism, whereas Mansfield confesses he has "pretty much given up on liberals" and "appeal[s] more directly to conservatives" (pp. ix–x). The problem with liberalism for Mansfield runs deeper than for Dionne. Indeed, the problem lies precisely with pragmatism, participatory democracy, and the abandonment of the formalism and "liberal constitutionalism" of limited government.

America's Constitutional Soul is largely a collection of previously published essays. Part 1 attends to each presidential and congressional election in the 1980s. Mansfield looked toward a possible party realignment and President Ronald Reagan's returning to public discourse respect for the Constitution, while also warning against the New Right's embrace of pragmatism and populism. Pragmatic adjustments nonetheless prevailed; Reagan's legacy is "summed up in the deficit" (p. 62). Part 2 addresses pride and constitutional conservatism, while part 3 deals with religion, separation of powers, and choice and consent in modern constitutionalism. Finally, part 4 defends formalism and "constitutional forms" against the other preoccupations of political science, the mass media's "formless representation" and the informalism of populism and participatory democracy (p. 174).

Americans, according to Mansfield, have been led astray by pragmatism into believing in interpretative choices in their individual and collective self-determination and thus turned away from the "soul" of American constitutionalism: "What America needs is a certain constitutional space between people and government to allow the government to regain control of its affairs, thereby enabling the people to regain control of its

government" (p. 45). By contrast, for Dionne, pragmatism and public choice remain the heartbeat of American politics. Scholars already converted to the work of students of Leo Strauss will find Mansfield's arguments useful, particularly in debates over the revival of republicanism. Others may find them, however engaging, less convincing in light of the robust debates waged over rival theories of republicanism (romantic, rhetorical, and antiscientific pragmatism vs. more behavorialist, quasi-scientific versions), as well as over the call for a return to a jurisprudence of original intention. At the same time, Dionne's invocation of the republican tradition and recommendations for reaching compromises on "back-to-basics priorities, [such as] a kind of GI Bill for the 1990s" (p. 348) and vouchers will strike many readers as too easy and conciliatory.

In short, for Dionne, the "body politic" ails because voters confront "false choices," whereas Mansfield maintains that more is at stake: America's "soul is not healthy today" (p. 209) because voters believe they are entitled to choose and, confused about their interests and values, face too many choices. Dionne's solution—more deliberative pragmatism in renewing public life—for Mansfield, only perpetuates problems: what is needed is renewed respect for the formalism of the Constitution, government institutions, and public life. Both books thus merit as much attention for their opposing understandings of the theory and practice of constitutional politics as for their assessments of what is wrong with the political process.

University of Virginia DAVID M. O'BRIEN

Antitrust and the Triumph of Economics: Institutions, Expertise, and Policy Change. By Marc Allen Eisner. Chapel Hill: University of North Carolina Press, 1991. 314p. $34.95.

The first three paragraphs of this book hold out great promise for what is to follow. With a sense for the broad historic vista, the author links antitrust to the foundational principles of the American political economy. He then begins to build his argument, contending that major transformations in governing institutions and alterations in antitrust policy are closely intertwined. Happily, the book fulfills its initial promise. Although the author does not establish connections between changes in antitrust and large-scale political and economic changes that have taken place over the hundred years that antitrust law has existed (as one is led to hope for by the initial discussion), overall, he superbly integrates institutional, organizational, and process perspectives in explaining the evolution of antitrust policy.

The author offers an encapsulated version of his core argument in the introductory chapter and then plays out the various strands in subsequent chapters, rarely losing command of that argument, which is built on the premise that "to understand changes in antitrust, it is essential to examine the way in which the institutions of antitrust have evolved" (p. 15). The engine of policy transformation after World War II has been the "status and organizational presence of economic expertise" (p. 15).

As the author explains, antitrust policy, as embodied in the Sherman Act, was originally ambiguous. It encompassed a host of social, economic, and political goals and was animated by "a liberal apprehension with respect to growing concentration of economic power and its potential effects on local economies and individual autonomy" (p. 228). The ambiguity, reflecting a lack of consensus about what beyond the mere existence of trusts ought to be the object of public policy, placed a substantial burden on the institutions responsible for enforcement—the attorney general and the courts, soon joined by the Federal Trade Commission. Underdeveloped as organizations, the antitrust divisions of the Justice Department and the Federal Trade Commission were largely at the mercy of the courts and Congress in their effort to give more definition and substance to antitrust. The result of court-centered policy refinement and congressional limits placed on agency activism was policy incoherence, and antitrust remained mired in triviality and irrelevance through the New Deal era.

The author argues that building administrative capacity in public organizations requires a conscious effort at professionalization and expansion of a core of expertise. Agencies successful at such an endeavor realize a greater capacity for planning and policy coordination. The price to be paid for this success is that an agency becomes open to, and interconnected with, the external body of experts and other actors concerned with the agency's policy domain.

When the policy domain is characterized by high levels of complexity and is relatively unpoliticized, a special set of subsystem actors employing a common, specialized set of analytical tools tends to emerge, which the author terms a "community of expertise" (p. 92). A community of expertise is "seldom monolithic and static: there are competing groups within the community promoting different visions of policy and administration" (p. 95). Hence, the community and the agencies are vulnerable to changes in doctrine as different groups and their visions rise to prominence.

This is precisely what happened in antitrust policy. Economics provided the logic for a coherent antitrust policy in both the courts and the antitrust agencies, as well as the structure upon which the agencies could build capacity for planning and coordinated implementation. As long as economic structuralism remained the dominant paradigm, moreover, antitrust policy dominated by economics remained consistent with the ambiguous liberal values at its foundation. Through the 1970s, however, Chicago School doctrine replaced economic structuralism in the antitrust community of expertise. The result was the transformation of antitrust policy in the courts and antitrust agencies (the so-called Reagan revolution in antitrust was merely an extension of this change) and the abandonment of the long-standing and still reasonable concerns about market organization and concentration.

I am most impressed with the author's success in tying the internal organizational dynamics of change to changes in the larger institutional and intellectual context of policy, although I believe he understates the extent to which his own case study evidence shows how political appointees provided effective leadership in organizational change.

I am less certain about his contribution to scholarly understanding of how ideas influence policymaking. He provides a challenging discussion; but given his concern for the historical and institutional context underlying antitrust policy and its transformation, it is troubling that he acknowledges only in passing other institutional

or even systemic changes to which the antitrust changes may be linked; particularly the rise to prominence of economics in policy analysis and policy decision making generally during the 1960s and 1970s. This led to penetrating analyses of the problematic nature of policymaking dependent on expertise by the likes of Henry Aaron. The author will need to consult these in any further research on the role of ideas and expertise in policymaking.

Eisner also misses the chance to point out that almost precisely the same kind of transformation to a new emphasis on economic analysis in the antitrust agencies occurred at the same time in the Environmental Protection Agency, Occupational Safety and Health Administration, and other regulatory agencies. It would be interesting to know how the author would interpret this more general phenomenon.

These relatively minor shortcomings notwithstanding, I admire this book. It provides rich case study material well placed in a larger historic and political–economic context, no small accomplishment.

Clark University BRIAN J. COOK

Order Without Law: How Neighbors Settle Disputes. By Robert C. Ellickson. Cambridge: Harvard University Press, 1991. 302p. $39.95.

Order Without Law brings academic discourses on dispute processing, informal systems of social control, state power, property law, rational choice, and the like to an analysis of property disputes involving traditional cattle ranchers, new "ranchette" owners, and local officials in Northern California's Shasta County. Just north of Butte County and not too far from Paradise, Ellickson studied how ranchers resolve disputes over cattle trespass, fencing, and negligence law. This is a multipurpose project. Ellickson wants to test empirically Ronald Coase's "theorem" about how cattle ranchers behave relative to law when their cattle wander onto someone else's property and thus confront theorem with data. He also wants to formulate a general theory of informal order pulled in part from law-and-society research yet modeled by economics and game theory and thus confront data with theorem. In addition, Ellickson wants to build bridges between law and economics and law and society by importing some of the "realism" of the latter and exporting the rational actor model of the former. The book earns considerable respect for imagining such a project and for moving it forward.

Ellickson assembles a picture of dispute resolution in this community, after studying the geography and history of Shasta County, conducting 73 interviews with ranchers and officials, and combing through state statutes, small claims court records, and California Highway Patrol logs. He tells us, "It turns out, perhaps counterintuitively, . . . [that] legal rules hardly ever influence the settlement of cattle-trespass dispute in Shasta County" (p. 40). When "adjoining landowners there decide to split the costs of boundary fences, they typically reach their solutions in total ignorance of their substantive legal rights" (p. 141), and they achieve cooperative outcomes "by developing and enforcing adaptive norms of neighborliness that trump formal legal entitlements" (p. 4). Even when ranchers turn to the law, in cases where cattle wander onto a highway, "most Shasta County residents badly misperceive the substantive law that applies to road accidents" (p. 141).

Do Ellickson's data square with his conclusions? Is law virtually irrelevant in property disputes among ranchers? Ellickson presents a compelling case *only* if we accept an excessively narrow and fundamentally formal view of law. Law *is* virtually irrelevant if in order to find it at work ranchers must know section 841 of the California statute dealing with the allocation of boundary fence costs. However, contrary to Ellickson's conclusions, his data suggest that ranchers do articulate a general principle of "fence cost allocation" that tends to "follow a norm of proportionality" quite similar to section 841 (p. 71). Yet Ellickson insists there is no direct evidence that the statute contributed to the evolution of the norm; thus, he regards "it as more plausible in this instance that norms had influenced law than vice versa" (p. 75).

Impact studies of the 1960s and 1970s also used formal indicators of law. To the surprise of a few, these studies found that the public did not know Supreme Court cases. Lawyers' law does not capture the dynamics of law as a social relation, nor is it an appropriate litmus test when studying popular legal knowledge. What is most interesting about Ellickson's data is not what it says about a knowledge gap between statute books and the experiences of ranchers but what it says about the interplay between property law expressed in texts, like statutes, and property law enacted everyday. Instead of concluding, as Ellickson does, that ranchers "misperceive" their legal risks when their livestock run loose and that they wrongfully worry about liability and the costs of insurance (pp. 104–5), we might reach the opposite conclusion with a more dynamic concept of law. We might study how insurance company–led tort reform in California shaped ranchers understanding of liability. In this type of analysis, law is embedded in, rather than separated from, cultural and political forces that are at work in constituting the everyday social relations of property.

It is against a narrow and formal conception of law that Ellickson portrays the ranching "community" as a nonhierarchical, close-knit, informal group whose norms produce "welfare-maximizing behavior and cooperation without law" in a setting of shared cultural symbols (e.g., living rooms decorated with portraits of John Wayne and rifles). Again, Ellickson's data tell a different story. Rather than confirm his theoretical assertion—"Informal groups are not hierarchies" (p. 232)—we learn that traditional cow-and-calf ranchers in Shasta County are struggling to determine the meaning of property rights with ranchette owners, who are the new retirees and young migrants from the city raising a few farm animals or doing some agriculture on 5 to 40 acres. Stratification among ranchers is not only evidenced in struggles between different classes of ranchers, but Ellickson's data also describes the allocation of status based on whether someone's "word and deed were most unswervingly committed to being good neighbors" or not (p. 211).

Ellickson's treatment of property holders as an "informal group" functioning outside the law is similarly questionable. Not only does the law give them title to the property on which their entire status is constituted for purposes of this study, but the power of property is central to how this group lives. Again, Ellickson's conclusions aside, his description of how traditional ranch-

ers respond to proposals for closed-range ordinances is evidence of the dialectic between property owners and local officials (country supervisors) at work in the constitution of property relations. Near the end of the book, Ellickson suggests that the arrows may in fact go the other way: "Legal policies themselves influence the vitality of informal systems of social control" (p. 284). Readers will be confused by this shift yet perhaps relieved to think that Ellickson is really less tied to the positivist conception of law and the model of causality he embraces earlier (see p. 158).

Ellickson is more successful when he speaks to law and society scholars about the relevance of a "theory of norms" than he is at interpreting his data with this theory in mind. He is on target when he says that a "key shortcoming of the law-and-society school has been its failure to develop a theory of the content of norms. . . . [They] have tended to treat observed norms as exogenous, rather than as dependent variables whose contents are to be explained" (p. 149). Understanding the relationship between rules and norms is a serious empirical and theoretical problem that has motivated law-and-society scholarship for nearly three decades. Ellickson is sensitive to the problem and develops a comprehensive "taxonomy of systems of social control" that serves as a useful heuristic device. Also key to this problem is understanding the relationship between norms and rules by studying the content of both; that is, through an analysis of the content of social ordering, we can better comprehend how boundaries between law and norms are constructed. In places, Ellickson goes beyond his taxonomy, breaks out of his categories separating law and norms, and suggests that indeed the "more ambitious theory would make the content of constitutive rules endogenous" (p. 136, n. 34). For Ellickson, "constitutive norms" are the "informal rules that help glue an informal group together. . . . In contexts where members might not otherwise recognize each other, constitutive rules governing dress, speech, or etiquette may aid identification" (p. 233). While Ellickson speculates that "constitutive and other norms emerge to provide the glue that makes possible a surprising degree of order without law" (p. 236), some law-and-society scholars are now analyzing how law constitutes those norms in particular settings. Studies of the production of legal ideology in ostensibly "informal" settings like mediation question how boundaries between law and norms are constructed by law itself. Again, Ellickson escapes his own boundaries in places, noting that the "law itself explicitly authorizes self-help in many situations" (p. 144). Nonetheless, because of his adherence to a formal conception of law, Ellickson gives little attention to the interpretive processes that invoke and translate legal rules into everyday legal practices.

To law-and-economics scholars and game theorists alike, this book provides a healthy dose of data on the everyday of cattle ranching. Once again, the fiction of zero transaction costs is dispelled. More important for Ellickson, law-and-economics scholars are exposed as "legal centralists"—believing that people bargain from "any initial set of legal entitlements to achieve an identical, and optimal, allocation of resources" (p. 280). It is, however, ironic that while Ellickson seeks to move beyond legal centralism, and even beyond the study of informalism as "bargaining in the shadow of the law," to negotiating "beyond the shadow of the law," he retains a formalist view (p. 40). He marginalizes discursive approaches that might help explain how constitutive norms are produced and circulated in symbolic orders, such as law, because he is unwilling to stray far from the assumptions of the rational actor model, the model that props up legal centralism.

Despite his self-admitted tendency to "exaggerate" law-and-economics and law-and-society perspectives, Ellickson is to be commended for taking on the daunting task of arguing with both camps, serving as translator for each, and in the end seeking to bring them closer together. This book not only examines the debates, it engenders debate. In this sense, *Order without Law* will play a significant role in future debates on the order of property.

New York University CHRISTINE B. HARRINGTON

Political Ambition: Who Decides To Run for Congress. By Linda L. Fowler and Robert D. McClure. New Haven: Yale University Press, 1990. 247p. $25.00.

What decides congressional elections? Why do incumbents do well in most elections, but not in all? Why have the Democrats been able to win majorities in the House of Representatives for 19 consecutive elections, no matter what the national conditions or trends? These are questions that have been central in political debate in Washington among campaign and congressional professionals and among students of congressional elections. While political scientists for decades focused on macro issues of voter attitudes, economic and issue trends, and the impact of presidential elections on congressional races, the roles of recruitment of individual candidates and of political ambition have increasingly come to be a central focus of research.

Early work on political ambition by Joseph Schlesinger and progressive ambition by David Rohde and insights by Samuel Kernell and Gary Jacobson on candidate recruitment in congressional elections have given us a base to build on and spurred further research. *Political Ambition: Who Decides To Run for Congress* is one of the products, and it is a very good one.

While Jacobson and Kernell analyzed broad patterns of candidate recruitment and quality, Linda Fowler and Robert McClure decided on the case study approach. They looked at ambition and recruitment by focusing in detail on one congressional district, New York's Thirtieth District, which, with Barber Conable's retirement in 1984, became one of the most competitive open districts in the country. They believe that by looking at how individual ambition shapes the choices voters have on their ballots and how local traditions and forces shape that ambition by creating or deterring opportunity, we can learn a great deal about how congressional elections work.

Their look at the Thirtieth District extended through both the 1984 and 1986 election cycles—the election of Conable's Republican successor, Fred Eckert, and his defeat two years later by Democrat Louise Slaughter.

By focusing in depth on one district, Fowler and McClure were able to look at all the *potential* candidates for Congress in both parties, including the unseen candidates who never surface in broader press and public speculation because they decide, often tacitly, not to run. In the Thirtieth District in 1984, one of the most

interesting and surprising features was that an expected donnybrook over Conable's successor never materialized. Of the more than 30 potential candidates mentioned in the press or by party and community leaders, only 6 formally announced an intention to run, including only 3 who had the recognition and resources to be serious contenders.

In other words, for each formal candidate, 10 serious possibilities opted out. One of them was Louise Slaughter, Democratic member of the state assembly and a top-flight contender, who, for a variety of political and personal reasons, despite the lure of the open seat, decided it was not the right time to run. In 1986, even though it meant taking on an incumbent in the age of incumbency, Slaughter came to a different conclusion and won the seat she continues to hold.

Fowler and McClure carry us through the entire political season in Rochester and its environs, looking at all the possible candidates, placing them in the geographic and political contours of the district, examining their psyches and political and social circumstances. They look at the role of the state legislature and its leaders on the ambitions and decisions of the large number of state lawmakers who considered the congressional race; the role of important interest groups; and the roles of gender, personality, timing, and luck.

Choosing a case to study is risky business; the bill you examine to show how a law is made may never be enacted, and the election contest you study may prove to be no contest at all. Indeed, Conable's seat, when it opened, seemed to be a perfectly sound district to analyze but proved to be anything but routine for the authors:

> In 1984 we followed a low-cost, low-key campaign—without even a primary—that met none of our expectations about a race for an open seat, but still managed to convert a safe Republican district into a highly competitive one. Two years later we saw a modern election involving big money and the mass media, in which a liberal, Democratic woman defeated a right-wing, incumbent Republican in a district with a conservative tradition dating back a century. We could not have invented so many unlikely twists and turns, much less expected them. But this extraordinary chain of events served our purposes exceptionally well, because it drove home the basic premise of our book: results on Election Day depend on decisions made by prospective candidates months or even years beforehand.

The unexpected twists and turns of these two campaigns did, fortuitously, serve Fowler and McClure well. More important, they took full advantage of them to make a good book even more interesting and compelling. With vivid writing, a sophisticated understanding of the district and of politics in both parties and real insights into the motives and perspectives of all the personalities involved, Fowler and McClure have written a gem of a book. There may be no theoretical breakthroughs here, but *Political Ambition* makes a genuine and important contribution to the political science literature on Congress and elections. Scholars of these subjects are not the only ones who can enjoy this work. Everybody interested in politics and elections, from reporters and columnists to lawmakers and campaign professionals, would benefit from reading it.

American Enterprise Institute NORMAN ORNSTEIN

Regulatory Choices: A Perspective on Developments in Energy Policy. Edited by Richard J. Gilbert. Berkeley: University of California Press, 1991. 339p. $39.95.

Reinventing Rationality: The Role of Regulatory Analysis in the Federal Bureaucracy. By Thomas O. McGarity. New York: Cambridge University Press, 1991. 384p. $59.50.

These two books focus on the role of economic analysis of regulation. Significantly, neither book is written by a political scientist McGarity being a law professor and Gilbert an economist. McGarity carefully explores the role that analysis itself (particularly, detailed benefit–cost analysis of particular regulations) has played in recent federal regulatory decisions. Gilbert's book applies microeconomic analysis to energy policy in our largest and perhaps most influential state, California. Both books have implications for the political process of regulation. Both are written well and provide scholars interested in regulation with much useful information, although neither can be described as seminal, path-breaking, or widely appealing to broader audiences in political science.

McGarity contrasts benefit–cost analysis with other approaches to the development of regulations, presents case studies, summarizes positive and negative elements of benefit–cost analysis, details several structural models used in federal agencies, and considers review of agency analysis by the president's Office of Management and Budget and by the judicial branch. Using case studies from the Environmental Protection Agency, Occupational Safety and Health Administration, Food and Drug Administration, and National Highway Traffic Safety Administration from the 1980s, McGarity compares what he characterizes as the technological or bureaucratic approach to regulatory decision making to the analytic or rational–comprehensive approach that has been increasingly favored by economists on theoretical grounds and by recent presidents, most prominently Ronald Reagan, because it tends to support their policy agenda of minimizing government intervention into private markets.

McGarity includes substantial detail on regulations dealing with leaded gasoline, airborne particulate matter, mechanically processed meat, driver obstructions in automobiles, and workplace hazards. The bureaucratic and interest groups battles reflected in these stories will be fascinating to those who study regulation but might seem tedious to a more general reader. The book provides the reader with an excellent sense of the increasing importance of regulatory analysis as one input into the politics of governmental choice. The detailed stories provide a strong case for the importance of both bureaucratic and presidential staff actors, in contrast to more narrow congressional dominance theories. If the book included a chapter on congressional review of agency analysis, it could be a valuable text for graduate courses in regulation or policy analysis.

The case studies are more interesting than the many typologies or taxonomies McGarity develops to characterize models of agency structure that bring analysis into the decision-making process. To be of greater use, these models should be tied more closely to existing literatures on public administration generally, the role of the policy analysts in government, and the politics of structure, such as Terry Moe's chapter in John Chubb and Paul Peterson's *Can the Government Govern?* (1989). Of the

many models he considers, McGarity stresses the superiority of a teamwork approach, an adversarial approach, or a hybrid, as compared to hierarchical or outside advisor approaches.

The balanced treatment of the value and limitations of regulatory analysis is very much like Steven Rhoad's discussion of microeconomic analysis of public policy (*The Economist's View of the World*, 1985). The reader's head nods in agreement when McGarity discusses the positive aspects; but when he next presents the limitations, you wonder why you were so enthusiastic about the value of benefit–cost analysis in the previous chapter.

Gilbert's book applies the microeconomists' useful set of tools to energy regulation. As the product of research by Universitywide Energy Research Group of the University of California, the book covers a broad range of energy policy issues, including electricity rate structures, nuclear power plant economics, natural gas deregulation, alternative energy supplies, the role of supply subsidies, and conservation programs. Chapter authors, oddly, are not identified by profession or affiliation; but they seem to be economists or engineers.

Gilbert does a fairly good job tying the different chapters together to form a coherent book, partly through his role as author or coauthor of 6 of the 10 chapters. Like McGarity, the authors include much substantive detail, illustrated well by many charts, tables, and graphs. The economic analysis is not too complicated and is explained fairly clearly (at the level of a typical Brookings Institution policy book); but political scientists without much microeconomic background may find it difficult. The reader who is not specifically interested in energy policy may find the book too detailed. Still, if the reader wants to know the economics of the various American responses to the "energy crises" of the 1970s, from wind power to conservation to nuclear safety concerns, this book has answers.

The extent to which the book reveals anything about the politics of regulation is overstated. The first sentence states, "This book is an attempt to apply an economist's microscope to the process of public utility regulation." This is misleading, since Gilbert and his coauthors do *not* analyze the process in the way that a political scientist would but, rather, analyze whether the results of regulation meet the criterion of economic efficiency. The politics of the process are not addressed (as they are, e.g., in William Gormley's 1983 classic *The Politics of Public Utility Regulation*). When the authors find that regulation does not measure up to economic efficiency standards (as is usually the case), they then offer some general comments about the likely politics of the issue and why efficiency is not the main goal of politicians and regulators; but the political angle is not emphasized at all.

To someone familiar with microeconomic analysis of public policy, the conclusions of most of the chapters will not come as a great surprise. Typically, the public regulatory intervention has distorted market efficiency and achieved whatever specific goal at a very high cost in efficiency terms. The authors do take great care to test whether the policy choices, like the Diablo Canyon nuclear power plant, were appropriate, given reasonable expectations at the time the choices were made about likely future energy price hikes.

While both books focus on economic analysis, McGarity discusses much more how the analysis influences the politics of regulation and, conversely, how the politics influence the analysis. Gilbert's book treats politics more as a constraint on achieving economic efficiency, rather than something to be studied explicitly. The choice of analytic technique is less controversial in Gilbert's book because it involves comparisons of costs using technologies that have revealed themselves over recent years, while McGarity's discussion of benefit–cost analysis inherently involves the larger unknowns of social regulation's long-term benefits and costs.

Regulatory Choices will be a more useful piece of policy analysis for the regulators (the California Public Utility Commission, other state public utility commissions, the Federal Energy Regulatory Commission, and energy analysts)—, while *Reinventing Rationality* will be more useful to political scientists and policy analysts studying the more general phenomenon of bureaucratic use of analytic information. At its best, the McGarity book can be seen as a parallel to Derthick and Quirk's *Politics of Deregulation* (1985), which focused on how ideas shaped "economic" deregulation. McGarity shows how "social" regulation, too, has been influenced by the increasing penetration of analytic ideas, crystallized in the form of benefit–cost analysis, over the last two decades.

State University of New York, Stony Brook PAUL TESKE

Forging the Military–Industrial Complex: World War II's Battle of the Potomac. By Gregory Hooks. Urbana: University of Illinois Press, 1991. 303p. $39.95.

The Rise of the Gunbelt: The Military Remapping of Industrial America. By Ann Markusen, Peter Hall, Scott Campbell, and Sabina Deitrick. New York: Oxford University Press, 1991. 341p. $35.00.

Why did the military industrial complex develop? What are the effects of this development? These questions are the immediate focus of these two books. Each book also contributes to a more general literature. *Forging the Military–Industrial Complex* contributes to the literature on the theory of the American state, *The Rise of the Gunbelt* to that on the theory of industrial location.

Sociologist Gregory Hooks, whose concern is to describe "the domestic influence of the military in economic governance" (p. 15), traces the origins of the military industrial complex to the New Deal:

> Ultimately the most enduring legacy of the New Deal was not that of reform—it was the legacy of state building, the centralization of power within the executive branch, and the state's growing capacity to shape economic activity. These trends were apparent in the institutions that gave direction to the World War II mobilization—the Roosevelt Presidency, Congress, emergency civilian planning agencies, and military bureaucracies. (p. 83)

Against those who have written that New Deal state development fizzled out in the late 1930s, Hooks argues that during the World War II the New Deal state was converted into the national security state. If anything, the state as a whole was strengthened; but the New Dealers lost out to military bureaucrats and corporate leaders. This happened because "the New Deal depended first and foremost on a secure niche in the federal government and the active support of the president" (p. 123). But the war preoccupied the president and nurtured three agencies—the armed services, the

Reconstruction Finance Corporation, and the FBI—that were hostile to the New Deal (p. 123).

Hooks contends that a major policy instrument used in the mobilization to war was state-sponsored industrial investment, which increased from less than 5% of the nation's capital investment in 1940 to 67% between 1940 and 1943 (p. 127). In the aviation industry, for example, "by 1945, the Reconstruction Finance Corporation held title to 71% of the plants and equipment used to manufacture aircraft and aircraft engines. . . . Only 11% . . . was privately financed" (p. 242).

Finally, although the military industrial complex was born and the New Deal died in the mobilization, both changes were consolidated during the reconversion to peacetime production:

> Reconversion enabled monopoly-sector firms in civilian industries to emerge from the war better insulated from statist planning initiatives and capable of exerting sectoral control. Conversely, the potential for civilian planning by the federal bureaucracy declined as a defense-oriented state sector emerged. . . .
>
> In the process of reconversion, the Pentagon acquired the production facilities at the heart of the military–industrial complex and the administrative authority to operate them in peacetime. (pp. 151, 160)

In short, the engine in Hooks's military–industrial complex theory is the enhanced capacity of the military to govern American industry.

The Rise of the Gun Belt sees the major geographic effects of the military–industrial complex as occurring after the war. After showing that military industry is located far from the industrial heartland, Markusen and her colleagues attempt to explain why. As with Hooks, the explanation for the location of defense industries stresses the "shifting national military mission [and] changing service roles" (p. 5) But it also includes "rapidly evolving technology [and] the adaptive capacities of local entrepreneurs in industry, science, and civic boosterism" (pp. 5–6).

The argument is that World War II involved a change in the military mission of the United States from defense of the domestic shores to a global one. Aviators succeeded in convincing policymakers that World War II proved the superiority of air over land and sea forces. The Air Force became separate from the Army and, in interservice competition, won large shares of the postwar military budget (pp. 16–17). Since Air force doctrine favored "going to industry and having industry develop and produce for us" (p. 32, quoting a general) and the Army had been wedded to developing weapons in the in-house arsenal system and contracting with the automobile industry for fabrication, the military–industrial complex was born.

Which areas won? Basically, a few new centers in areas in which older industries were either absent (as in the South and West) or dying (the Northeast). They were located outside the industrial heartland partly to avoid having to deal with unions and a somewhat antimilitary culture, partly because industry in the heartland had civilian customers and sought to avoid Pentagon regulation, and partly because a new technology was required. The new military requirements were for a limited number of technologically advanced weapons whose construction was extremely labor-intensive. Therefore, the big Midwest-based manufacturing companies with unionized labor forces that had bent metal for World War II weapons were replaced by "seedbeds" of small firms in areas to which highly educated technicians could be attracted. These were often in sunny areas near military bases or engineering schools. The decisions of local boosters (mayors, chambers of commerce), entrepreneurs (founding fathers), and generals and colonels also affected such locational decisions. Case studies of Los Angeles, New England, Seattle, Colorado Springs, and Washington, D.C. show that combinations of these factors were involved in the development of military industries in these areas.

Which areas lost? Mainly the Midwest. For example, about Detroit, which had produced aircraft during the war and had been allied with the army's Huntsville missile production team, Markusen and her colleagues say, "What stands out in this story is the difficulty of a truly distinctive, revolutionary technology taking root in a region where another, entirely different one is still robust and commands resource markets and business culture" (p. 68). Much the same was true of Chicago. Thus, "Until the late 1970s, the major midwestern companies, heavily invested in producer goods and highly successful in consumer durables markets, saw no reason to pursue purely defense outlets" (p. 77). Moreover,

> It was not that the midwest failed to innovate, just that innovation is carried out so differently in commercial and in defense markets. Particularly in consumer goods, the product has to be very simple. . . . This produced a management style inimicable to that favored in defense procurement, where tailor-made esoteric equipment is fashioned with little concern for cost and no presumption of long-term mass production. (pp. 77–78)

Both books address the economic effects of this pattern of military–industrial development Hooks sees it as a failed industrial policy—misdirected capital investment resulting in de facto national deindustrialization and economic decline. Markusen and her colleagues agree but also see it as a regional development policy with other pros and cons. It did promote economic equality among the regions and stimulated seedbeds of technological innovation although it also created undifferentiated local economies subject to boom–bust cycles and contributed to the urban problems of the current period, including suburbanization and the residential segregation of the poor in declining inner cities. Markusen and her colleagues also say that by attracting middle class migration toward the coasts, it contributed to the political realignment of the country.

These books make several contributions. First, they go a long way toward moving the apparently narrow subjects of American military and military procurement policy toward the center of the study of American politics and political economy. Second, to those who have focused on cross-sectional studies to test parochialism, ideology, deference to the military, and other hypotheses about military policymaking behavior, they offer developmental theories and carefully and subtly interpreted evidence about the historical context to worry about. Third, they challenge researchers in a variety of subfields (e.g., federalism, urban politics, public administration) to investigate historically how the military industrial developments may have affected, and been affected by, their subject matter. Fourth, they, of course, imply a host of questions about how the winding down of the Cold War will affect interservice competition and regional development. Finally, they challenge comparativists to ask whether in other countries the

same causes (or their absence) account for military–industrial relations with the predicted societal effects.

Gunbelt is written for a more general audience and would be useful in undergraduate courses, especially if it comes out in a less expensive paperback edition. Both books would do well in graduate courses.

University of Illinois, Chicago BARRY RUNDQUIST

The Logic of Delegation: Congressional Parties and the Appropriations Process. By D. Roderick Kiewiet and Mathew D. McCubbins. Chicago: University of Chicago Press, 1991. 286p. $34.00 cloth, $12.95 paper.

Parties and Leaders in the Postreform House. By David W. Rohde. Chicago: University of Chicago Press, 1991. 232p. $39.95 cloth, $13.95 paper.

After more than 40 years of American political scientists' decrying the decline of party in the U.S. Congress, it is heartening to read two books that point to its resurgence and characterize its importance. The books, *The Logic of Delegation* by D. Roderick Kiewiet and Mathew D. McCubbins and *Parties and Leaders in the Postreform House* by David W. Rohde, reach similar conclusions about the importance of parties in Congress (and principally the House). This is all the more interesting in that they treat entirely different subjects. On the one hand, the Kiewiet and McCubbins book is concerned with whether Congress has abdicated its role in overseeing the appropriations process. On the other hand, the Rohde book is propelled by the empirical observation that in recent years, partisanship and party leadership have increasingly left a mark on the Congress. Both books conclude that party plays a crucial role for shaping behavior in Congress and for providing oversight over important components of the policy process.

There is a good deal that these books share. They begin from a common perspective that has its foundation in contemporary formal models of political actors. This is somewhat unusual, since those ordinarily identified with the rational choice school have given party short shrift. Parties have usually been viewed as temporary amalgams of interest or conveniences used by representatives to minimize information costs for their constituents. By and large, however, political parties have had little status for this group for trying to understand Congress.

Both books share an affinity for providing party a role that is more than descriptive and holds theoretical content. They share a view that political parties are crucial instruments for holding together otherwise atomistic actors. For some time congressional scholars have noted how members of Congress have become insulated from one another and how this has transformed representatives into independent agents who act on their own agendas. The usual caricature is the incumbent—an individual aloof from partisan concerns, a specialist over a narrow policy range, and a delegate tied to the fancy of constituents. What is absent in such a view, however, is the important role played by party in Congress for binding together a majority of incumbents. Kiewiet, McCubbins, and Rohde recognize that party is crucial and begin to offer a theoretical justification for why party is important.

While the clearest statement of a theory of party comes from Kiewiet and McCubbins, a similar view is implicit in parts of Rohde's book, as well. Political parties within the institution are crucial for solving three key problems. The first involves solving coordination problems. In the absence of parties (or leadership, as Rohde argues), even groups of individuals with similar interests can find themselves working at cross purposes. Parties and leaders serve as a focal point around which members of Congress can easily coordinate. Second, parties are key for solving collective action problems. These problems stem from its being in the interest of each representative to pursue a particular policy end (e.g., obtaining a new grant for mass transportation in the district). Yet if all pursue conflicting private ends, all are left worse off (the budget is broken). The key problem here is how to ensure that all bind themselves to agreements not to raid the Treasury. Parties again provide a useful means for binding individuals to collectively desired goals. Finally, the collective choice problem must be solved for legislatures dominated by independent agents who are bound to highly variable districts. In such instances, vote cycling is an all too real problem. While, in recent years, institutional structure has been viewed as one way in which impermanent and cyclic majorities can be thwarted, the authors here suggest that parties also play a crucial role. Parties are important for minimizing voting cycles on the floor through agenda control, constraints on rules, and binding members together. While such observations may not seem earth-shattering, developing a theoretical basis for party in the Congress is key. Then to use those theoretical insights to inform empirical scholarship is impressive and represents a progressive shift for scholars.

Both books point out that party is far more than a label or descriptive attribute that should be included when studying Congress. Instead, party must be understood from a theoretical basis. Both books provide a useful start for stimulating increased theorizing about the nature of parties in Congress.

Rohde's *Parties and Leaders in the Postreform House* begins by surveying the large literature decrying the steady decline of partianship in the House of Representatives. At the same time, Rohde notes that during the 1980s there was an abrupt resurgence of party-based voting. This empirical observation, then, frames the book and raises the important question, "What happened?"

First, Rohde explores the counterintuitive idea that congressional (particularly Democratic) reforms in the 1970s had a delayed effect that contributed to increased partianship in the 1980s. The standard view of those reforms is that they led to increasing decentralization in the House and further undermined already weakened parties. Through a careful blend of theoretical argument, institutional interpretation, and empirical analysis, Rohde shows how the Democratic Caucus was transformed into a useful coordinating mechanism in the 1980s. This was accomplished not just by institutional fiat but with a decline in sectional divisions.

Second, Rohde demonstrates that these reforms strengthened party leadership, with leaders' granted tools that helped them in structuring the legislative agenda. Yet leadership was not given carte blanche. While leaders could shape the agenda, they were bound as agents to the caucus. The combination of enhancing the ability of leadership to lead and binding leaders to an

increasingly homogeneous Democratic party contributed to increased partianship.

Rohde is not insensitive to the reaction of Republicans. The 1980s have perhaps best characterized the conflictual nature of divided government, with House Democrats proposing their own agenda to that offered by a Republican President. Rohde is careful to elaborate on the role of Republicans in the House and their response to a more unified majority party. However, the result led to an increasingly frustrated minority cut out of policy formulation. Presidential leadership, he finds, has an interesting effect. While Democrats responded with greater unity in response to Reagan's programs, Carter's own policy agenda tended to fragment the Democratic party.

At the conclusion, Rohde has a very thoughtful, extended discussion of his findings. These are useful in a number of ways. On the one hand, he raises a number of points that question conventional wisdom. In particular, the claim that divided government will inexorably destroy parties does not find much empirical support. On the other hand his speculations give rise to a number of new areas for research. Further exploration of the linkage between electoral responses and the strength of partisanship are certainly in order. So, too, are concern expressed over the nature and strength of leadership.

Kiewiet and McCubbins' *Logic of Delegation* begins by detailing a common view that Congress has abdicated its role of policymaking through delegation. These authors, however, challenge that view, claiming that "it is often the case that desired outcomes can be achieved *only* by delegating authority to others" (p. 3). The received wisdom and Kiewiet and McCubbins' counterclaim are thoroughly subjected to theoretical and empirical scrutiny. The empirical case they detail is the appropriations process. It is an interesting case, since the opportunities for abdication are manifold: the Appropriations Committee has often been portrayed as an independent watchdog of the Treasury, presidential budget making sets important parameters on the appropriations process, and bureaucrats claim specialized information that limits the questioning of their claims. All are sufficient to undermine Congress from generally playing any positive role in the appropriations process.

Kiewiet and McCubbins make an important distinction between abdication and delegation. It is largely accomplished through a theoretical discussion of principal–agent problems. Central to their argument is that political parties are crucial instruments which constrain their agents. To support their point, the authors then dissect a number of institutions in Congress concerned with the appropriations process. Beginning with the role played by party leadership, they then show the power exerted by the party over the Appropriations Committee and committee assignments more generally. Their empirical work certainly undermines the view that the Appropriations Committee acts as a watchdog of the Treasury and clearly points out that both parties have been successful in pursuing their own policy ends. Likewise, their data generally support the point that appointments to Appropriations are not taken lightly and that the parties manage to clone likenesses of themselves on that committee. So, with shifts in party fortunes, we can expect Appropriations to reflect those changes.

After detailing why the Appropriations Committee functions more as a delegate of party than as an independent entity, Kiewiet and McCubbins go on to explore how appropriations bills survive scrutiny on the floor. Certainly, members have every incentive to alter appropriations once such bills come to the floor. Allowing this to happen certainly plays into the hands of those who argue that Congress has abdicated its responsibility. However, Kiewiet and McCubbins contend that party control of Conference Committee appointments provides an ex post veto over amendments. Their theoretical insights here are very useful, and their empirical examples are quite suggestive. Their conclusion seems quite reasonable: the order of the day is delegation, not abdication, since party holds a firm grasp over the final play of the appropriations game. Even when the budget process is removed from Congress, as with the executive and the Office of Management and Budget's initial proposal, Kiewiet and McCubbins demonstrate that congressional parties exert enormous control over what is churned out of Congress.

The most persuasive part of this book lies in chapter 8, where the authors take a careful look at a number of competing theories to see whether party influence stands up. Using some rather elegant econometric estimations of presidential budget requests and actual appropriations, it is quite clear that congressional parties are key to understanding the appropriations process. While the preceding seven chapters are interesting and suggestive, they never cement the argument. Chapter 8, however, ties together the earlier chapters in a very satisfying way.

All in all, the conclusion from this book is quite simple. Congress has not abdicated its responsibility, as too many scholars have argued. Instead, Congress has chosen to delegate authority in particular ways in order to grapple with the contemporary complexity of the appropriations process. While this system of delegation may look rather weak, it is integrally tied to political parties. Kiewiet and McCubbins' argument and findings deserve careful consideration. While the conclusion may seem simple, as they point out, the implications of their argument and findings are quite general and suggestive.

Both *Parties and Leaders in the Postreform House* and *The Logic of Delegation* bear considerable attention from scholars. Both go a long way toward documenting the role of party in Congress. Both rely on important new theoretical considerations about parties in Congress. Both are well-crafted pieces of scholarship. Finally, both will surely pave the way for new research.

Rice University RICK K. WILSON

The Union as It Is: Constitutional Unionism and Sectional Compromise, 1787–1861. By Peter B. Knupfer. Chapel Hill: University of North Carolina Press, 1991. 285p. $29.95.

Too few scholars attempt to draw connections between political culture and politics. It is this attempt that makes Knupfer's book worth reading. The central task of this book is to demonstrate the relationship between political culture and political compromise, or to examine the relationship between civic attitude and constitutional development. While most scholars, especially those focused on legislative relations, look at compromise from the stance of political process, Knupfer presents compromise as a political ideology unto itself.

Instead of being a means to an end, compromise becomes an end unto itself. More than a process of mutual concession, compromise was to be a form of association with distinctive qualities. Compromise is then a metaphor for the union as it was from 1787 to 1861, forming the basis of constitutional theory from the Founders' early vision. By 1861, however, that vision had eroded and was beginning to yield to a new theory.

It is this notion of political compromise transcendent of political process which makes the thesis unique and therefore a contribution to the field. To make his case, Knupfer presents the sectional quarrels of the 1840s and 1850s as a subtext for demonstrating the collapse of compromise as political ideology. The key figure is none other than Henry Clay, the Kentuckian who had served as Speaker of the House. Though Clay is by no means presented as a deep thinker, he is portrayed as a politician who seriously understood the meaning of compromise as the basis for maintaining a constitutional union, which as Madison asserted in *The Federalist* was to be neither wholly federal nor wholly national. Just as Madison, whom Clay admired and worked with, understood that the politics of limited government would flow from a constitutional structure designed to contain power, Clay understood that the politics of compromise arose from the premise that the union was founded on compromise as its own theory; for the most consistent theme of Clay's public life was unionism—fidelity to union. This is what guided his understanding of the constitution and the policies needed to preserve it (p. 123). It is an understanding within the context of a constitutional tradition whose roots are in the early republic. If there is a hero, it is Clay who emerges as the last exponent of compromise.

While Knupfer uses compromise to symbolize the old political order, the rise of political parties, especially the Radical Republicans, comes to symbolize the new order, in which the sectional differences that were tolerated in the past would no longer do. The Compromise of 1850 is then presented as the culmination of a compromise tradition that many had regarded as a successful means of managing sectional conflict: "The political crisis of the 1850s would test a new generation's confidence that parties could save the Union rather than destroy it" (p. 200). Ultimately, then, 1861 is a watershed in American history, since it signifies the death of Madison's union and the birth of Lincoln's.

To this end, Knupfer has succeeded. He has offered us a thesis and supported it through historical scholarship. Nevertheless, some omissions must be noted. While the book begins with an interesting theoretical discussion of the meaning of compromise, one is left to wonder, to what end? What is he trying to show, exactly? If he is trying to add to the general debate as to the Framers' intentions (or original intent, as some might suggest), he nowhere mentions such debate. Knupfer presents compromise as though it was Madison's intention to create a federal system within the context of a liberal framework in which each state could structure itself in accordance with its own political culture. Yet there are those who have suggested that the Framers really had a hidden contempt for federalism, along the lines of the late Martin Diamond. If so, compromise is the result of—not the theory underpinning—the union. It is the process by which the union endures (as Knupfer has correctly noted) and consequently the politics of maintenance. Whether or not the Framers did have contempt for federalism is a debate that may never be resolved, but to omit it does much to undermine the premise that compromise is ideology. Or it may be that compromise was inevitable if the Union was to be at all, in which case compromise could spawn its own culture and brand of politics. That, however, is unclear and remains to be shown.

Nevertheless, the book should be considered for what it is—as attempt to make sense out of a deeply divisive period in American history. And insofar as Knupfer attempts to draw a connection between political culture and politics, he will have made a contribution that is sadly lacking in the political science literature. In this vein, I believe that the strengths of the book do outweigh whatever weaknesses it has. The notion of compromise as political culture and ideology is surely a notion that deserves examination. Insofar as this may add a new dimension to the general debate on the origins of American constitutionalism, the book will have succeeded in making an additional contribution.

Seton Hall University OREN M. LEVIN-WALDMAN

Representation Rights and the Burger Years. By Nancy Maveety. Ann Arbor: University of Michigan Press, 1991. 257p. $34.50.

In this book, Nancy Maveety seeks to accomplish two tasks. First, she argues that in the area of representation rights, "the Burger Court made a unique and distinctive jurisprudential contribution and was not simply an atheoretical interregnum" that followed the Warren Court. Second, she sets forth "the current status of representation law as a result of the Burger Court's work" (p. 221). Demonstrating that the Burger Court undertook the unenviable task of trying to develop a workable case law directed toward the goal of ensuring "fair and effective representation" set forth by the Warren Court in *Reynolds* v. *Sims*, Maveety shows that in so doing it broke away from the simplistic one-person-one-vote standard and propagated what she describes as a "group-balancing approach to representation rights" (p. vii).

By focusing on group-based representation, the Court, Maveety argues, brought constitutional doctrine "into harmony with pluralist politics" (p. 221). The manifestation of this pluralist vision took on several forms, which she orders and explicates in her typology of the diverse groups and images of group representation that the Court recognized. This typology forms the core of her work.

The Burger Court, says Maveety, ultimately recognized five types of representation: geographic (by political subdivisions), economic (e.g., by special district governments), demographic (e.g., racial), partisan (the rights of political parties to police primaries and set membership requirements) and administrative–litigational (access to the courts in order to restrain government agencies). Drawing upon the diverse decisions and opinions in each of the five areas, Maveety demonstrates not only the complexity of the concept of representation but also the Court's difficulties in using the concept as a basis for a coherent case law.

Although the extent to which it can be argued that the Court itself consciously sought to organize its jurispru-

dence along such lines may be a matter for debate, Maveety's organization of the Court's attempts to cut its way through the thicket of representation serves the vital purpose of bringing order to what appears to be a very chaotic province of the law. As she illustrates, the concept of representing or being represented is quite protean. Not all forms of representation are necessarily inclusive of the interests of the entire society, nor are they intended to be. Accordingly, the establishment of organs for the representation of specific types of interests (such as water district rights) may occur within a society despite the fact that the interests and population of the community involved are distinct and separate from the rest of the society. Furthermore, as the Court argued in *Salyer Land Company* v. *Tulare Lake Basin Water District,* the weight of one's representation in such special apparati may be based justifiably on criteria other than a one-person-one-vote standard.

Maveety's bringing a sense of order to an area of jurisprudence rife with internal contradictions is a welcome and desperately needed addition to our understanding of the Court's attempt to develop a jurisprudence of representation. Instead of exposing a particular resolve within the Court's collective mind, however, it seems that her typology portrays a Court being pulled in several different directions.

One gets the impression that there may, in fact, have been less method to the Court's madness than Maveety contends. In order to sustain her assertion that the Burger Court followed a coherent theoretical path in addressing representation issues, Maveety must subdivide her categorizations several times in order to accommodate and reconcile cases and decisions that do not always complement each other. As a result, the typology is somewhat complex.

This is especially evident in her discussion of partisan representation and the rights of political parties. Despite her primary division of the Court's decisions along the lines of the three functions of the political party (organizational, electoral, and governmental) and the twofold "dualistic historical tradition between a hierarchical party tradition and a populist party tradition" (p. 149), Maveety is still compelled to concede that the model is not comprehensive and that "the justices could be accused of giving primacy to their individual views of what constitutes freedom of association in party operations" (p. 184). Although she shows that the Burger Court's decisions regarding partisan representation were evenly balanced between the two traditions (p. 185), one concludes ultimately that her imposition of an order upon the Court's approach serves more as a means of tracking the Court through a thicket than of excavating a coherent jurisprudence.

This, however, is a reflection more on the Burger Court's inability to grapple with the complexity of representation issues than on Maveety's analysis. Her work is a thorough tour de force of the Burger Court's meanderings in an extremely complex area of constitutional law. It provides an ideal benchmark from which future analysts might embark when attempting to assess the status and development of the Court's conceptualization of representation.

Washington and Lee University MARK E. RUSH

The Closing Door: Conservative Policy and Black Opportunity. By Gary Orfield and Carole Ashkinaze. Chicago: University of Chicago Press, 1991. 254p. $22.50.

In the late 1970s articles began to appear in the nation's press touting Atlanta as a black mecca. The city's growing economic dominance of the entire southeastern United States, its pragmatic embrace of the New South ethic, and the rise of black political power had all helped to attract black in-migrants, many of them from the North, and to encourage the emergence of a significant black middle class. If the barriers to opportunity for the black masses were ever really to fall, it certainly should have happened in Atlanta. But as Gary Orfield and Carole Ashkinaze demonstrate in this passionate book, for most blacks the promise of Atlanta was stillborn.

The Closing Door documents in careful and painful detail not only the relative absence but the deterioration of opportunities open to black Atlantans today; and its authors lay the blame on a decade of conservative politics in Washington and the state capital. Along the way, the book offers reflections—indeed, a sort of test—of various elements of the underclass thesis and of various expectations that accompanied the emergence of black political power at the local level.

Using mainly aggregate data from government records in chapters devoted to income distribution, housing, high schools, college access and performance, and job training, Orfield and Ashkinaze show that things have gotten worse. Rent burdens have increased for poor blacks since the 1970s. Black home ownership rates have declined. Schools are more segregated than ever. Black college matriculation rates are lower, and so on. In part, they blame decisions in the Reagan administration not to enforce civil rights and affirmative action rules vigorously and not to push busing, the dispersion of subsidized housing, or fair housing enforcement. But they also blame the more diffuse conservative assumption that with legal discrimination laid to rest, the market would eventually solve the problem of racial inequality: economic growth and, as William J. Wilson argued, tight labor markets would yield enough work for all and incomes would provide access to better housing, better schools, and better colleges. Finally, Orfield and Ashkinaze blame a misplaced faith in the efficacy of black local political power (though why they consider belief in the benefits of minority political achievement part of the conservative credo is not clear).

What the conservative solution fails to take into account, they argue, is that the color line in the real estate system impedes the operations of the rest of the market. In this book, housing location is everything. Residential segregation is the "fundamental underlying feature of urban racial inequality," and "where a family lives is the single most important factor in the quality of its members' lives" (pp. 12, 29). The black underclass, pinned and isolated in the ghetto—and, indeed, the black lower middle class segregated in suburban enclaves—find it almost as difficult to get a good public education, advance to college, and find meaningful employment as if there were a formal color bar in those institutional settings.

Furthermore, the limits of local political power become painfully obvious, especially in a black central city surrounded by white suburbs. Blacks have controlled Atlanta's city hall since the election of 1973. They control

the school district and many city agencies. Black mayors can expand opportunities for blacks in public employment, guarantee a share of city contracts for minority-owned firms, and heighten the sensitivity of public servants, particularly the police, to their minority clientele. But control of city government offers no serious way of gaining access to the suburbs, where employment, educational, and housing opportunities are generated. Nor does city hall have anything to do with education financing or the enforcement of federal civil rights policy.

If neither economic growth nor accession to local office serve to open opportunities, then what is to be done? The authors offer various suggestions, few of them very novel, some of them probably politically impossible, many of them dependent on federal action. A restoration of civil rights enforcement, metropolitan approaches to desegregation of schools and housing, grants for college education, basic skills training, and public employment are among their recommendations. But this is a pessimistic book, and one does not finish it with much sense of possibility.

The book is occasionally repetitious and is full of typographical errors; but these, finally, do not detract from its several important contributions. One of these is the compelling quality of the sheer cumulative impact of the case for declining opportunities, even if much of the material is familiar to students of race relations in the United States. And here and there, amongst the well-established documentation that realtors and lenders discriminate and that schools are segregated, the authors do provide some novel material, such as the account of black Atlanta's trading control of the school system for an end to busing and the data on black access to college.

A second contribution is the book's demonstration that a rising tide does not float all boats. Blacks are worse off in "the city too busy to hate" after a decade of extraordinary economic growth. Market solutions to racial inequality fall short when blacks do not enjoy unimpeded access to the market.

Finally, the book provides a convincing antidote to the charge that the black condition is a product of the failure of the liberal welfare state (as Charles Murray and company would argue). Orfield and Ashkinaze point out that liberal Democrats controlled the national agenda only for a couple of years in the mid-1960s and that after the Great Society, black opportunities widened, and measurable gains were recorded. The deterioration that the authors document sets in under President Reagan's watch and in response to the intellectual agenda of the conservative revolution. It is hard to see in this grim account how it will be undone.

University of Wisconsin, Madison PETER K. EISINGER

The Transformation of the Supreme Court's Agenda: From the New Deal to the Reagan Administration. By Richard L. Pacelle, Jr. Boulder: Westview, 1991. 264p. $39.95.

Deciding to Decide: Agenda Setting in the United States Supreme Court. By H. W. Perry, Jr. Cambridge: Harvard University Press, 1991. 316p. $39.95.

In the last two decades, case selection in appellate courts has been studied with great effectiveness by Larry Baum, Greg Caldeira, and other students of judicial institutions. Since almost all the work on appellate court agendas has appeared as journal articles, however, the two book-length treatments reviewed here are welcome additions to the literature. While both books focus on Supreme Court agendas, the vectors of inquiry are somewhat different. Perry wants to provide a better understanding of how the Supreme Court sets its case agendas and what influences play upon the process. Pacelle's queries have a different accent. Examining 14 policy areas, he asks, What changes have occurred in relative space allocations on the Court's issue agenda over a 56-year period, and what factors have structured the pace and scope of that change?

While Perry focuses on the individual justice as decision maker, Pacelle, following the lead of Thomas Walker, Lee Epstein, and a few others, has characterized his approach as neoinstitutional. This follows from his belief that institutional rules and arrangements "structure the aggregation of individual preferences" (p. 13). Perry suggests, further, that "Cert. votes are primarily not the result of strategic calculations about outcome on the merits" (p. 15). Such calculations are considered "the exception . . . for all of the justices" (p. 276). Again, by contrast, Pacelle sees the "personal values and attitudes [of the justices] and their conception of judicial role" as primary but the impact of neoinstitutional considerations as significant.

In seeking a better understanding of the Court's agenda-building decisions, Perry interviewed 64 of the 161 clerks who served in the 1976–80 terms. He also interviewed five current members of the Court, seven judges of the District of Columbia Circuit Court of Appeals, four solicitors general, four attorneys in the solicitor general's office, and one Court employee. Pacelle, in tracing changes in the Court's issue agenda, uses 7,688 cases decided in the 1933–88 terms.

Perry's volume consists primarily of excerpts from his interviews—information from which he eventually constructs a "process" decision-making model. Other chapters cover such topics as bargaining behavior, internal procedures, and the signals or index of importance to the justices. A useful discussion of jurisdiction is appended.

Pacelle offers an impressive theoretical framework for agenda building and change in his second chapter. Other chapters deal with the transformation of the Court's agenda through relative space allocations to economic, federalism, regulatory, and civil liberty issues. While Pacelle has no concluding chapter, he offers a concluding statement in his last chapter. The book also includes four appendixes, the first three of which define his policy areas, list Supreme Court chief justices by tenure, and catalog Supreme Court cases cited in the book. The fourth is a digest of the important cases used. Both books are extensively footnoted and indexed; and Pacelle also adds a 10-page bibliography.

In spite of Bob Woodward's (1979) *The Brethren* and the circling of the wagons that occurred in the Court subsequently, Perry's work is encouraging. It shows us that the interviewing of Court elites is not impossible and that such interviewing can produce massive amounts of detail in areas in which our knowledge is somewhat general, or perhaps only tentative. Perry's conduct of these interviews also makes the implicit point that best results are obtained when the interviewer "learns" from one interview to the next. Perry has been

quite creative in refusing to stick to preproject interview protocols. He frequently shapes later questions to reflect what he has learned in earlier encounters. He is also adept at asking the same question in several different ways during the same interview, (sometimes, it may be noted, gaining a different response). Equally implicit is the significance of "acceptance" by those who are to be interviewed: through his professional contacts and reputation, Perry gained that acceptance. As a consequence, the degree of openness that characterize many of his interviews is quite surprising.

This openness occasionally produces suggestions that conflict with earlier reports in the literature. For example, on the basis of interviews with four justices, Perry reports that the "chief justice begins discussion of the first case on the discuss list" (p. 43) followed by the other justices in order of seniority. He states further that certiorari votes are usually announced at the time of discussion and quotes one justice as confirming the same order for votes on the merits. This is inconsistent with reports by other political scientists relying (as Perry did) on unattributed justice sources.

In the process of preparing this review, I took the liberty of calling this confusion to the attention of Chief Justice Rehnquist. He has now graciously informed me that at present, the justices vote on argued cases beginning with the chief justice and continuing in descending order of seniority to the most junior justice. The discussion and voting is a combined process. Each justice, in turn, first discusses and then votes. When considering petitions for certiorari, the person who put the case on the discuss list goes first, then the vote to grant or deny is taken in order of seniority. This adds more and different information to the picture from an authoritative, attributed source. It should help clear up the confusion to which Perry has directed our attention.

Pacelle's analysis of 56 Court terms reveals that in general, the space allocated to economic, federalism, and regulatory issues declined. At the same time, that allocated to due process, substantive rights, civil liberty, and equality issues increased. While such empirical findings are certainly important, one should not overlook the theoretical and conceptual richness that characterizes Pacelle's work. His efforts in this realm are quite superior to what we normally see in our journals. His ability to start with a general theory and then break it down into components and sketch their interrelationships and ramifications is starkly evident. And once one begins to think he has exhausted the parameters of a theoretical construct, he peels another layer off the onion and stimulates potentially new insights or an occasional feeling of serendipity, though not strictly accidental.

Pacelle's notion of a "bifurcated agenda" for issues and his institutional focus are departures from much earlier work in this area. By devoting his attention to issues, rather than to the decisions of the justices to grant or deny jurisdiction, he directs inquiry to an area about which we know very little. Here he reminds us of issue fluidity and the fact that the decision of the Court to grant jurisdiction is distinct from a decision to confront and resolve any given question. We know from earlier research that issues may metamorphose even in the relatively short time between the jurisdictional decision and case decision. Thus, in the long run, a metamorphosis in agenda space allocations should cause no great surprise. Previous research on the behavior of justices over time has shown that behavior to be dynamic and, in several instances, parabolic. By contrast, Pacelle finds change in issue space allocations to be generally linear. Pacelle's choice of a longitudinal analysis over a static or cross-sectional cut reminds us again that with the exception of Burton Atkins and a few others, we have given insufficient attention to the dynamic processes that characterize the Court and the behavior of its justices.

Pacelle's explanation for the linear patterns he discovers on issue agendas is not extremely radical if stated without adornment. It says simply that institutions—the Court included—reflect their environment. But by conceptualizing the Court as a political entity and suggesting certain changes in the environment as a significant influence on agenda space allocations, he takes a tack that is pregnant with opportunity. Concomitantly, he has left it to others to corral and impose measurement on relevant environmental variables and to determine systematically whether his theoretical notions will bear pineapples or cockleburs.

Finally, it may be noted that the epistemological foundations of these two studies appear to be somewhat disparate. Pacelle appears to accept the very associational or variance models of analysis that Perry finds inadequate and incomplete. Whether the more direct approach of interviewing Court elites is better is certainly subject to question. Almost all the accepted generalizations that characterize our knowledge pool in this area have been produced via the use of quantitative data and some form of statistical analysis. Recognizing that his population of Court elites is not a scientific sample, Perry says, "Much of this book, without apology, is descriptive" (p. 17). But, of course, no apology is necessary, since he offers the book for what it is—a highly qualified and carefully written book—rather than for what one might wish it to be.

Nevertheless, Perry's use of descriptive data permits one to pose a rhetorical question suggested by the comments of two of the justices he questioned. In interviewing his elites, Perry found that the justices were more willing to discuss bargaining and strategic behavior than were the clerks, many of whom, apparently, went into shock when those terms were mentioned. Thus, one of the justices, denying that Warren gained unanimity in *Brown* by force of brute leadership skills, reported that it resulted from a trade of "all deliberate speed" for one or more votes (p. 148). Another said, "I think any one who suggests that this is an objective institution is just wrong; the notion that we are objective is just fallacious" (p. 201). It may be suggested that such a justice would have no qualms about bargaining or strategizing to gain his subjective ends or, at least, that the Court would have no such reservations. This raises the question: Whereas Perry interviewed 64 clerks and 5 justices, would his book have been the same had he interviewed 64 justices and 5 clerks?"

In the final analysis, Perry does not claim to have delivered on his promise to provide a better understanding of how the Court sets its case agendas. He concludes, indeed, that the matter is more complex than he realized and that a broader theory is needed. Yet at bottom, the book has a number of attractive qualities and quite pronounced "discovery" values. It is chockfull of comments by clerks and justices that beg for systematic conceptualization and testing with hard data, if such can be obtained.

To the extent that a book makes a significant contribution to the study of a particular subject, it does so by bringing something new, different, novel, or innovative to a relevant intellectual arena. Another standard is whether the book stimulates one to think in new ways about familiar topics. By either measure, I would give high marks to both the books reviewed here.

University of Kentucky S. SIDNEY ULMER

The Case for Justice: Strengthening Decision Making and Policy in Public Administration. By Gerald M. Pops and Thomas J. Pavlak. San Francisco: Jossey–Bass, 1991. 199p. $26.95.

The authors argue that public administrators understand and implement justice, grounded in morality and practical application, just as implementation of justice strengthens bureaucratic decision making and government policy. Justice, according to the authors' interpretation, incorporates serving the public interest, achieving social equity, and striving for efficiency.

The contribution of this book to existing scholarship is that Pops and Pavlak raise a much-needed agenda of justice in public bureaucracy. This book, for the most part, is descriptive and is not analytical of public administration literature and its varied particulars. The authors never define *justice*, nor do they establish a paradigm for implementing justice in public bureaucracies. The authors offer a paradigm of *administrative injustice* but fail to qualify and/or quantify it.

An antecedent of this work is Frank Marini's *Toward A New Public Administration* (1971). The authors, however, make no attempt to comment on the variety of issues developed in the Marini volume or other relevant literature. For example, the absence of pertinent literature (e.g., Theodore Lowi's interest group liberalism, Arthur Okun's equality and efficiency, Harold Seidman's politics of federal organization, Herbert Simon's decision-making formula, and Aaron Wildavsky's partisan, policy, and systems politics) makes this research quite incomplete. Without references to such literature, any discussion of justice in public bureaucracy is out of context.

This treatment of justice in public bureaucracy is rather esoteric. One presumes, however, that abstractions of justice, by definition, are only intended for, or comprehended by, a relatively narrow audience. In order to attract a more diverse audience, the conceptual material should be interpersed with illustrations and/or case studies.

Despite these flaws, *The Case for Justice* raises an important agenda for partisan–policy–systems politics. The authors do stimulate thinking about administrative justice issues. They argue that a commitment to justice may fill the "legitimacy gap" confronting public administration, stating that justice underlies our constitutional system, that bureaucrats must accept personal responsibility for their discretionary actions, and that advocacy of administrative justice promotes acceptance of change.

Pops and Pavlak examine administrative justice as administrative law with a focus upon the roles of administrative law judges and the Administrative Procedure Act. The authors state that such judges "have shifted increasingly away from rulemaking and an administrative perspective toward adjudication and a judicial perspective [and] have become politically active in representing and promoting their own interests" (pp. 42–43). Meanwhile, the Administrative Procedure Act has helped "to legitimate the role of public administration in American government" (p. 45).

As decidedly more descriptive than analytical, *The Case for Justice* lacks a definitive and consistent methodology. Pops and Pavlak review an array of literature, but they provide virtually no interpretative analysis of illustrations or case studies. Qualities of the just administrator include knowledge and cognitive skills, attitudes, and personal attributes.

The authors elaborate on the multiple justice roles of public administrators. Roles include adjudicator, policymaker, manager, supervisor of professionals, advocate, public role model, and consumer of goods and services. However, Pops and Pavlak do not illustrate or apply these concepts and roles to particular behaviors of public administrators or case studies.

The most analytical—and valuable—aspects of this text explore types of administrative injustice with pertinent examples. Sources and causal agents of administrative injustice are external and internal, individual and systemic. Type 1 injustices originate from external sources and individualistic causation (e.g., unjust laws); type 2 injustices take origin from external sources and systemic causation (underfunding); type 3 injustices derive from internal sources and individualistic causation (unethical acts of administrators); type 4 injustices result from internal sources and systemic causation (work-flow problems).

Pops and Pavlak end their case for justice on an optimistic note. The crisis of legitimacy in public administration, unethical and illegal conduct by civil servants, and the necessity for expertise, technologies, and resources in public bureaucracies may reform and refocus citizen interest in administrative justice and lead to a more just public sector.

The case for applying justice, for moral and practical reasons, in all public sector activities is a pressing one. Although Pops and Pavlak raise an important agenda, they do not argue very effectively how justice will strengthen decision making and policy in public administration.

Ball State University JOHN E. ROUSE, JR.

The Hollow Hope: Can Courts Bring About Social Change? By Gerald N. Rosenberg. Chicago: University of Chicago Press, 1991. 425p. $29.95.

No one who is seriously concerned about social reform and the role of the courts in the American system can afford to ignore the lessons of this important book. Over the last three decades, *Brown* v. *Board of Education* and *Roe* v. *Wade* have stood at the center of virtually all of our theories, normative and empirical, about the role of the Supreme Court. Social reform litigation more generally has been seen as the hallmark of the modern Court, serving as a lighting rod for both praise and critique. *The Hollow Hope* examines much of the significant social reform litigation of the last three decades, giving particular attention to *Brown* and *Roe*. It persuasively demonstrates that for the most part, the Court has been ineffective in producing "policy change with nationwide impact" (p. 4). The author argues that as a

result, groups seeking significant social reform foolishly squander their resources by turning to litigation and, by implication, that in spinning arguments both for and against a Court that serves the interests of political minorities, we have foolishly assumed that the Court has an important independent role in shaping social reform. If Rosenberg is right, and it is very difficult to leave the text unpersuaded, one has to question why scholars, politicians, and reformers attribute as much influence to the Court as they do.

Rosenberg begins with the question, "When and under what conditions will U.S. courts be effective producers of significant social reform?" (p. 9). He juxtaposes two prototypical views of the role of the Court in producing social change, extracting from them an analytical framework for systematically understanding when and why the Court can succeed in producing social reform. Specifically, Rosenberg posits three familiar constraints on the Court that impede the success of social reform litigation: (1) the limited nature of constitutional rights, (2) the lack of judicial independence, and (3) the judiciary's lack of powers of implementation. These constraints can only be overcome when three corresponding conditions are met: (1) ample legal precedent for change, (2) support for change from substantial numbers in Congress and from the executive, and (3) support from some citizens (or at least low levels of opposition from all citizens), with one of the following: (a) incentives for compliance, (b) costs to noncompliance, (c) implementation left to the private market, or (d) individuals crucial for implementation seeking either political leverage or cover (pp. 35–36).

Rosenberg compiles a formidable array of empirical evidence to assess both the direct and indirect effects of social reform litigation. For example, in assessing the influence of *Brown*, Rosenberg not only demonstrates that by and large integration of the schools occurred as a result of congressional and executive branch action, not Court action, he also demonstrates that it was *not* the Court's decision in *Brown* that placed civil rights on the political agenda, precipitated congressional and executive branch action on civil rights, favorably changed the views of white Americans, or served as a stimulus for the Civil Rights movement of the late 1950s and early 1960s. The same thoroughgoing analysis is applied to *Roe* v. *Wade* and various other women's rights cases. A brief overview of how the constraints-and-conditions model can be used to explain the successes and failures of environmental, reapportionment, and criminal procedure litigation is presented in the final section of the book. Rosenberg concludes that "given the constraints and conditions, . . . U.S. courts can *almost never* be effective producers of significant social reform" (p. 338).

The power of this book comes, in part, from the ease with which we can accept Rosenberg's specification of constraints and conditions; they have been drawn from a rich heritage of public law scholarship. What is new is the way in which he synthesizes the common wisdom into a coherent analytical model supported by an impressive compilation of empirical evidence. While every reader will undoubtedly have a quibble with some specific piece of empirical evidence relied on, the totality of the case Rosenberg makes is overwhelming and compelling. This is not a polemic; it is first-rate scholarship.

The combination of Rosenberg's specification of constraints and conditions and his empirical attempts to disentangle the effect of the Court from the effects of the other branches sometimes creates the appearance of a heads-I-win-tails-you-lose scenario. Political support is one of the three conditions necessary for Court-ordered social reform to be effective; in practice, when Rosenberg finds such support, it is sometimes used as evidence that the change would have occurred without the Court's intervention, especially in regards to abortion reform. Given the way the evidence actually plays out, however, this is not as problematic as it may seem at first. Indeed, one of Rosenberg's most intriguing findings is that predecision elite support often evaporates once the Court issues its decision. Similarly, in examining the indirect effects of social reform litigation, Rosenberg intimates several times that while the Court's decisions did not serve to mobilize support for social reform, it did seem to mobilize opposition, although the latter observation is not subjected to the same careful empirical testing that the former is. Repeatedly, the book suggests that not only does social reform litigation unwisely divert group resources away from more productive strategies but that the Court's decisions themselves at times have a counterproductive effect.

As the title intimates, many will find *The Hollow Hope* disheartening (as one suspects the author himself did). They may find some solace in the thought that Rosenberg may be asking too much of the Court by asking that the Court go it alone. His empirical analysis is directed at assessing how much social reform the Court is able to bring about independently; but acting alone, neither Congress nor the executive branch can bring about much social reform, either. Those branches each have their own sets of constraints and conditions. The unasked question is whether the Court, in concert with one of the two branches can bring about social reform (either directly or indirectly) without the support of the other—or in concert with the states, without either of the branches. While it is clear that social reformers who rely solely on the Court for significant reform are bound to fail, that does not necessarily mean that they will not find it useful to employ litigation strategies in conjunction with explicitly political strategies, something few of the reformers involved in the litigation Rosenberg examines attempted to do. Further solace may be gained from remembering that Rosenberg's analysis is limited to "litigation attempting to change the functioning of a whole set of bureaucracies or institutions nationwide" (p. 4). Such aberrational litigation is far from being the whole of the Court's participation in the process of policy development.

This is a tremendously important book. In profound and persuasive ways. it challenges many of the assumptions that undergird the Supreme Court scholarship of the last three decades. *The Hollow Hope* will fundamentally reshape how we see the courts and what questions we ask about them.

Rutgers University SUSAN E. LAWRENCE

The American Political Nation, 1838–1893. By Joel H. Silbey. Stanford: Stanford University Press, 1991. 348p. $35.00.

Imagine a society in which parties dominate the political system. They select the candidates, articulate distinct ideologies, oversee the conduct of elections, virtu-

ally monopolize political communications, mobilize voters, organize legislatures, structure roll-call votes, and (more generally) determine the shape of public policy. Perceptibly connected to the daily experiences of most citizens, these entities function, in other words, as political science texts claim parties should.

For those of us familiar with the parties of the 1990s, imagining such a political order strains our creative capacities. Yet only about a century ago, the United States was such a society. And Joel Silbey's book brilliantly captures and recreates that political world.

The American Political Nation, 1838–1893 synthesizes an incredibly diverse array of materials, primary as well as secondary. In the process, it offers an analysis of the genesis of parties as organizations; of their slow, sometimes halting, but eventual acceptance by the mass public; and of the dominant roles they played in campaigns, elections, and governing. There are chapters devoted to each of these topics, as well as treatments of the patterns of popular voting, the ethnic–religious and socioeconomic bases of partisan loyalties, and of those who challenged the reigning partisan norms. Each of these chapters offers a cogently argued, detailed, and richly textured picture of a bygone world in which citizens were avowedly, uncompromisingly, and proudly partisan. It was also a world marked (not coincidentally, Silbey argues) by high rates of turnout on election day, by the enthusiasm of citizens for politics and political activities, and by the capacity of elected officeholders to respond to grassroots impulses.

Silbey's analysis contributes materially to understanding nineteenth-century politics. Along two distinct dimensions it provides a comprehensive picture of that lost world. First, Silbey's view is cross-sectionally comprehensive. It treats all types of political activity—governing, as well as electioneering. Thus, he shows how parties bridged the gap—how voting became related to public policy.

Second, Silbey's view is longitudinally comprehensive. While most studies of nineteenth-century politics focus on what distinguished the second and third electoral–party systems from each other, Silbey, emphasizing what they shared, treats them together as a distinct *political era*. The spread of sectional norms during the 1850s led to the breakup of one major party, the creation of another, shattered electoral coalitions, and culminated in civil war. Yet through it all, "the central reality of partisanly defined and shaped political activities remained" (p. 139).

More generally, Silbey offers the concept of a political era as something of an antidote to the dominant party–electoral system framework. This new perspective, he argues, allows the development of a classification scheme based on political attitudes and actions other than popular voting. The outline of that scheme he offers as a supplement to the critical-election model (pp. 6–8). This aspect of Silbey's argument is less persuasive than most others. To justify his new taxonomy, he construes the party–electoral system framework too narrowly. In Walter Dean Burnham's analysis, for example, it is as broad and inclusive—as capable of dealing with the complex web, or relationships and interactions among voters, officeholders, and political institutions—as the notion of a political era. Moreover, Silbey's rubric really does not seem to designate anything more than others have included under the label *political culture*.

There is another soft spot in the book: too much of its evidence and too many of its examples (especially the great political one-liners) are drawn from the years prior to 1870.

Those who have specialized in looking at shorter time slices and/or smaller geographies, no doubt, may find other reasons to quarrel with some specifics. That is inevitable with syntheses of this sort. But the fact that this book will raise questions, evoke intellectual disputes, and stimulate new research is simply another sign of its significance and importance.

Northern Illinois University PAUL KLEPPNER

Eisenhower and the Management of Prosperity. By John W. Sloan. Lawrence: University of Kansas Press, 1991. 192p. $25.00.

Was the real Dwight David Eisenhower the complacent, reluctant president described by intellectuals in the 1950s or the savvy "hidden hand" president portrayed by revisionists in recent years? The truth is somewhere in between, concludes John Sloan in his analysis of economic policy making in the Eisenhower administration. Far from complacent, Eisenhower emerges in Sloan's book as a conservative activist, deeply involved in domestic policy. Yet contrary to the revisionists, Sloan argues that Eisenhower's political effectiveness dropped precipitously during his second term, when he was less likely to employ hidden hand tactics or effectively balance righteous zeal with political finesse. And despite the revitalization of his image as a leader, presidential aspirants still do not evoke the memory of Eisenhower as they have those of Roosevelt, Truman, and Kennedy.

Drawing upon the resources at the Eisenhower Library in Abilene, Kansas, as well as a wealth of secondary sources, Sloan sought to fill a gap in the revisionist literature concerning Eisenhower's economic policy. He adopts Graham Allison's bureaucratic politics approach to analyze how economic policies were a product of accommodation among various actors, their interests, and their relative influence. The bureaucratic politics approach is more commonly applied to foreign policy decision making; but it is quite helpful in analyzing various economic decisions during the Eisenhower administration. Because of the more formal, hierarchical organization of the administration, the bureaucratic politics approach probably works better for the Eisenhower White House than it would for other presidencies.

Sloan sets the stage for his analysis with a review of the economic philosophy of the administration, the key players involved, and the president's economic advisory system. Brief biographical profiles of advisors such as George Humphrey and Arthur Burns and key decision makers such as William McChesney Martin are offered. The political economy of the 1950s is also considered: the state of the economy, the Cold War, and the partisan context. As frequently occurs with a bureaucratic politics emphasis, however, consideration of external factors is relatively thin. While some of the relevant elements of the political and economic environment are reviewed, the analysis would have benefited from greater breadth and depth of coverage.

Sloan develops evidence to support his assertion that Eisenhower was more of a "conviction politician" than is often assumed, actively pursuing firmly held economic ideas. One of those convictions was to balance the

federal budget, which he accomplished three times during his administration, a remarkable feat viewed from the 1990s. However, Ike and the Republicans paid a price for his obeisance to the balanced budget in the 1958 and 1960 elections. The main thrust of the analysis in the book is contained in separate chapters dealing with the budget, monetary policy and anti-inflationary efforts, and fiscal responses to the recessions of 1953–54 and 1957–58.

One of the more interesting sections of the book is a reevaluation of the battle of the budget for fiscal year 1958. This episode has been enshrined in the presidency literature through Richard Neustadt's widely read *Presidential Power* (1960). Neustadt disparages Eisenhower's amateurish political skills because he allowed Secretary of the Treasury George Humphrey to undercut his budget on the very day it was released. Sloan's analysis of the record reveals that not only was the president aware of Humphrey's proposal for further cuts in spending but actively worked with his advisors to determine exactly how the revised proposal would be presented. After editing Humphrey's original four page memo suggesting further cuts, Eisenhower decided that Humphrey should hold a press conference, duly noting his support for the president's budget message. Unfortunately for Eisenhower, the media coverage focused exclusively on Humphrey's extemporaneous comment about a possible "depression that will curl your hair." Sloan shows that there was much more involved in this episode than is indicated by Neustadt, although Eisenhower's political wisdom in allowing the press conference in the first place remains suspect.

Eisenhower and the Management of Prosperity makes an important contribution to the scholarly literature on the presidency, economic policymaking, and in particular to the study of the Eisenhower era. By focusing on the different aspects of economic policy, he provides a sense of how presidents balance competing objectives and interests, use their advisory systems, and respond to external developments. Because the administration portrait presented in the book is necessarily incomplete, this work is most likely to be useful in conjunction with other recent works, particularly Greenstein's *The Hidden Hand Presidency* (1982); it provides a policy focus that supplements biographies such as Steven Ambrose's *Eisenhower: The President* (1984); and it reveals Eisenhower to be less Keynesian in outlook than is suggested by Herbert Stein in *The Fiscal Revolution in America* (1969).

Despite the book's many merits, the contribution to the field is less than it could have been. The book simply is too brief and condensed to do justice to the topic. Much more should have been added about many of the topics covered, both in terms of original data and analysis. The author briefly quotes, but rarely presents, original sources. On many occasions, reproducing the actual notes, memos, or correspondence would have helped make the case much more convincingly and created more of a feel for the Eisenhower White House. The separate chapters treating the budget, monetary policy, and the antirecession measures are not as well integrated in terms of process and outcomes as they could have been. The writing is stiff, sometimes ponderous, and occasionally hard to follow.

While we could have learned more along the way, Sloan's conclusions end the book on a strong note. Eisenhower clearly was not "above politics"; he was more ideological than popularly perceived and occasionally dogmatic about issues such as a balanced budget. He turned away from the more liberal aspects of "modern Republicanism" and towards fiscal orthodoxy as his term progressed. Despite his relative success in the management of prosperity during the 1950s, both he and his critics were dissatisfied by the end of his term. As Sloan concludes, "By the end of the 1950s, Eisenhower's presidency seemed to lack the required vision, and he left office with feelings of rejection" (p. 162). Despite some shortcomings, John Sloan's book does a good job in showing when and how the president's vision faltered, diminishing his political effectiveness and policy achievements.

University of Missouri, St. Louis LANCE T. LELOUP

The Pro-Choice Movement: Organization and Activism in the Abortion Conflict. By Suzanne Staggenborg. New York: Oxford University Press, 1991. 229p. $27.50.

Following the Supreme Court's 1989 *Webster* v. *Reproductive Health Services* decision, journalists spoke of a rebirth of prochoice activism and renewal of energy for moribund prochoice organizations. This view comported with the conventional scholarly wisdom that prochoice groups languished after *Roe* v. *Wade*, while antiabortion forces mobilized. Suzanne Staggenborg challenges this perspective, arguing that the prochoice movement remained viable in the post-*Roe* era by developing formal organizational structures, hiring professional leaders, and employing a conventional political strategy. Becoming an insider, however, did not stop the movement from continuing grassroots organizing. Institutionalization and ongoing grassroots activism prepared prochoice forces for *Webster*. Furthermore, Staggenborg notes that prochoice mobilization following *Webster* was not unprecedented; the Hyde Amendment of 1976 (limiting use of federal Medicaid monies for abortion) also stimulated organizing.

Staggenborg examines newsletters, correspondence, and other documentary evidence of 13 (6 national and 7 Chicago-based) prochoice organizations. Her analysis includes representatives of the population organizations, single-issue abortion organizations, and women's organizations (e.g., Zero Population Growth, the National Abortion Rights Action League, and the National Organization of Women, respectively). Interviews with 50 activists supplement documentary evidence. *The Pro-Choice Movement* provides the most richly detailed and nuanced narrative of the strategies and tactics of prochoice organizations available. It also is one of the first works written by a scholar (rather than an activist, journalist, or freelance writer) chronicling activities of the prochoice movement. Specialists in abortion politics will appreciate the first two appendixes; one presents a brief history of each organization and the other discusses the location and accessibility of the documents used.

The Pro-Choice Movement is organized chronologically into four sections. Part 1 discusses the origins of the prochoice movement from the early 1960s to 1973. Staggenborg contends that the movement originated from within established institutions, as well as through outsider challenger groups (e.g., the National Organization of Women). Seasoned activists who had worked on reform proposals formed new organizations devoted to

abortion repeal and were later joined by the burgeoning women's liberation and population control movements. Staggenborg's research expands that of Kristin Luker (*Abortion and the Politics of Motherhood*, 1984) and Raymond Tatalovich and Byron W. Daynes (*The Politics of Abortion*, 1981), who view the transition from reform to repeal largely as a response to the nascent women's movement and to a redefinition of abortion as a moral argument (controlled by women), instead of a medical issue (controlled by physicians).

From 1973 to 1976, intense countermobilization kept the movement alive, as did activism within the women's movement. National prochoice leadership became more professionalized, organizational structures more formalized. After *Roe*, support solidified among such inside organizations as the American Civil Liberties Union and Planned Parenthood (groups now synonymous with the prochoice label). Transformation, rather than demobilization, is the appropriate way to classify this period.

Paradoxically, the prochoice movement gained strength between 1977 and 1983 following passage of the Hyde Amendment (the first prolife victory). The threat to abortion rights represented by the passage of Hyde created additional resources that accelerated organizational trends toward professional leaders and formalized structures. Prolife organizations, however, dictated the terms on which the abortion battle would be fought. In response, prochoice groups emphasized grassroots organizing, increased their use of institutionalized tactics (legislative lobbying, party politics), and adopted a single-issue political focus. Prochoice organizations that advocated multiple-issue agendas (notably the Reproductive Rights National Network) met with little success. Unlike other scholars, Staggenborg contends that growing professionalization did not lead to the demise of grassroots organizing.

In 1983 prochoice victories in the Supreme Court and Congress quashed immediate threats to legal abortion. While many prochoice constituents considered the battle over, direct action tactics by prolife activists coupled with antiabortion activities of the Reagan and Bush administrations kept the prochoice movement alive and prepared for *Webster*. While Staggenborg notes that prochoice organizations are employing increasingly sophisticated strategies and tactics that may not require grassroots activists, they still are promoting grassroots efforts. If these organizations remain resource-rich, conflict between professional and grassroots organizing need not occur.

As a political scientist, I find several weaknesses in the book. Staggenborg writes of triumphs and difficulties in organizing the National Abortion Rights Action League of Illinois, yet the legislature remained staunchly antiabortion after *Roe*. What effect did Springfield's hostile climate have on organizing? Staggenborg's findings may have limited generalizability because her cases do not include variations in state political context. Few references to relevant political science scholarship appear; noticeably absent in Jane Mansbridge's *Why We Lost the ERA* (1986). Last, the book's theme is that mobilization begets countermobilization, an analysis that is easily subsumed within Trumanesque pluralism.

Staggenborg's principal theoretical contribution is to the study of social movements. Social movements that survive the "dry" periods of mobilization generally do so by establishing formalized structures and professionalized leadership (or its equivalent). Her first-rate, theoretically informed history of the prochoice movement is a much-needed addition to the abortion politics literature. While not as lucidly written or as powerfully argued as Luker's work, *The Pro-Choice Movement* ought to be read by students of women's politics, abortion politics, interest groups, and social movements.

Furman University GLEN A. HALVA-NEUBAUER

Public Service Liberalism: Telecommunications and Transitions in Public Policy. By Alan Stone. Princeton: Princeton University Press, 1991. 296p. $35.00.

In renascent conservative ideology, government "is the problem, not the solution." Since the 1970s, this message found receptive ears in a populace disillusioned both by government's social programs and the performance of the economy. Celebrating the miracle of Adam Smith's invisible hand, the virtues of free markets, free enterprise, and laissez-faire, policy designs advocating privatization, deregulation, and competition have been formulated by the policymaking communities in the United States and Western Europe.

The revival of conservatism as a theory of public policy is due more to the lack of competing, consensual theories than to its intellectual rigor or empirical validity. Like other ideological movements, it has reconstructed its own idealization of history mixing myths liberally with facts. One of the most cherished myths about the nineteenth century in the United States is that there was no government intervention in economic and social affairs and, consequently, remarkable technological progress and economic growth, enhancing, in turn, social progress. Historical evidence contradicts this myth. Government intervention was extensive in local and state levels, where most economic activities and social initiatives took place.

The economic–political history of the American telephone industry from its inception until 1934 is examined in this interesting book. More than yet another well-documented case study of government intervention, Stone advances the thesis that public service liberalism was responsible for the many impressive achievements in the nation-building stages and that its revival as a theory of public policy, rather than the indiscriminate institutionalization of conservatism, could assist in resolving certain current problems, such as the savings-and-loan institutions and the cable television industry.

According to Stone, the state role in a capitalist society is not simply intervention versus nonintervention, regulation versus laissez-faire. Policy choices are wider and more complex. They involve not only economic criteria (efficiency being the acid test) but social and political values, as well. If the free market cannot satisfy the desired values, government intervention can help to do so. Intervention can be short, long, or permanent (e.g., the regulation of public utilities) depending on the values to be achieved. In fact, even heavily regulated sectors, such as the telephone industry until 1934, can operate efficiently when they are monopolies. At the core of public service liberalism is a "presumption in favor of free competition that can be rebutted by showing that government intervention is necessary to attain social values that the unfettered market may not achieve" (p. 4). For example, primary and secondary education are now considered as necessary for society's

well-being as water distribution, gas for cooking, or electricity. As society modernizes, increasing numbers of services become central to the nation's well-being, presenting opportunities for government–business cooperation. Such cooperation often enables economic efficiency, as well as social progress (as illustrated by the telecommunications industry).

The idea of public service liberalism differs from other forms of government regulation and intervention. When an industry is classified under the heading *public service*, explicit obligations are imposed on it: to serve all who apply and are willing to abide by the reasonable regulations of the firm, to serve adequately, to act impartially to all, and to charge only reasonable rates. These obligations are intended to attain social values in addition to economic efficiency. Conflicts over the practical operationalization of these obligations are inevitable, and the rules covering their application have become more complex and intricate as the volume of cases brought to courts and commissions has increased over time. Furthermore, the research controversy over the extent to which public service decisions tend to favor the regulated industry (the "capture theory") is far from being resolved. To make his theory more tenable, Stone introduces another controversial assumption: "Public decision makers can cooperate in achieving the public welfare not by balancing the demands of interest groups, but by making explicit, reasoned decisions designed to attain predetermined goals" (p. 50).

Public Service Liberalism is a fascinating case study of the history of government–business relations in the telephone industry. The concept is intuitively appealing and more humane than the value of economic efficiency as the exclusive consideration in public policymaking. At present, however, public service liberalism is neither an explanatory, nor a predictive, theory of public policy. More conceptual and empirical research is needed to clarify basic assumptions, contextualize propositions to contemporary structural issues, and explicate testable hypotheses amenable to comparison within and between vital industries.

University of Wisconsin, Milwaukee DAVID NACHMIAS

Mobilizing Interest Groups in America: Patrons, Professions, and Social Movements. By Jack L. Walker. Ann Arbor: University of Michigan Press, 1991. 272p. $39.50 cloth, $14.95 paper.

In January 1990 the profession lost one of its most thoughtful students of American politics, Jack L. Walker, in an automobile accident. At the time he was at the midpoint of a year at the Center for Advanced Studies in the Behavioral Sciences working on a book that was to have been the culmination of a decade-long investigation of interest groups in Washington. An outline of the entire manuscript and several chapters had been drafted. Other significant pieces existed in the form of a distinguished set of papers that had appeared at regular intervals throughout the preceding decade.

A group of present and former students and colleagues (Joel Aberbach, Frank Baumgartner, Thomas Gais, David King, Mark Peterson, and Kim Scheppele) have assembled the pieces and crafted this volume as Walker's legacy to all of us who care about American politics as he did. In the process, they created a book; they have not merely stitched together a set of preexisting papers. Their efforts constitute a tribute to a cherished colleague and friend, a testimony to his ability to inspire students and work with colleagues, and an immense service to the study of American politics.

Based on large-scale mail surveys—conducted in 1980 and 1985—of membership associations active in Washington, *Mobilizing Interest Groups in America* weds systematic data to clear-headed thinking about American politics to probe basic questions about interests groups: Where do they come from? What inducements do they offer supporters in order to stay in business? What influences the choices they make with respect to political strategy? Especially significant are the findings on group mobilization and maintenance. Walker points out the irony in political scientists' adopting the perspective contained in Mancur Olson's path-breaking work and pointing out the barriers to organization just when Washington was witnessing a massive mobilization of new groups into politics. Without losing sight of Olson's fundamental insight that those who wish to start an organization or keep one going must provide some kinds of incentives to supporters, Walker uses multivariate analysis to probe the multiple sources of recent group mobilization. Of particular note is the extent to which contributions from patrons (wealthy individuals, foundations, and the government itself) have facilitated the organization of new groups, especially among those not based in occupational or commercial communities. In addition, in a chapter coauthored by King, Walker presents extensive data about the benefits that group leaders deem important for attracting members to their organizations. The findings not only present a challenge to Olson's by-product theory by demonstrating that according to association leaders, collective goods are important inducements to members but also elaborate the circumstances under which an organization's members are likely to value such nonselective benefits.

Walker also addresses the question of the multiple factors that influence an organization's choice of political strategies. The data in a chapter coauthored by Gais indicate that among other things, groups that operate in what is perceived to be a conflictual policy environment are more likely to be politically active and, in particular, to adopt such insider tactics as seeking influence through established government channels. A chapter coauthored by Scheppele on the litigation strategies pursued by membership associations is particularly subtle. Rather than treating litigation in an undifferentiated manner, Walker presents alternative models to demonstrate the differences among the factors that predict three different forms of legal action: filing amicus briefs, filing suits on behalf of members, and filing class action suits. In addition, he presents an unusually complex picture of the meaning of group litigation for the political process, demonstrating that consistent with the stereotype, the disadvantaged and liberal advocates of public interest causes do go to court to file class action suits but that the groups most likely to engage in litigation are unions and well-heeled organizations in the profit sector whose aims are conservative.

It is difficult to know how to treat the shortcomings in this important book. Presumably, it does not diminish the achievement of those who labored so unselfishly to assert that this is not the same book that Walker would have produced had he lived. Therefore, it is unfair to hold him responsible for failings in a volume that he left

unfinished. Still, it would seem churlish to blame any imperfections on six people who worked quickly under difficult conditions to complete a project they did not originally conceive. But clearly, the unfortunate circumstances under which this book was finally assembled exacted a price. With six surrogate authors standing in for a single intellect, the chapters do not achieve the coherence one might have otherwise expected had Walker been able to complete it himself. Also, although the bibliography appended to the book is extensive, several of the chapters are stingy in citing recently published work that reaches similar conclusions.

Some of the difficulties are more intrinisic to Walker's style and to the project itself. Walker tends, as do so many of us who do quantitative analysis of survey data, to reify the findings. Often the strong assertions in the accompanying prose do not contain sufficient reminders to the reader that what is, in fact, being characterized is a probabilistic relationship of statistical significance based upon indicators that are approximations of the concepts for which they stand. Moreover, although Walker's data are superb for answering certain questions, they are less suited for others. Because he surveyed membership organizations, Walker has no data about the thousands of organizations active in Washington (e.g., corporations, universities, and think tanks) that have no members in the ordinary sense. In addition, the poor return rate from labor unions forced him to omit them from most of the analysis. This means that Walker's conclusions about the aggregate set of organizations attempting to influence Washington politics are much less convincing than his analyses of the internal workings of membership organizations. Furthermore, although Walker makes a seminal contribution to our understanding of group mobilization, he neglects a parallel process. The expansion of the Washington pressure system over the past few decades has resulted not only from the birth of new groups but also from the politicization of existing, but previously apolitical organizations. Their movement into politics has had a substantial impact on the character of organizational activity in Washington.

I wish I could have made these points to Jack himself while the book was still in manuscript and congratulated him personally on his achievement. Like his six comrades who undertook to complete what had already come so far, I shall miss him.

Boston College KAY LEHMAN SCHLOZMAN

Senate Elections and Campaign Intensity. By Mark Westlye. Baltimore: Johns Hopkins University Press, 1991. 265p. $38.50.

The central thesis of this well-written and insightful volume is that elections to the U.S. Senate are a good deal more varied than current wisdom suggests. Against the backdrop of an avalanche of research on elections to the House of Representatives, there has grown a small body of work that describes Senate elections as quite distinct from House contests. The literature presents a view of Senate elections as competitive races with incumbents facing tough policy-oriented challengers compared to House contests in which nearly invisible challengers are ritualistically trounced by popular and well-financed incumbents. Westlye argues that only some Senate contests fit this description.

Senate Elections and Campaign Intensity examines Senate elections over the 1968–84 period to argue that campaign intensity is a fundamentally important variable for differentiating among Senate elections. Westlye groups campaigns as *hard fought* or *low-key* in terms of their campaign intensity, and this distinction becomes the principle organizing concept of his analysis. Campaign intensity is conceptualized as the amount of information available to voters during the election period. There is a potentially troublesome distance between this theoretical definition and the operational measure of campaign intensity, which is based on Congressional Quarterly preelection summaries, augmented (in cases) by actual election returns. Westlye does, however, demonstrate that campaign intensity, as measured, is related to amounts of newspaper coverage in a sample of races.

Hard-fought contests, by Westlye's count, amount to 57% of Senate elections in the 1968–84 period. These generally have more experienced challengers and are the more issue-oriented and ideological battles of the conventional wisdom. The remaining, low-key contests generally have a popular incumbent facing an inexperienced, underfinanced, and ineffective challenger. These elections closely resemble the general view of House elections. The failure of previous research to appreciate the diversity among Senate elections, Westlye argues, stems from researchers' reliance on the National Elections Studies data, which (until the recent special Senate election studies) had too few respondents in the low-key contests. This is because the low-key Senate elections tend to cluster in the less-populated states, which are not well represented in national surveys.

To make his point about the variation in campaign intensity, Westlye arms himself with two important new data sources. One is an impressive collection of state-level surveys from a host of different organizations. Here, he does a fine job of skirting the dangers of data incomparabilities to pull out important comparative generalizations. Second, he draws on content analyses of the full preelection newspaper coverage in a dozen Senate campaigns. His focus on newspapers is well justified in an insightful discussion of the public's sources of news (chapter 3), and his case studies here persuasively demonstrate his point about the considerable variety in campaign intensities across Senate elections. Throughout, there is a skillful blending of his quantitative and case study analyses to reinforce and illuminate his basic argument.

Westlye takes on some important issues of electoral research, and generally does an excellent job of relating these to his organizing *hard fought/low-key* distinction. Ideology and issues, for example, loom as more important factors in hard-fought contests, while partisanship and incumbency are more dominant in low-key elections (chapters 5 and 6). He challenges the idea that state population size is a consistently important variable (chapter 7). Contingency tables are the primary vehicle for presenting his analyses. These make his findings easily accessible to a very wide audience; but in places, regression or probit might have provided stronger evidence for some of his conclusions.

The most sophisticated analysis is saved for the end with the examination of the effects of divisive primaries on general election outcomes. The conclusion is important and persuasive: strong challenges in the primaries

increase incumbent vulnerability. The election model Westlye develops for this particular hypothesis is more powerful and complete than anything that precedes it in the book, and it might have been adapted for more powerful tests of propositions offered earlier in the book. It is curious that his final analysis is not tied back to the central distinction.

Westlye seems to assume that mixing elections that vary by campaign intensity is like comparing apples and oranges. However, whether a campaign is hard fought or low-key (or even somewhere in between) is a function of variables we usually want to assess in congressional elections, not an independent factor that can be easily isolated. Thus, while Westlye properly focuses our attention on the important feature of campaign intensity and how it varies in Senate elections, he has not emphasized its endogeneity as completely as he might have.

The main argument of this book is solid and important. Senate campaigns vary a good deal, and research will be better for taking this into account in these and other subpresidential elections. This is a fine book and a useful addition to the burgeoning literature on Senate elections.

Indiana University GERALD WRIGHT

Coming to Public Judgment: Making Democracy Work in a Complex World. By Daniel Yankelovich. Syracuse: Syracuse University Press, 1991. 290p. $34.95 cloth, $16.95 paper.

In *Coming to Public Judgment,* Daniel Yankelovich begins with a premise with which few would argue: the quality of American public opinion could use substantial improvement. But the book is actually far more interesting than this common thesis might suggest. Yankelovich calls for a more engaged, thoughtful public dialogue; but unlike most who decry the quality of American public opinion, he emphasizes the limits of information in remedying this situation.

His argument revolves around several major concepts. Foremost among these is public judgment, which he describes as "the state of highly developed public opinion that exists once people have engaged an issue, considered it from all sides, understood the choices it leads to, and accepted the full consequences of the choices they make" (p. 6). If this seems a tall order for the public as most political scientists know it, Yankelovich has a set of practical suggestions for bringing the public closer to his ideal.

Coaxing public judgment into existence is seen as an urgently needed remedy for a widening gap between the public and policymakers. As the formal institutions of government have become increasingly democratic in terms of the participatory roles that they extend to average citizens, the public has had less actual influence on political decisions. Yankelovich claims that this problem is of such proportion that the distance between America's elites and its average citizens is as great or greater than the distance separating the aristocracies of prerevolutionary France and Russia from average people in their countries. Although this may strike readers as an overstatement, it seems less so when one comes to understand that he is speaking of attitudes, rather than differences in class or power. He refers to a "form of intellectual snobbery" in which political elites (and academics) denigrate public opinion and fail to respect its capacity for wise decision making (p. 4).

The reason for the public's lack of influence is a phenomenon he dubs *creeping expertism,* that is, the increasing tendency to allow expert opinions to guide policy decisions. This practice is deemed dangerous because expert opinion is qualitatively different from public judgment. Public judgment goes beyond well-informed opinion to incorporate values and ethical aspects of policy decisions, rather than emphasizing their factual, informational components. Creeping expertism is seen as a symptom of the *culture of technical control,* a societal emphasis on specialized knowledge, science, and empiricism that has undermined other modes of knowledge. Although Yankelovich's ideas in this vein are similar to other critiques of empiricism, they are particularly interesting as argued by a person whose livelihood is steeped in numbers.

Yankelovich's most intriguing and controversial claims occur in his discussion of knowledge, opinion, and public judgment as a form of knowledge. Drawing on philosophical and historical writings, he concludes that opinion is what society relies upon in the absence of knowledge; it is an inferior substitute that we make do with in a pinch. The underlying assumption is that if people were given more information, their opinions would come closer to knowledge and thus would be better opinions. But more information is not the answer, in his view, because public judgment is more than simply well-informed public opinion. He advocates moving away from a quality-as-information model for judging opinion and toward a model that values consistency, stability, and the extent to which people have thought through the consequences of their positions.

The book covers the expert side of the power imbalance between experts and the public less thoroughly. "Expert opinion" is depicted as if it were all of one cloth. Yankelovich implies that the objective character of technical expertise should lead all experts to roughly the same conclusions. Differences between mass and expert opinion are noted as if it were possible to identify a singular expert opinion on any given issue and evaluate its distance from the public consensus. This is troubling because it downplays the extent to which experts have become pawns in political battle. As any television news program demonstrates, experts can be found to espouse almost any policy position. The author contends that the imbalance between public and experts results from a cultural emphasis on scientific information, not from a power struggle; but the increasing importance and visibility of experts seems inextricably tied to struggles for political power.

Toward his goal of improving public judgment, Yankelovich provides Ten Rules for Resolution—instructions, essentially for leaders, on how to encourage the development of public judgment. These are useful suggestions, and the goal is clearly a worthy one; but the underlying assumption is that leaders are disinterested, which most leaders clearly are not, nor should they be. Thus, the goal of creating a national consensus becomes one of persuading people to a particular conviction. Although Yankelovich points out that cultivating public judgment has some benefits for leaders, it remains unclear what would motivate them toward this end when they would seem to have far more to gain from manipulating public opinion.

Despite antiempirical undercurrents, Yankelovich ar-

gues that polling can be utilized in the service of democracy when it is used to elicit public judgment, rather than mass opinion. Of particular interest to those concerned with theories of the survey response is his contention that stable opinions can and do exist and that they can be tapped through traditional survey methods. This is an interesting contrast to survey response theories suggesting that the search for pure underlying opinions is a misguided one.

In *Coming to Public Judgment*, Yankelovich provides an insightful commentary on the nature of contemporary public opinion, as well as a normative prescription for improving it. The book should be of great interest to political leaders, students of public opinion, and anyone concerned with the relationship between public opinion and policymakers.

University of Wisconsin, Madison DIANA C. MUTZ

COMPARATIVE POLITICS

Currents of Radicalism: Popular Radicalism, Organized Labour, and Party Politics in Britain, 1850–1914. Edited by Eugenio F. Biagini and Alastair J. Reid. New York: Cambridge University Press, 1991. 305p. $49.50.

The title of this collection of essays does not indicate that in its pages a battle is being waged—a battle over the past and, by implication, over the future of the British labor movement. For both editors and contributors, the history of the British labor movement in the nineteenth century has been needlessly and illegitimately divided into three distinct periods: a period of early radicalism (Chartism); a period of stagnation in which Labor was economistic in its demands and politically beholden to the Liberal party (old unionism and lib.-labism); and a period of the emergence of a genuine, independent labor politics (new unionism and state socialism).

The culprit responsible for this division is what the editors in their introduction call the "Marxist assumption" in the literature on the British labor movement, an assumption tacitly shared even by non-Marxist historians. This "assumption" (*paradigm* would have better captured the editors' intended meaning) tends to evaluate the experience and history of the British labor movement in the nineteenth century according to a priori notions of class struggle and state socialism as yardsticks of "maturity" ("identity" here would have been the better term): the closer the labor movement appears to conform to these yardsticks in its politics and programmatic goals, the more "mature" (genuine) it is deemed to be (p. 3). Against this orthodoxy, the 11 essays in this volume, ranging from political-biographical reassessments to studies of tax and labor law reforms, attempt to cast a wider net over the history of the British labor movement, integrating it in a larger history of British radicalism. Thus, nonconformity, liberalism, and trade unionism are viewed as three continuous "currents" in the history of the British labor movement that present a link to an original British radical tradition, as well as a source of continuous strength and vitality. To the editors and contributors of this volume, the future of the British Labour Party lies squarely in the renunciation of "socialism" and the general acceptance of the "currents of radicalism" presented in the volume.

Despite the high level of historical scholarship displayed, I have my doubts about the link between the essays and the general argument developed by Biagini and Reid in their introduction. To begin with, as the editors themselves realize, the contemporary focus on working-class *experience* (Who were those workers? What did they really do, think, read, etc.?) is significantly indebted to the (neo-)Marxist historians' dissatisfaction with the concepts of class and class struggle as *empirically* meaningful entities (p. 14). Furthermore, the emphasis on a distinctively British radical tradition that was fueled by "notions of rights, justice, 'fairness', independence, dignity and individual freedom" (Thane, p. 269) and that was hostile "to the intrusion of the state" (McWilliam, p. 60) is not that new or revolutionary.

Even more importantly, the essays themselves provide plenty of material with which to contest the positive spin editors and contributors want to give to the thoughts, ideals, and practices that the "Marxist assumption" casts aside as "immature" or "labourist." Thus, Rohan McWilliam, in his fascinating essay on the Tichborne case, sees the irony of a popular mass movement that was concerned with restoring the inheritance to an aristocrat—even if that mass support was based on the popular notion of "fair play" and an "alternative morality" (p. 56). Jon Lawrence, in his equally fascinating case study of Wolverhampton, explains working-class support for Conservatism by "a powerful suspicion of party organization and the party 'wire-puller'" that was channeled into support for "a new type of populist Tory politician" who could claim to be independent from the ruling Liberal apparatus. Yet Lawrence has to admit that this was not a particularly smart move on the part of Wolverhampton's workers, who were basically dazzled by antiestablishment rhetoric (p. 75, 84).

Ultimately, of course, the age-old question of reform or revolution (Whither Labor?) cannot and will not, be settled in a collection of essays. Nevertheless, political scientists interested in the history, ideology, and practice of the labor movement might miss in this volume sophisticated nods to industrial capitalism and system-transcending reforms. In my view, it is certainly not enough to laud Robert Knight's "instinctive libertarianism," as A. J. Reid does, by a simple reference to the contemporary British "practical experience" with "centralised welfare and nationalised industry" (p. 238).

The essays in this collection present a vital contribution to a general revision process under way in the fields of labor movement history and labor movement politics. Bolstered by a useful and sharp introduction, this book should interest both historians and political scientists working in those fields.

Rutgers University F. Peter Wagner

Of Time and Power: Leadership Duration in the Modern World. By Henry S. Bienen and Nicolas van de Walle. Stanford: Stanford University Press, 1991. 215p. $25.00.

The question of political survival—why some political leaders hold power longer than others—has been at the heart of political inquiry at least since Machiavelli penned *The Prince*. Although political survival has been the subject of a considerable amount of empirical investigation, this small but fascinating book is the broadest, most rigorous empirical consideration to date of the varying political life spans of national leaders.

The book is divided into two parts. The first part is a careful consideration of the possible effects of a number of factors on the political life span of individual national political leaders. The second part of the book presents the entire data set of 2,256 national leaders from the 167 countries upon which the study is based.

Using the statistical techniques of survival analysis as developed in demography, Bienen and van de Walle

draw upon two broad sets of variables to estimate political life spans. The first set of variables taps attributes of the leaders and the situations in which they found themselves. Here they recorded, for example, measures of age, temporal position in the leadership queue, whether the leader came to power by constitutional or nonconstitutional means, and whether they had a military or civilian background before coming to power. The second set is directed toward measuring characteristics of the leader's country and include, among other things, prior colonial rule, economic growth, income levels, the magnitude of ethnic conflict, and size of population.

The fundamental finding emerging from this study is simple, important, and intriguing. The best predictor of continued political survival is prior survival. Just as the cold, impersonal statistics of actuarial tables say, those who have survived have the best chance of surviving longer. There are some small wrinkles to this finding across different regions, but the result is generally robust.

Why is this the case? At least two possibilities present themselves. One is that those who manage to survive some minimal undetermined length of time successfully consolidate their power and use it to increase their tenure by suppressing the opposition and/or rewarding their supporters. The rival to this is a sort of Darwinian explanation in which "less clever leaders get winnowed out earlier" (p. 98). Although it is disappointing that Bienen and van de Walle do not pursue these two models of survival further, it is nonetheless encouraging that they are able to establish the basic accuracy of their empirical generalization across a broad spectrum of time and space.

The second part of the book reports in considerable detail the main elements of the data analyzed in the first part of the book. The data they report (understandably more limited than the total data set collected for the entire study) identify the state, its geographic region, the leader, the length of time the leader held office, the year of entry, whether entry/exit was constitutional, whether the leader was a member of the military, and how many separate times the leader was in power.

Although the data are extensive and carefully reported, they are not without their curiosities and problems. It is, for example, curious that although data for many Third World nations (particularly in Latin America) extend well back into the nineteenth century, the data for the developed nations typically begin in the early part of this century. Thus, O'Higgins' tenure in Chile is recorded beginning in 1817; but British prime ministers begin only with Balfour in 1905. No explanation is offered for this, nor is there any speculation on what effect such a gap might have on the findings.

Doubtless, anyone who is familiar with history will find things with which to quibble in the data. For example, is it accurate to regard the reign of Haile Selassie (1930–74) to have been uninterrupted, considering that he was forced into exile in London by the Italian occupation (1936–41)? Or can we consider Stalin as actually being the leader of the Soviet Union as early as 1924? Or how can it be recorded that de Rosas left power constitutionally in Argentina in 1852 when he fled after losing the battle of Caseros to insurgents abetted by both Uruguay and Brazil? There are a few other points on which the data seem questionable, but a data set of this sweep is inevitably open to doubt on the part of those with different information or views.

The limits of the analysis and a few problems with the data should not, however, obscure the fundamental contribution of this book to our understanding of political survival. Moreover, it points the way for further exploration of the data and the testing of a variety of models that may account for the differing fortunes of political leaders.

University of California, Davis RANDOLPH M. SIVERSON

Crossing the Industrial Divide: State, Society, and the Politics of Economic Transformation in Malaysia. By Alasdair Bowie. New York: Columbia University Press, 1991. 222p. $40.00.

Bowie has written an intellectually stimulating case study with interesting conclusions that are relevant to a larger sample of countries. In this book, he sets for himself the task of explaining how and why development strategies change in late-industrializing, ethnically divided, Third World nations. In Malaysia (the case he considers), development strategy has changed from a market-led approach during the country's first decade after independence (1957–69), to a mixed market–regulatory policy during the 1970s, and finally to a state-centered strategy for most of the 1980s.

In seeking an explanation for these changes, Bowie first reviews the literature on industrial policy choice in Europe and finds that extant explanations for policy change have little to offer for an understanding of the Malaysian case. Explanations that look to the material interests of actors (whether individually or aggregated in the form of the usual intermediate associations) cannot explain policy change in cases where the principal actors have in common their communal, not material, interests. These communal interests combine diverse material interests and cross class lines. Structural explanations emphasizing either domestic or international factors are inadequate, since the major policy changes occurred in 1970 and 1980 without any corresponding or precipitating structural changes. Likewise, a political culture framework is found to be inappropriate for explaining change that occurred within one generation in a single country.

With the literature derived from the European experience unable to explain his case, Bowie returns to, and reformulates, a more societal-based explanatory framework. In this effort, he joins other recent graduates of the University of California, Berkeley in providing a needed corrective to the more state-centered approaches to explaining Third World developments. This seems especially appropriate for the study of the rapidly industrializing parts of Southeast Asia, where the state has generally not been as central to industrialization as elsewhere in Asia. Bowie's position is that "changes in Malaysian economic development strategy can best be explained as products of the changing nature of a communal settlement that has prevailed in Malaysia since 1957" (p. 9).

The original settlement, engineered by the British at the time of independence, gave the Malays control of the political system. In exchange, the Malays agreed to accept the ethnic minorities as legitimate residents of Malaysia and not to use their control of the state to

challenge minority economic interests. During this market-led period, the Chinese and foreign-owned corporations flourished.

A market-led development strategy did not, however, advance Malay interests, especially those of the poor. This contributed to the 13 May 1969 communal violence, which, in turn, contributed to the revision of the communal agreement and the emergence of the New Economic Policy of the 1970s. In effect, the result was a mixed market, regulatory, affirmative action program for the Malays.

The next shift in policy in 1980 led to a far more activist, state-led process of heavy industrialization. The economic justifications for this policy shift are acknowledged, but Bowie argues for the importance of political considerations. In essence, the Mahathir government, given its ethnic base, was sufficiently independent of Chinese economic interests to use state power to promote a Malay-controlled set of heavy industries.

This last portion of Bowie's argument is less well substantiated; the evidence of Malay pressure for a more activist government role is less clear than in the earlier, 1969 period of communal violence. Since his explanatory framework looks for causal variables in society, it would have been helpful to see more evidence of how the Malay community influenced their communal leadership. Lacking this kind of supportive evidence, the reader is left to speculate about governmental claims to be advancing communal interests. This becomes more important in the light of scholarship such as that of Jomo Kwame Sundaram, which asserts that a Malay administrative elite has, since the end of British colonialism, used its position in government to transform itself from a "statist petty bourgeoisie into a statist capitalist class accumulating on the basis of control of state power" (*A Question of Class* [1988], 289).

Thus, for the Malaysian specialist, Bowie's book will leave unanswered the question of who are the primary beneficiaries of policy choice. However, Bowie's goal was not to write a political economy of policy outcomes. He wanted to explain policy choice, and his major contribution is to the scholar interested in the comparative political economy of development in the Third World. For this audience, Bowie has written a persuasive—even compelling—argument that we must be more sensitive to societal explanations for policy choice, especially in ethnically divided nations.

Ohio University GARY HAWES

Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America. By Ruth Berins Collier and David Collier. Princeton: Princeton University Press, 1991. 877p. $75.00 cloth, $19.95 paper.

This is a monumental work, a tour de force. It is one of the most important books in the field of Latin American politics in several years. The "Collier thesis" will now have to be confronted by all students of the role of state–labor relations in political development.

The Colliers emphasize the type of labor incorporation as a key to the modern political trajectory of developing nations. That focus is convincing, whether explaining the phenomenon of Juan Perón in Argentina or Salvador Allende in Chile. To make their case, the authors use the comparative historical method to provide excellent coverage of eight countries. Their achievement will be welcomed by historians and sociologists, as well as political scientists. It will make valuable reading for scholars concerned with democracy, dictatorship, corporatism, the state, political parties, or trade unions.

Their central argument is that countries differ significantly as to whether the working class was incorporated by the state or by political parties. Whether labor was admitted to the political arena under government control or through mass mobilization set in motion a distinctive sequence of conflict and accommodation. The initial period of institutionalization and legalization constituted a "critical juncture." It followed a spurt of reform and tended to crystallize certain types of political parties and systems. That period of assimilation was succeeded by predictable political consequences in the short-term "aftermath" and long-term "heritage." The Colliers trace these processes both from the top down and from the bottom up. They do so in the eight Latin American countries with the strongest development of an urban working class. They place those cases in the context of very wide reading on comparative politics and labor movements in other parts of the world.

Their classification of cases is surprising, provocative, and fruitful. The Colliers cleverly juxtapose similar patterns in different countries in different time periods. (For example, for labor incorporation under authoritarian rule, the 1920s in Chile resembled the 1930s in Brazil.) Although bound to be controversial among country specialists, their typology will change how scholars think about the timing, sequence, and direction of alternative paths to development in Latin America. The Colliers establish four categories: (1) state incorporation (Chile and Brazil); (2) electoral mobilization by a traditional party (Uruguay and Colombia); (3) labor populism (Peru and Argentina); and (4) radical populism (Venezuela and Mexico). These four contrasting modes of bringing labor into legitimate politics produced, respectively, (1) multiparty polarizing systems, (2) electoral stability and social conflict, (3) a stalemated party system, and (4) an integrative party system.

These pairings illuminate unusual parallels between political systems. Although Chile possessed much stronger parties and democratic traditions than did Brazil, both countries underwent political polarization and fragmentation that ushered in durable antilabor authoritarian regimes. Uruguay succumbed to military dictatorship longer than did Colombia and suffered less private violence, but both maintained electoral domination by multiclass parties with weak links to labor organizations. Although highly different economically and ethnically, both Argentina and Peru saw their attempts at working-class incorporation result in the banning of the major labor parties and the repeated breakdown of democratic governments. Meanwhile in oil-rich Mexico and Venezuela, although the first was a one-party and the second a two-party system, unions in both cases were tied to centrist parties and became stabilizing influences. By spotlighting these similarities between otherwise contrasting political histories, the Colliers underscore the autonomy of some political factors and the decisiveness of the form of integration of trade unions.

The only drawbacks to this book are the excessive length and repetition, as well as the imprecision of some of the organizing concepts. Particularly fuzzy are the different historical stages and, above all, the notion of

critical juncture. It is difficult for an objective observer to determine when such an inflection occurs, what causes it, how long it lasts, and what it produces. Although the authors make a convincing case that such watersheds, or turning points, profoundly affect political development, they are unable to sharpen its definition much beyond "a period of significant change" (p. 782). Their identifications of critical junctures in each country case are persuasive, but the concept remains slippery and elusive as a traveler across borders in comparative politics.

Quibbles aside, this extraordinary synthesis will stand as a starting point for Latin Americanists for many years. For students of other world areas, it should be a major source of reference and debate. As a bonus, the authors provide an appendix listing all the heads of state since 1900 and a stupendous bibliography of secondary sources. This book is further enhanced by the clarity of the writing and arguments. It will undoubtedly find its way into many libraries and classrooms.

University of California, San Diego PAUL W. DRAKE

The Accidental Proletariat: Workers, Politics, and Crisis in Gorbachev's Russia. By Walter D. Connor. Princeton: Princeton University Press, 1991. 374p. $39.50.

In the political section of a March 1992 issue of the *Independent Newspaper*, a Russian biweekly, the respected labor sociologist, Leonid Gordon, asks a question whose answer is likely to be of central importance in determining the outcome of political reforms now underway in that country: "With tough reform days ahead, whose side will the workers take?" Walter D. Connor's excellent study of the emergence of a Soviet working class and its implications for politics in the former USSR does not try to offer a definitive answer to this question, but it does provide us with the best basis to date for informed speculation.

A key to Connor's thesis can be found in the title of his book. He contends that industrial development under Stalin unintentionally, but inexorably, created a social basis for the emergence of a genuine working-class movement in the USSR. In the post-Stalin period, the hereditization and greater education of workers made possible by industrialization laid the seeds of a class consciousness that began to germinate under Khrushchev and Brezhnev but came to fruition only in the Gorbachev era. According to Connor, the Soviet proletariat had gradually come to realize that their interests were subordinated to those of a new set of bosses, who claimed legitimacy by ostensibly ruling in their name. As they began to acquire consciousness of the division between "us" and "them," opposition along class lines became unavoidable, although it was rarely expressed openly until Gorbachev's leadership. That the Soviet proletariat acquired *class* characteristics, despite every effort by the regime to ensure the opposite, leads Connor to label them "accidental." That they are finding their voices at a time of great economic trouble could seriously complicate the introduction of market-oriented reforms, many of which can easily be portrayed as "antilabor." This is what gives particular significance—even urgency—to Gordon's question and to Connor's book.

The theoretical framework for this work is minimal and employs the four criteria for class consciousness set forth in Michael Mann's *Consciousness and Action among the Western Working Class* (1973). After a historical overview of the Stalin years, Connor examines Soviet labor policies and practices from 1953 to 1988 for evidence that these criteria are becoming apparent. Most of the book is given over to chapters detailing worker career patterns, wage equalization, employment, and control of the workplace. In each case, the reasons for growing worker dissatisfaction are shown; and Connor makes a convincing case that class identity and class opposition were well advanced even before *perestroika* and are only likely to be accelerated by the economic reforms currently under way. Particularly valuable is the extensive documentation of strike activity before 1985, which the author has compiled. The final chapter and an epilogue chronicle events through the spring of 1991.

To say that the theoretical framework is minimal is not to say that the book is ill-informed theoretically. The author is clearly aware of the controversy over state-versus-society-based explanations for political change in the former Soviet Union. Although the weight of the analysis is intended to show that long-term societal transformations are driving contemporary political change, Connor is not unmindful of the particular role played by the state in the Soviet context (p. 6). The state's ability to manipulate the terms of the "social contract" or, when necessary, to employ coercion account for the general absence of a politically active working class prior to 1985.

The information on which this book is based comes not from extensive field research (a brief visit to Moscow in early 1988 is noted) but from a magisterial review of Russian- and English-language source materials. As a result, the picture we get of the Soviet blue-collar worker lacks the feel of the shop floor, although Connor makes good use of anecdotal material from published sources. In its place is a surfeit of descriptive and statistical information—meticulously noted, but often leavened only by the author's felicitous writing style. This is not a book for the casual reader.

I find Connor's central thesis persuasive and of critical importance for anyone who wants to understand Russian politics today. One may differ, however, with Connor's emphasis on what might be called blue-collar conservatism. He pays considerable attention to the right-wing United Worker's Front and to the attempt of the older trade unions to create a new constituency among the workers by appealing to antimarket populist sentiments. He also finds relations between workers and intellectual reformers characterized chiefly by mutual antipathy (pp. 227–29). Much less space is accorded to proreform worker movements. If, as Connor frequently points out, most of the voting-age population are workers and if there is, indeed, hostility among workers toward reformers who favor market solutions, why did the UFT, and conservatives generally, not do better in the elections of 1990? And why did Yeltsin win 54% of the presidential vote in 1991?

The article from which Gordon's question is quoted portrays a more differentiated proletariat than does Connor's book—a proletariat in which support for democratic market reform is at least as strong as opposition to it. This, of course, does not contradict Connor's main thesis of an emergent and politically relevant working class. But if Gordon is right, our assessment of how that

working class will affect Russian politics would be more hopeful.

Villanova University JEFFREY W. HAHN

The Invisible State: The Formation of the Australian State, 1788–1901. By Alastair Davidson. New York: Cambridge University Press, 1991. 329p. $65.00.

This account of Australian state building is likely to be controversial. Alastair Davidson uses conceptual tools forged in the thought of Antonio Gramsci and Michel Foucault, among others, to move beyond a more traditional descriptive approach to political history. He focuses on the production of citizens in Australia and how this particular citizenry shaped the political institutions that ultimately emerged. In a striking conclusion, Davidson argues that the result was and is an Australian state that lacks popular sovereignty and consequently is not a democracy.

His chronological presentation begins with the conditions in England leading to the use of Australia as a penal colony. Two groups not usually considered central to state building are highlighted: the criminal class that grew up around the perimeter of London (to which most of the transportees belonged) and the theorists and administrators of punishment, who were responding to the growing problem of crime. It was from the descendants of these two groups—the convicts and their jailers—that the raw material for Australian citizens would be drawn. Consequently, the particular cultural and political values of these groups became the British legacy shaping Australian political development. The values that went across the Indian and Pacific oceans to Australia were not the same as the fear of strong government and commitment to personal rights that came across the Atlantic to the United States. This is at the root of why Americans revolted and insisted on popular control of government, while Australians, ultimately, did not.

Given these beginnings, it is not surprising that the process of colonization and expansion was unique. In a community where control had traditionally been the first concern, the freedom implied by a frontier posed problematic issues of personal responsibility, property, and even family structure. Davidson describes how these issues became administrative concerns, resulting in the authoritative construction of everything from work habits to marital relations. Against this background, various segments of Australian society vied for power as political institutions evolved during the nineteenth century. The result was that the inherently hegemonic place of the law in a prison was transformed into a traditionally hegemonic place for the people involved in the judicial system in the postprison community. Lawyers emerged as the dominant group in Australian public life; and in a series of conflicts leading up to the Constitution of 1901, the relative place of political and legal authority was established, effectively carving out a space for political activity while maintaining the dominance of the law. Even in the central political document establishing the state, the long arm of the law was institutionalized; for leaving critical issues of authority vague left ultimate sovereignty to the courts.

Davidson has made an important contribution to the state-building literature by using a general theoretical perspective to explore a unique historical case. Indeed, it inspires the kind of inconsistent criticism that only surfaces when a book is really worth reading. On one hand, the sheer volume of detail can be overwhelming, so that the multiple strands of the description do not always link up easily to the major thread of the argument. At the same time, it is impossible not to wish for more, especially more comparisons with other countries and more contrasts with related literature. The obvious comparison is with the United States, which also had a frontier and ended up with judicial review. Bringing wider experiences and theoretical concerns into the discussion could be valuable in many ways, however, for Davidson's work falls within the purview of some very significant efforts by other scholars. For example, consider the interesting questions that would be provoked by trying to situate the Australian experience into the framework used by Barrington Moore in *Social Origins of Dictatorship and Democracy* (1966) or by examining the early authority structure and relation of citizens to political organization in Australia and looking for parallel insights in the work done by Perry Anderson in *Lineages of the Absolutist State* (1975) and Reinhard Bendix in *Nation-building and Citizenship* (1964). Comparison with a nonrevolutionary change in state structure during the same time period like that described by Stephen Skowroneck in *Building A New American State* (1982) could address issues of continuity in the administrative side of state development.

These are, of course, unfair expectations; but it is important that this work not be classified as simply a history of Australia. The wider significance of Davidson's achievement is the way in which he supplies a new dimension to the discussion of fundamental issues such as political development, state formation, sovereignty, and democracy. These are complex and difficult issues; but when history and political science can be brought together to mine generally significant insights from unique cases like the development of the state in Australia, real progress can occur.

Pennsylvania State University JAMES L. CURTIS

The Japanese Voter. By Scott C. Flanagan, Shinsaku Kohei, Ichiro Miyake, Bradley M. Richardson, and Joji Watanuki. New Haven: Yale University Press, 1991. 497p. $45.00.

This book sets out to bring the study of Japan into the mainstream of cross-national and comparative research on voting behavior. Rather than treating Japan as the unique case of non-Western industrial democracy, the authors apply theories derived from Western Europe and North America to the Japanese case. It accomplishes, however, far more than being merely "Voting Behavior: The Japanese Case." In addition to applying European-style sociological models and American-style psychological models, the authors develop a *social network model* that owes its heritage primarily to Japanese studies. Though derived from Japanese studies, the social network model is also a comparative model, supported by evidence from Europe and North America and meant to be applied to voting behavior in any industrial democracy. Thus, the authors both apply Western models to Japan and also challenge Western researchers with a "Japanese" model.

The volume contains a trove of valuable insights into Japanese voting behavior, many of which also raise questions about Western voting behavior. Perhaps the most surprising findings concern family versus adult socialization. Miyake finds that "when parents' partisanship conflicts with one's group partisanship, the latter tends to prevail" (p. 210). Miyake's exploration of partisanship in Japan is also stimulating and informative on several levels. Less surprising are Kohei, Miyake, and Watanuki's findings on the effect of issues on the vote, concluding, "If over a quarter of the voters have no positions on the issues, over half are unable to identify any party that represents their views, and over three-quarters fail to link their issue preferences with the party they voted for, then it seems unlikely that a study of issues will contribute greatly to an explanation of voting outcomes" (p. 284). Watanuki's treatment of social structure and voting behavior provides an excellent analysis of the development of the postwar cleavage structure. Flanagan's final chapter uses the theories developed in the volume to analyze the most recent electoral results and to think about the future of Japanese politics.

The two most exciting chapters deal directly with the social network model. Although we do not, in the end, arrive at a coherent new model of Japanese voting behavior, we do make significant progress in that direction. The literature on Japanese politics contains many studies of social networks from the candidate's point of view. The picture we get from these studies is of a tightly knit, candidate-centered voting bloc (*jiban*) that effectively insulates the candidate from electoral tides. These two chapters give us the first systematic look at social networks from the voters' point of view and find little support for the picture of a highly organized hierarchical social structure.

Scott Flanagan deals with the mechanisms of social network influence on Japanese voting behavior, distinguishing assimilation effects, reference group effects, opinion leader effects, and mobilization effects. He finds that "Japanese social networks will in many cases supply an apolitical voter not only with name recognition and a favorable image for a particular candidate but also with a strong motivation to vote" (p. 158). Bradley Richardson analyzes influence communications and finds that "the choices of roughly one-quarter to one-third of the Japanese electorate are directly affected in some way by influence communications processes in House of Representative elections" (p. 355). Not only are these proportions smaller than the literature had led us to expect, but candidate images prove more powerful than personal requests. In the 10% of the cases in which there was a conflict between these two forces, "candidate images prevailed over requests three times as often as requests overwhelmed candidate images" (p. 361). Candidates may have effective organizations but they seem to consist of activists and opinion leaders and do not include a large number of loyal voters.

These findings bring our image of the Japanese voter closer to our image of voters elsewhere. Instead of a "feudalistic" personality imbedded in a social structure so tight that it often renders individual preferences irrelevant, the Japanese voter emerges as relatively unconcerned about politics, with political beliefs and behavior influenced by many different factors. The picture that emerges is complex and not particularly complimentary but is completely recognizable as a variant on the theme of voting behavior in industrial democracies.

University of Alabama STEVEN R. REED

Parties and Voters in France. By John Frears. New York: St. Martin's, 1991. 240p. $49.95.

For the first 20 years of its existence, the French Fifth Republic was regarded suspiciously by domestic and foreign observers: How could a political system so deliberately shaped to conform to the political philosophy and skills of one man—Charles de Gaulle—survive the disappearance of that great man and his immediate successors? Well into the second decade of its existence, many still talked of the Fifth Republic as only a "parenthesis" in the history of French democracy. However, in the last 10 years, the regime has successfully met two key challenges: the transfer of power from the Gaullists and their allies to the Left and the division of power (*cohabitation*) of a president and the parliamentary majority from political adversaries.

In his latest book, John Frears points to these events as confirming conclusively the strength and durability of the regime and the breadth of cross-partisan support for the institutions of the Fifth Republic. He suggests that this may very well end the kind of regime instability that has characterized French political history for the past two hundred years. He argues that the consolidation of an effective and simplified party system played a major part in these successes. Frears points to the emergence of parties with the capacity to form and support governments in power as the major innovation of the Fifth Republic and the source of the strength of its institutions. He argues, further, that it was the transformation of the Gaullist party from one of personal supporters of General de Gaulle into a large, catch-all party that brought together almost all the previously contentious strains of the French Right and Center. The early successes of Gaullism created a "majority habit" that compelled opposing parties to unite and seek to develop their own majoritarian potential. Thus, Frears sees the changes in the Right as pressing the parties of the Left to change and to unify.

After presenting these arguments in a persuasive manner, Frears then provides separate chapters treating the background, policy positions, electorates, and organizational structures of the politically significant parties and party coalitions of contemporary France. Frears includes discussions of some parties that are relatively neglected, such as the French Greens. Later chapters describe the rules and procedures for presidential, parliamentary, and local elections and referenda and give brief descriptions of the issues and major contenders in each national election since 1958.

Frears' contentions about the importance of party change in explaining the success of the Fifth Republic and the key role of the Gaullists in bringing about these changes are briefly but persuasively presented. However, the major strength of this book is the valuable resource it offers students and scholars on the overall party system and election scene in France. It is the only up-to-date general survey on this subject in French or English. It is clearly written and accurate. The chapters on the individual parties are rather short; and a longer bibliography would have been helpful in directing those

who need more information to more comprehensive studies.

This is not a book that applies sophisticated theories on political parties to France, nor does it report on important new field research. Specialists on French politics will find little new. But the Frears book does do something that we sometimes neglect in American political science by providing a reliable, readable, and thoughtful synthesis of the theoretical and empirical research on a key set of political actors. As such, *Parties and Voters in France* will serve as a valuable resource for students and scholars.

Purdue University FRANK L. WILSON

Revolution and Rebellion in the Early Modern World. By Jack A. Goldstone. Berkeley: University of California Press, 1991. 608p. $34.95.

Living in an age when new-style revolutions have toppled regimes from Bucharest to Budapest, we find ourselves once again (as in the days of Vietnam) seeking to explain why and how revolutions occur. Familiar theories of imperialism (Eric Wolf, James Scott), class conflict (Barrington Moore, Jeffery Paige), relative deprivation (Ted Gurr, James Davies), or state incapacity (Charles Tilly, Theda Skocpol) seem less applicable to the revolutions of 1989 than to the "great revolutions" that occurred earlier in this century. Perhaps our models of revolutionary change will have to become more historicized, more tailored to the specifics of different periods and cultures.

Jack Goldstone, in his stimulating study of revolution and rebellion in early modern Europe and Asia, makes a similar point. He notes that most theories to date have been "concerned with explaining twentieth-century revolutions, in which the collisions of international colonial and economic forces with traditional regimes played a major role. . . . The theory of revolutions has yet to address early modern political crises in their own right" (p. 23). Goldstone further criticizes previous work for neglecting "the impact of cultural differences, without which one can hardly comprehend the varying effects of state breakdown in early modern Europe and Asia" (p. 23).

For his own part, Goldstone offers a "demographic/structural model" of state collapse, arguing that revolutions occurred when population pressures overwhelmed intransigent political, economic and state institutions. This is not crude Malthusianism; Goldstone concentrates on "the *distributional* effects of relative shifts in population and resources, rather than on massive shortages for whole societies" (p. 37; emphasis original). Moreover, he sees population pressure as developing in a pattern of recurrent waves (driven by changes in climate, disease, food supply, etc.), rather than in linear fashion.

Goldstone's focus on early modern uprisings, on ecological crises, and above all on European–Asian comparisons (the English Revolution, the French Revolution, the crisis of the Ottoman Empire, and the transition in Ming–Qing China) brings a fresh perspective to the study of rebellion and revolution. The chapters on England and France are impressively detailed, presenting both qualitative and quantitative evidence to raise questions with prevailing scholarly interpretations of these familiar events. The analysis of the Ottoman and Chinese Empires, though less original, is useful for further debunking the old myth of an East mired in stagnation while progressive revolutions in the West gave rise to democracy and capitalism.

Insightful as his approach is for the study of early modern rebellions, Goldstone seems overly anxious to extend its applicability across time and space. Area specialists will surely shudder at his claim that "the states of early modern Eurasia—including the European monarchies, Russia, China, the Ottoman Empire, and Japan, in roughly the sixteenth through the nineteenth centuries—were not greatly different from each other" (p. 4). And historians are likely to recoil from the temporal leap involved in the assertion that "the study of early modern history suggests that the current U.S. course [of failing to reduce consumption] will lead to heightened factional conflict and economic weakness" (p. 496). Greater attention to the peculiarities of different periods and places would have strengthened, not diminished, the overall impact of the book. As it is, the concluding chapters (one on ideological and cultural factors, the other on lessons for the present) seem like afterthoughts, intended to stretch the conclusions of the study beyond their legitimate boundaries.

Whether or not this volume has much to tell us about the current fiscal predicament of the United States or, for that matter the revolutions of 1989, it is in any case a major contribution to our understanding of earlier uprisings. While other scholars (most notably members of the Annales school of French history) have shared Goldstone's interest in demography and ecology, none has developed the analysis in such rigorous fashion. This is an important book from an innovative and broad-gauged scholar. It belongs on the reading list of anyone interested in the dynamics of revolutionary change.

University of California, Berkeley ELIZABETH J. PERRY

Public Choices and Policy Change: The Political Economy of Reform in Developing Countries. By Merilee S. Grindle and John W. Thomas. Baltimore: Johns Hopkins University Press, 1991. 222p. $38.50 cloth, $13.95 paper.

Ideas and Institutions: Developmentalism in Brazil and Argentina. By Kathryn Sikkink. Ithaca: Cornell University Press, 1991. 263p. $35.00.

Political and economic upheaval have refocused attention on the origins and determinants of major policy shifts; and growing dissatisfaction with reductionist, often agentless explanation has reawakened interest in the roles of individual policymakers and the ideas and values they hold. The timing is therefore auspicious for the publication of this pair of first-rate books, both of which (despite differences in emphasis and intended audiences) address these critical subjects. As Kathryn Sikkink observes, "It is a paradox that scholars, whose entire existence is centered on the production and understanding of ideas, should grant ideas so little significance for explaining political life" (p. 1), while Merilee Grindle and John Thomas note that despite their absence from many explanations of policy reform, "people . . . change policy, and do it from motivations and perspectives that are imbued with personal and profes-

sional values that frequently include serious concern for the public interest" (p. xiv).

Both books seek to explain where policymakers' ideas come from and how and how much their ideas and actions shape outcomes. Both offer similar criticisms of existing approaches: societal (class, pluralist, public choice), statist (state autonomy, bureaucratic politics), and rational choice. In general, the authors conclude, such arguments are meaningless without reference to, as Sikkink (p. 243) puts it, the ideas "without which no understanding of interest is possible"; or else they fail to account for the significant room for maneuver that policymakers possess or for how policymakers choose among options within it. Neither Sikkink nor Grindle and Thomas dismiss these approaches entirely or treat ideas and individuals as fully autonomous. Instead, they fashion arguments that locate policymakers, their ideas, and their motivations within historical, institutional, and societal settings, permitting us to assess how specific policymakers come to decisions about specific policies and how and how fully they succeed in implementing their choices, given state institutional capabilities and state–society relations.

Of the two books, Sikkink's *Ideas and Institutions* has the wider academic appeal, fitting neatly into the growing literature devoted to "bringing ideas back in," where it compares well with the work of Ernst Haas, Peter Hall, Aaron Friedberg, and John Odell. Sikkink poses two questions: "Why were similar developmentalist policies adopted by Brazil and Argentina [in the 1950s and early 1960s]? and why did these economic policies have such different outcomes?" (p. xiii). *Ideas and Institutions* offers an excellent study of a critical era in the history of two major Latin American countries during which their development trajectories diverged sharply in ways which still shape their capacities to implement structural adjustment. Sikkink draws on extensive archival research and interviews with many of the key figures. Finally, as befits a book about ideas, *Ideas and Institutions* is gracefully written. Sikkink shows a nice ability to bring high-flown analysis back to earth with a single, telling observation.

Sikkink begins with a précis of her argument and a review of alternative explanations and then moves to a fascinating intellectual history of developmentalism in Latin America as a whole and Argentina and Brazil in particular. The chapters on the developmentalist experiments in each country offer cogent explanations for the very different ways in which developmentalism was adopted, implemented, and consolidated in Argentina and Brazil and challenge pat understandings of this period. In fact, for Latin Americanists, Sikkink's major contribution may be her richly documented addition to our knowledge of this critical, but underresearched, subject. Not content to discuss ideas in isolation, however, Sikkink devotes two chapters to where developmentalist ideas lodged in the Argentine and Brazilian states and to "how differences in state autonomy, state capacity and state purpose influenced the implementation of developmentalist policies" and thus explain "the different economic and political outcomes" (p. 28). A final chapter returns to the role of ideas and the problems of constructing explanations that capture the recursive relationship among objective conditions, material interests, and actors' ideas.

While very good, *Ideas and Institutions* has faults. Perhaps most important, Sikkink does not always demonstrate the need to turn to ideas when more parsimonious explanations exist. She expresses surprise, for example, that Argentine industrialists failed to support the Frondizi government, which was, she asserts, "firmly committed to industrial development," a failure attributed to the distorting "lens" of industrialists' "prevalent ideology" (p. 108). Conversely, she ascribes Brazil's greater success to the support of industrialists who shared the Kubitschek government's developmentalism. Yet Sikkink notes that Frondizi's industrial policy called for a 50% cut in import taxes, which threatened inefficient Argentine firms (p. 109), while Kubitschek's tariff policy called for "strongly protectionist . . . new rates," which protected domestic industry (p. 142).

Though they, too, have academic readers in mind, Grindle and Thomas address themselves first to practitioners. They endeavor to provide a model of the policy reform process in order "to enable policymakers better to manage implementation" (p. 122). They offer not a predictive theory of policy reform but a "framework" that "systematizes thinking" and maps out "the intuitive knowledge of skilled policymakers and public managers" (p. 187). To illustrate their story, Grindle and Thomas use "mini-cases" of policy reform from a variety of Third World countries. They thus achieve that rare hybrid—a readable fusion of abstract argument and nitty-gritty detail that should interest and enlighten students, academics, and policymakers alike.

Public Choices begins with a generic picture of government decision making in the Third World and a critical review of the literature, whose aim is to show that reform is the norm, not the exception, and that policymakers are more autonomous and possess more room for maneuver than is generally conceded. The book then follows the policy process from start to finish, seeking to specify what accounts for variation. Thus, having identified environmental factors and personal characteristics affecting policymakers, Grindle and Thomas analyze agenda setting, focusing on the implications of crisis versus "politics-as-usual." They go on to analyze how these differences shape the decision process in light of the public and bureaucratic characteristics of specific reforms. Finally, they apply their model to the dramatic reforms of the 1980s and "test" it (though this is too strong a word) in 14 mini-case studies.

Public Choices, too, has faults. The dependence on mini-cases is methodologically suspect. They are at best illustrative and cannot provide a test of the authors' propositions. Moreover, their cumulative effect leaves the impression that each situation is unique and that generalization is impossible. This impression is reinforced by the authors' model, which is as complex and ambiguous as reality itself, since it includes, just by way of scene setting, the "ideological predispositions, professional experience and training, memories of similar policy situations, position and power resources, political and institutional commitments/loyalties, and personal attributes and goals" of policymakers and the "societal pressure, historical context, economic conditions, international context, administrative capacity, and other policies" that constitute the context of any given policy choice (p. 184). In other words, while this model may permit the descriptively accurate categorization of all the elements of completed reform initiatives, it possesses no *explanatory* power for an academic analyst and limited utility as a guide to practitioners.

Their problems notwithstanding, these are important,

informative, and compelling books. Sikkink must be congratulated for her excellent paired case study and for her contributions to the intellectual history of Latin America and to the effort to understand how and under what conditions ideas shape policy outcomes. Grindle and Thomas offer a critical reminder of the role individual policymakers play not as "creatures of the societies or classes from which they come" (p. xii) but as leaders in the proper sense of the word. Their work should, as they hope, "help generate principles of action for reform leadership" (p. 193) and challenge scholars to look beyond the obstacles to change and engage the reforms that are transforming the Third World.

Rutgers University D. MICHAEL SHAFER

Public Enterprise in Kenya: What Works, What Doesn't, and Why. By Barbara Grosh. Boulder: Lynne Rienner, 1991. 223p. $32.00.

African Successes: Four Public Managers of Kenyan Rural Development. By David K. Leonard. Berkeley: University of California Press, 1991. 406p. $55.00 cloth, $18.95 paper.

Kenya has long been considered a development success story on a continent where most other countries have recorded negative economic growth and significant cutbacks in social development programs. Using a cross-section of economic and social indicators listed in the annual *World Development Report* (published by the World Bank) as the measuring rod, Kenya comes out among the top five African countries in almost all instances. More recently, however, questions have been raised about the solidity of the Kenyan successes. The increasingly autocratic and corrupt nature of Kenyan politics under President Moi has left many admirers of the country's proud development record in doubt about the prospects of keeping it alive in the 1990s. In late 1991, Kenya's principal donors took the unusual step of freezing their development aid for six months or until they see proof that President Moi has introduced democratic reforms and curbed the widespread corruption in the public sector.

The two books under review here help us understand many of the reasons for Kenya's progress but also draw our attention to how arbitrary and ill-conceived decision rules have increasingly come to threaten further advancement. Both authors assume that the human factor is important in development and provide convincing evidence that what public managers do makes a difference. This emphasis on agency, rather than structure, is refreshing and provides a useful counterpoint to the overwhelmingly structuralist interpretations that have for long dominated the literature on Kenya and other African countries.

Barbara Grosh's study covers the operations of 32 public enterprises over a 25-year period (1963–88). The sample contains almost all the firms that depend on revenue from sales and that are overwhelmingly state-owned. It includes enterprises serving agriculture, finance, and development finance institutions, as well as parastatal enterprises providing infrastructure services.

The principal conclusion of her study is that public enterprises in Kenya work remarkably well. She questions, therefore, the validity of the prevailing opinion in the Kenya government, as well as among donors, that they constitute a major problem. In her view, there is cause for neither extensive privatization (as the donors insist there is) nor greater political control (as the Kenya government believes is necessary). To be sure, not all enterprises operate equally well; but the reason for poor performance is typically external, not internal, to the enterprise. A main culprit is the government itself, by setting prices or insisting on decision rules that leave management without a chance to run the enterprise in the black. Kenya's parastatal managers, therefore, are not villains but victims of circumstances beyond their control. With greater sensitivity to the commercial needs of these enterprises, the Kenya government could provide them with a much more enabling environment.

Grosh has no major theoretical pretensions in her study. It is a down-to-earth examination of a long series of financial records. Its principal value is twofold. The prevalence of public enterprises in African countries notwithstanding, few independent studies have been done of these institutions. To be sure, there have been several public inquiries and reviews; but largely because of difficulty of access to relevant records, only a small number of academics have tried their luck in this field. Thus, Barbara Grosh's study is filling a vacuum. Second, her pioneering effort is enhanced by the comprehensive and longitudinal nature of the investigation. The result is a more nuanced picture of these enterprises and a greater appreciation of the many factors that shape their performance.

The flip side of the extensive examination of performance records is, of course, that the author has no time to carry out any in-depth study of the many cases she includes: statistics are speaking for themselves. One wishes that Grosh had provided her readers with more feeling for the reliability of the records she examines. Another criticism I have is that she fails to recognize that the conclusion she draws about the variable performance of these enterprises and the constraints placed on "heroic" managers echoes concerns expressed by these managers for over two decades now without any effective reforms having been taken. For example, a closer examination of the published records of the seminars held in the 1960s and 1970s under the auspices of the African Association of Public Administration and Management and its precursor, the Inter-African Seminar of Public Administration, would have placed the analysis in this study in a better historical perspective.

Fifteen years ago, Leonard made a major contribution to the field of development administration with his *Reaching the Peasant Farmer*, a systematic study of the agricultural extension service in Kenya's Western Province, using hypotheses derived from organization theory. With *African Successes*, he confirms his position as one of the leading scholars in this field. This study, however, is very different from his first one and contrasts also with that of Grosh. Using the life stories of four exceptionally successful public managers, Leonard explores the reasons for Kenya's positive development record since independence. Thus, he is this time more concerned with generating, rather than testing, hypotheses. This inductive approach leads him to two major conclusions: (1) that Kenya's advance can be attributed to the emergence of a class of independently wealthy people based in agriculture, rather than in urban-based enterprises, and (2) that professionalism makes a difference and needs to be carefully nurtured.

Leonard's book is not your usual political science text.

Though bold and imaginative, his methodology in the end earns him a lot of empirical mileage. With the help of some three hundred intensive interviews (some lasting almost a whole day) with the four principal personalities of this study and their relatives, friends, and former colleagues, he is able to provide a unique and fascinating insight into how policies were made in Kenya and what factors seem to have shaped the evolution of its politics. In weaving together the innumerable threads that his study engenders, Leonard moves in a masterful manner back and forth between the micro and macro levels of his analysis.

The author never gets lost in his rich empirical material. The book is very well organized. A principal set of arguments is consistently pursued. To make his story as readable and accessible to as broad a readership as possible (a gesture that few academics make), Leonard relegates his quantitative analyses to two appendixes. The result is that when you come to the end of the book you realize that you have a human interest story and an academic treatise in one, perfectly merged. This is a mark of outstanding scholarship, rare among even the more experienced and meticulous.

There is so much about this unusual book that one would wish to discuss, but suffice it to say that it is of interest not only to scholars in development administration. It makes a contribution in its own right to the study of Kenyan politics and also, albeit more distantly, to broader concerns of comparative politics at large. One also wishes that as many political scientists as possible get a chance to become familiar with his methodology, including his choice of emphasis on "deviant cases."

The latter is obviously not uncontroversial. What conclusions can one really draw from, in this case, the lifetime achievements of four public managers who stand out as different by virtue of their professionalism and leadership abilities? Leonard may be accused of overdramatizing the significance of his conclusions, particularly as they relate to the managerial significance of "pockets of professionalism" in the contemporary African developmental context.

This and other criticisms notwithstanding, Leonard's book must be counted as one of the most thorough and best-crafted studies by an Africanist political scientist in recent years. What is more, its significance and value transcends the boundaries of African studies.

University of Florida GORAN HYDEN

Free to be Foolish: Politics and Health Promotion in the United States and Great Britain. By Howard M. Leichter. Princeton: Princeton University Press, 1991. 281p. $35.00.

Doctors and the State: The Politics of Health Care in France and the United States. By David Wilsford. Durham, NC: Duke University Press, 1991. 355p. $49.95 cloth, $19.95 paper.

Whenever governments intervene in a policy area, they encounter complex webs of technical, economic, and political interrelationships. All these factors are present at an extraordinary level in the health policy arena. Governments confront well-organized and powerful interest groups of health care providers. They must also confront powerful funding agents for medical care. The politics are no less intense when government itself is the principal funder of medical care. Increasingly, government also confronts interest groups attempting to redefine the very nature of health care and to redirect the attention of well-structured, functioning policy communities. Understanding these relationships between state and society is important within any one country; but to conduct the analysis comparatively presents a much greater opportunity for developing political theory, as well as generating more real substantive knowledge about the policy area.

The two books reviewed here make significant contributions toward an enhanced understanding of the changing role of government in health care. David Wilsford's *Doctors and the State* discusses the more conventional question of the confrontation between the power of the state and that of the organized medical professions. Howard Leichter's *Free To Be Foolish* addresses the less frequently discussed issues of the role of the state in health promotion and the use of its power to make individual citizens behave differently to improve their own health. While the subject matter may appear somewhat different, the fundamental questions raised by the two volumes are really very much the same. One question is how the state can exercise its power effectively to improve the health of its citizens. The other question is the normative one of the appropriate role of government in altering behavior of both providers and consumers. Does the financial power, combined with its substantive knowledge, justify government's substituting its collective judgement about health for that of individuals?

My saying that the Wilsford book addresses a rather conventional question is not intended to denigrate this volume. The question is a familiar one, but this is an excellent treatment of the subject of the relationship between government and the health professions. The discussion extends the usual arguments to examine a range of connections between state and society. The selection of the United States and France as the two cases appears to make the question of state power versus professional providers too obvious for the researcher and reader; and to some extent it does. However, the author pursues the implications of the differential power of the state in interesting ways and also points out many of the subtleties of the two political systems. This is especially true in the case of France. In particular, Wilsford points out that the (relatively) strong and autonomous state in France is not omnipotent and cannot guarantee policy success, as the characterization of the "continuity of crisis" in French health care indicates. In addition, the two-country comparison is extended to include four other systems with varying degrees of government involvement in the provision of health care. This extended comparison helps to clarify the author's findings for the two major cases.

As noted, Howard Leichter addresses a somewhat more novel question of the role of the state in modifying individuals' behavior that can affect their health. It is hardly news that a great deal of an individual's health and well-being is a function of life-style. The question is whether government should encourage or require people to change unhealthy life-styles. Not only can government be seen to have some moral responsibility to improve the health of its citizens, it has a real financial stake as a major funder of health care (even in the United States) to reduce illness. These claims must be set against the claims of individuals for personal autonomy

and individual freedom. These conflicting claims have very direct life-and-death importance in health care; but analogous conflicts can be pointed to in other policy areas in which government can be argued to be producing a merit good (e.g. education or the environment).

Leichter also bases his analysis of health policy on the comparison of two countries: the United States and the United Kingdom. He discusses an interesting and significant range of health issues, ranging from smoking to AIDS. Here, the findings are somewhat different from those in Wilsford's book, with the United States' being somewhat more interventionist and the British government tending to rely more on voluntary agreements. In each case, however, the effects of this "second public health revolution" is to shift much of the blame for poor health onto individuals and their own behavior, leaving government only the option of altering individual lifestyles if they are to be successful in promoting health. That leaves unanswered, however, the more basic question of whether governments should be in the business of promoting health, especially when it requires intervention into personal liberty. Leichter is to be complimented for his manner of raising these thorny questions and willingness to accept indefinite and partial answers to them. The analysis might have been improved, however, by some greater attention to issues of agenda setting and to how policy issues are socially and politically constructed.

These two books, taken together, are a useful introduction to the health policy issues facing governments in the 1990s. On the one hand, governments must make difficult economic decisions about funding health care and their willingness to use their powers to combat the powers of private health providers who can profit from public programs. On the other hand, governments must think about how to make citizens healthier as another means of reducing their costs of health care. The two strategies of intervention are complementary, and both involve substantial political costs. Readers will understand both the strategies and the costs better by having read these books.

University of Pittsburgh B. Guy Peters

Communities of Grain: Rural Rebellion in Comparative Perspective. By Victor V. Magagna. Ithaca: Cornell University Press, 1991. 277p. $42.50.

Serious study of peasant rebellion is a relatively new phenomenon; but the field has been greatly enriched in recent years by the work of Migdal, Paige, Popkin, Scott, Tutino, Wolf, and others. Magagna's volume seeks to make a contribution to this growing body of literature by proposing a different framework to answer the question, Why do peasants rebel?

The central thesis of this work is that agricultural societies cannot be understood in terms of class (as Marxists theorists argue) or in terms of subsistence (as those coming from the moral economy tradition would have it). Moreover, historical sociologists, such as Barrington Moore, Jr. and Theda Skocpol, who have analyzed such societies primarily from the point of view of nonagricultural elites and states are also in error. Such analyses have been faulty, it is argued, because they have led to ambiguous conclusions by an undue emphasis on economic issues.

According to Magagna, the most appropriate way to understand rural rebellion is to view the problem from the perspective of community, specifically "communities of grain." He acknowledges that the use of the community as the unit of analysis is not new, since it has been the staple of ethnographers for many years. But Magagna seeks to tighten the definition of community by focusing it on territoriality and land tenure and arguing that bucolic notions of egalitarian communities are largely inaccurate, since most communities are characterized by inequality and hierarchy, based on such factors as status, gender, and age. Despite these inequalities, they are offset by reciprocity and widely accepted rules that draw together communal residents, thereby allowing them to undertake mobilized political action. It is out of this community action that rural rebellion becomes possible.

Although there are many specific causes of rural rebellion, including conflict over land, religion, taxation, and so on, Magagna argues that no one of these can really explain the phenomenon. Neither relative, nor absolute, deprivation sparks such rebellions. Rather, such insurrections are a form of a broader phenomenon that he calls *representative violence*—an unfortunate use of terminology that will end up confusing many readers. He rejects the clearer term *bargaining by riot* for he fears its use might lead to confusion with urban movements. What is meant is that rural rebellion is to be interpreted as a means for communities to present (or re-present), in a violent fashion, their demands for a change in elite behavior. These movements, then, are a mechanism of community defense.

Communities vary along two closely related dimensions: autonomy and cohesion. This framework gives rise to three types of communities: redistributive, regulative, and residual. The case studies used in the book to illustrate these various types are compiled from cases that meet two criteria. First, they must be cases for which an extensive secondary literature has been compiled, a factor that results in the bulk of the cases being selected from Europe. Second, the cases must be central to the debate over the historical sociology of rural rebellion. These criteria motivate Magagna to select cases from France, Spain, and Russia. He adds England and Japan to the list as control groups—England because the individualistic tradition found there presents the most difficult test for a theory based on community and Japan so as to include a non-Western case. The illustrations are, for the most part, pre–twentieth century. The focus of the chapter on England is the period 1500–1800, that on France covers the period 1500–1900, that on Japan, 1600–1868. Those on Spain and Russia cover more modern periods, 1800–1939 and 1300–1930, respectively; but none of the material deals with the contemporary era. Each country is covered in a separate chapter, and extensive use is made of an impressive array of secondary materials, all carefully footnoted to enhance its accessibility to the reader.

The review of the cases produces two broad categories of rural rebellion: movements of secession and movements of reconstitution. The Russian and Japanese cases are an illustration of the former process, whereas the illustrations from England, France, and Spain are seen as efforts of communities to alter the nature of the relationship between local community and external elites and markets. The type of rebellion is not a function of macro societal factors, such as the evel of modern-

ization or commercialization. Rather, when commercialization occurs from above and becomes "coercive commercialization," secession is most likely to result, whereas, when it comes from below, a rebellion demanding reconstitution is the likely outcome.

The concluding chapter of the volume attempts to draw together all of this material via the presentation of nine hypotheses. Magagna admits that the hypotheses are not operationalized; but, he says, "The application of these hypotheses, even to third-world countries, may help to explain much about the nature of rural politics" (p. 256). He goes on to argue that the communities-of-grain concept is applicable, at a minimum, to Mexico and the Andean countries and portions of sub-Saharan Africa and Southeast Asia. One group of hypotheses deals with the question of the stability of communities; a second, with differences in levels of violence; and a third, with the variation in the impact of ritual on community cohesion.

The hypotheses presented will disappoint those who are looking for a theory of peasant revolution. Indeed, Magagna argues specifically that the rural rebellions he has studied, "do not add up to a revolution in the classic sense" (p. 258). Moreover, Magagna acknowledges that the enormous power of modern armies makes the rural rebellion of the type dealt with in this book increasingly obsolete. Yet he suggests that Vietnam may be a case in which rural rebellion is still an active force today, although few readers would agree that this is an instance of local community violence. In the end, he is compelled to admit that rural rebellion may now be "out of date" (p. 263), so that his study appears to be of little utility to those who wish to understand such important contemporary cases of rural rebellion as those found in Guatemala, El Salvador, Peru, and elsewhere.

This is a book with a large scope, both theoretically and substantively. It is an excellent illustration of comparative analysis; and the reader cannot fail to be impressed by the richness of its treatment and the effort to synthesize greatly divergent case material into a parsimonious set of nine hypotheses. The ultimate utility of the volume, however, will depend on scholarly acceptance of the notion of "communities of grain" as the appropriate organizing construct for understanding and explaining rural rebellion.

University of Pittsburgh MITCHELL SELIGSON

Militarism and Politics in Latin America: Peru from Sanchez Cerro to Sendero Luminoso. By Daniel M. Masterson. Westport: Greenwood, 1991. 345p. $47.95.

Daniel Masterson has done Latin American (especially Andean) specialists a considerable favor with this book. By concentrating on one country (Peru) and one fairly short historical period (60 years), he has produced a monograph that in extraordinary detail describes the decades-long search for what Masterson calls the Peruvian military's professional mission or rationale—in a word, its raison d'être. All militaries everywhere have two basic missions: to protect the nation–state from external aggression and to assure domestic order. But Third World militaries may not be threatened by outside aggressors or may play only a peripheral part in global power struggles. For example, in the twentieth century, Latin America did not have a major role in hemispheric defense, because the United States assumed such responsibility throughout the Cold War. Latin American militaries have thus found themselves doing either or both of the following: (1) protecting borders that (with a few notable exceptions) were not about to be invaded from the outside (which did not prevent arms races and very large military budgets), and (2) concerning themselves with internal affairs. The question that occurs, of course, is, What shape or form should such concerns take? The search for answers to this question comprises the core of Masterson's book.

Peru has for many years had one of Latin America's more idiosyncratic militaries. It has intervened in the political arena to such an extent that a constitutional transfer of presidential power has occurred only three times since World War I (1945, 1985, and 1990). Yet these interventions have not taken place simply because one caudillo wished to take the place of another. One of the strengths of Masterson's book is his ability to point out the multiple, often overlapping, and even contradictory motives behind the military's coups. Masterson traces the influences first of France and later of the United States, as countries that dominated Peruvian military training and describes the schizophrenic reaction as Peru switched from one to the other. But Masterson also takes care to show how domestic political circumstances in Peru affected the military. Conventional wisdom, for instance, has long held that the military intervened throughout the 1930s and 1940s primarily because of its deep-seated enmity for (APRA), Peru's then radical party. But Masterson demonstrates that such an explanation is simplistic by stressing the divisions within the military about APRA and the degree to which young, reform-minded officers often sided with APRA in planning uprisings and coups against oligarchic rulers who were supported by senior military officers. He also convincingly argues that interservice rivalries based on ideological and personal differences also played major roles, especially during the 1950s and 1960s and notes, as well, that the United States' insistence on anticommunist strategic planning and counterinsurgency techniques also played a role, as did the military's self-perception during the late 1960s as the only organization within Peru able to take on the reforms needed for independent development to take place. All of these arguments have been made previously, of course, but Masterson is especially successful in showing how these several themes become differentially interwoven over time and how they influenced the military's activities. Such success is possible thanks to his use of a wide variety of primary and secondary sources, including archival materials in Peru, as well as the United States, Peruvian military journals, and personal interviews.

Masterson's intent—to detail the Peruvian military's search for a clear professional rationale—is thus accomplished. For political scientists searching for an understanding of Peru and its military within some larger, more generalizable context of civil–military relations, however, the book is less satisfactory. No mention or use is made, for example, of Stepan's work on Latin American militaries or of the 1980s literature on transitions to democracy (O'Donnell and Schmitter; Diamond, Linz, and Lipset, etc.) to understand Peru's own transition in 1980. Social scientists attempting to fit the Peruvian case into a larger explanatory framework will therefore be left to their own devices. Since Masterson is, in fact, a historian, perhaps such a complaint is unjustified;

nevertheless, the main title should perhaps be dropped, since the book addresses Latin America in general not at all. That being said, Masterson allows Andean specialists to fill in numerous gaps in their knowledge and Latin Americanists to gain a much greater comprehension of Peru and its military than was previously available. How Peru can be best analysed and explained by a more general framework, or how such a framework might have to be modified to take account of the idiosyncracies of the Peruvian case, remains to be done.

University of Texas, Austin HENRY DIETZ

Partial Visions: Culture and Politics in Britain, Canada, and the United States. By Richard M. Merelman. Madison: University of Wisconsin Press, 1991. 287p. $40.00 cloth, $15.95 paper.

This is an ambitious, highly innovative, and partially successful work. Richard Merelman has produced an important and idiosyncratic study of popular culture and its relationship to democracy in the United States, Canada, and Britain. Though the back cover of the book hails it as a "pathbreaking study of political culture," the author is careful to distinguish his approach from "the dominant mainstream political science approach to the study of political culture," which is fraught with "insufficiencies," has "yielded few fruits," and consequently is a methodological antimodel, an example of "how not to proceed" (pp. 39–40, 44).

Though Merelman shares with Almond and Verba a deep concern for the cultural ecology of democracy, he insists that their approach, which relies on survey data and analysis of political attitudes, must be placed "within a broader theoretical framework" derived in part from anthropology (p. 44). While recognizing that there are several different anthropological schools of thought about culture, Merelman opts for the "symbolic" approach, which sees "culture as a system of discourse" and contends that "social behavior proceeds from and acts out symbolic systems, texts, or . . . *collective representations*" (p. 45).

Merelman undertakes to study cross-nationally collective representations of four collectivities (the *family*, the *corporation*, the *polity*, and the *world of consumption*) and their corresponding social roles (*family member*, *employee*, *citizen*, and *consumer*). Each of these collective representations become infused into the "system of discourse" of the "popular culture." Each therefore affects the understanding and practice of democracy, whose essential qualities, Merelman insists, entail a healthy amount of "conflictive democratic participation." He summarizes his argument by stating that "certain qualities of a society's popular culture subtly prepare people either to seek out political participation and welcome group conflict or to resist political participation and to reject group conflict" (p. 8).

In order to analyze these cultural tendencies in each of four collectivities, Merelman turns to the source of what he believes is the most influential collective representation of their corresponding social roles: television programs (family member), corporate publications (employee), high school textbooks (citizen), and magazine advertisements (consumer). Using qualitative analysis of these primary sources, supplemented by contextualizations drawn from selected secondary sources, Merelman provides us with interpretative essays on the unique "cultural configurations" of each of the three countries. This is followed by a chapter entitled "The Political Embodiment of Liberal Democratic Culture," in which he analyses voting behavior, public opinion, and elite political discourse, which are "surface embodiments" of the "deep structures" that comprise the cultural configuration. By far the most interesting aspect of this analysis is the quantitative and qualitative examination of political language in each of the three societies. Merelman presents paired comparisons of debates in Britain and the United States on party reform and texts of important documents on bilingual education in Canada and the United States. Like his use of the primary sources that underlie his characterization of the cultural configurations in the three countries, these analyses are methodologically innovative and enlightening. They do not, however, link up very well to his hypotheses; nor do they provide convincing elaboration of his definition of democracy as implying conflictive participation.

In a final, reflexive chapter, Merelman looks back over his work which he succinctly indicates is a "partial argument" and criticizes its limited validity. Merelman accepts that the problem of the intelligibility of culture, its relationship to institutions, and the problem of cultural dynamics and political change are not taken up thoroughly in his book. But he never extends his self-criticism to his characterization of democracy itself, which remains idiosyncratic and incomplete.

Despite its shortcomings, this book is, nevertheless, an important path-breaking study that should open the way to new research directions and inspire much controversy in the discipline. Merelman has explored new ways of conceptualizing culture and displayed exciting examples of qualitative and quantitative empirical analysis. Although it is perched uncomfortably on the conjuncture of positivist and postpositivist methodologies, it has much to contribute to the renaissance of interest in culturalist approaches.

York University, Toronto DAVID V. J. BELL

Social Currents in Eastern Europe: The Sources and Meaning of the Great Transformation. By Sabrina P. Ramet. Durham, NC: Duke University Press, 1991. 434p. $49.95 cloth, $19.95 paper.

This lengthy and ambitious volume seeks to describe and explain the social roots of the collapse of the socialist states in Eastern Europe in 1989. The author boldly includes all the countries of the region, although the coverage of Poland, East Germany, and Yugoslavia is more thorough than that of the remaining countries. The final chapter offers a hasty summary of political developments in 1989 and 1990, but the main focus of the study is on independent social groups and movements in the 1970s and 1980s.

The author's argument is that the popular basis of the East European regimes was steadily eroded by a broad variety of social processes, so that by the late 1980s, the arrival of Gorbachev could serve as a catalyst sufficient to trigger their collapse. In the abstract, one can hardly disagree with such a thesis; but the author does not offer a more probing analysis of the precise links between the social phenomena she describes and the regime changes that took place in 1988–89.

The social processes covered in the book are a fairly eclectic selection: Solidarity in Poland, dissident intellectuals in East Germany and Czechoslovakia, Yugoslav feminists, rock musicians, and churches and sects throughout the region. The author states that these are all manifestations of a process of "modernization"; but apart from some rather mechanical references to the works of Samuel Huntington and Crane Brinton, the analytical framework is not really elaborated and explained. For example, there is no systematic discussion of social mobility, urbanization, or education levels, all usually regarded as central to modernization theory. Most surprising of all, there is virtually no discussion of the role of nationalism, apart from a case study of the Albanians of Kosovo, Yugoslavia (Chapter 7). Developments since 1989 have, of course, clearly shown that nationalism has the capacity to displace group identity in several, if not most, countries of the region.

Thus, the value of the book is that it provides a selection of detailed case studies of social phenomena of East Europe in the 1970s and 1980s. Although the book is primarily derived from journalistic sources, including the invaluable Radio Free Europe reports, the material is so rich that even area specialists will discover something new in this volume.

Duke University PETER RUTLAND

Crisis and Choice in European Social Democracy. By Fritz W. Scharpf. Ithaca: Cornell University Press, 1991. 303p. $49.95 cloth, $17.95 paper.

This absorbing study, first published in German in 1987 and now appearing in a clear and elegant English translation, exemplifies two current trends in comparative political research. The first is the move away from general, inclusive theories toward more nuanced and theoretically more modest treatments that take account of differing national and institutional circumstances. The second is the renewed interest and concern with the impact of institutions themselves and the correspondingly greater emphasis on what is conditional and contingent.

Scharpf's subject is the question of political control of the economy. His data concern the experiences of four European countries (Austria, West Germany, Sweden, and the United Kingdom) during the economic recession of the 1970s and, more specifically, the differing policy responses of the then social democratic governments of these countries to that crisis. Rejecting any notion of "crisis as fate," Scharpf points out that while there was a general worsening in the indicators of economic performance throughout Western Europe in the early 1970s, variation between the subsequent performances of the different countries also increased. His goal, then, is to account for this variation by selecting a group of countries governed by essentially similar social democratic governments that found themselves, at least initially, in similar economic circumstances. This "most similar systems" design enables him to assume a communality in the goals of each of the four governments and to assume, in particular, a shared commitment to full employment. This also allows him to assume that the subsequent variation in performance did not derive from differences in policy priorities. Rather, and of necessity, this variation must have derived from the differences in the capacity to govern and the ability to steer the economy through the crisis, which, in turn, depended at least in part on the differing institutional circumstances of each government.

This is the thesis that Scharpf sets out to argue in his overall analysis. His results are very convincing, and the end product is a genuinely excellent piece of work. Theoretically sophisticated, empirically grounded, and closely and persuasively reasoned, this book is a powerful example of how good comparative political analysis can be; and it constitutes one of the most important contributions to the rapidly growing literature on the political economy of contemporary Europe. We learn that politics and governments matter and that institutional structures are more than simply neutral arenas but have a capacity to condition and facilitate particular policy responses, while discouraging others. And along the way, we gain a valuable insight into the dynamics and constraints of macroeconomic policymaking, as well as a clear understanding of the parameters of the relations between trade unions and governments. There is, in short, a wealth of wisdom here, extending far beyond the ostensible limits of a study of four countries during a relatively short and distant period of time.

What also becomes clear from this study (albeit less directly) is that while governments in general matter a lot, the sheer importance of institutional structures also means that any individual government does not matter so much. This becomes evident when Scharpf addresses two of the key questions set by his study: under what institutional conditions unions will opt for a politically motivated strategy of conciliation with government, rather than an economically motivated strategy of confrontation and under what conditions governments can actually act on the basis of their chosen policies (pp. 169–237). In both cases, it seems, the die was pretty well cast well beforehand. In the case of union strategy, confrontation tended to be avoided in circumstances where there already was an established tradition of employer–labor cooperation and union–government cooperation and where there existed a cohesive union organizational structure. In the case of the government, the governing capacity itself tended to be enhanced when central government had direct control of the relevant monetary and fiscal instruments. Yet both sets of circumstances are clearly long-term legacies, quite beyond the scope of any government to alter substantially in the short term. In this sense, the fact that the Austrian social democratic governments managed to weather the crisis more effectively than their British counterparts may have more to do with their being Austrians than with their being better social democrats.

Scharpf concludes his study with an insightful account of the changing international economic circumstances of the 1980s and an analysis of the difficulties that confront social democratic ideologues in the new, non-Keynsian world: "Ideologically, and in public debate, at any rate, the hour of supply-side policies has always been the hour of employers and conservative parties" (p. 270). The continuing inability of contemporary social democrats to elaborate a convincing program that can tackle these new circumstances in the interests of their traditional constituencies clearly bodes ill for the future of the movement as a whole.

University of Leiden PETER MAIR

Radicalism and the Origins of the Vietnamese Revolution. By Hue-Tam Ho Tai. Cambridge: Harvard University Press, 1992. 325p. $34.95.

The Vietnamese have a profound consciousness of history—are, in fact, burdened by it (unlike the Americans). And, as their late prime minister Pham Van Dong once observed, they are incurable romantics about politics. Both of these cultural characteristics are amply demonstrated in this study of the principles, ideals, and operational methods of Vietnam's young radicals of the 1920s. The author is a leading ethnic Vietnamese woman in the American academy. Her field is twentieth-century Vietnamese intellectual thought, which locates her in that grey area where history overlaps political science.

This work has a distinct biographical orientation to it, a roll call beginning with the godfathers of the Vietnamese revolutionary movement (members of the turn-of-the-century Scholars movement who launched the intellectual revolt against traditionalism and French colonialism) and running through the nationalist and proto-Marxist figures of the next three decades. Tai's treatment is in the spirit of Woodrow Wilson's contention that history is but the lengthened shadow of great men. (Presumably, today he would add, "and great women"). A plethora of names appears here, although the radicals probably numbered less than two thousand of what today would be called dedicated activists led by not more than 250 of what today would be called cadres. Only a few of these are familiar to even reasonably well informed outsiders. Few survived to take a place in the history of the revolution that was to come, even as footnote.

It must have been an exhilarating time for these young political groundbreakers. And young they were, few past their midtwenties (Ho Chi Minh, in his early thirties, was their grand old man of revolution). They debated, plotted, published, organized, experimented with politics. They split into factions, regional groupings, and personal entourages. Most were elitist. Theirs was a cat-and-mouse game played with the French, encouraged by supporters in metropolitan France, and also, a darker game against the malignant French Indochina *colon*.

Their heritage was traditionalism with its strong Confucian, Buddhist, and Taoist influences that had created such a disciplined Spartan-like people; the millennia-long China threat of *han-hwa* (sinoization); a singular style of politics of clandestinism, whereby the Vietnamese dealt first with the Chinese, then with the French—all of which meant that by the 1920s, Vietnam was an ideal environment for political conspiracy.

The book is divided into eight chapters, treating the Confucian–French educational system, which shaped the young radicals and created the initial streams of revolutionary thought, more anarchial than anything else; the female radicals and feminization of the cause, about which virtually nothing has been written; the scandal sheets where young editors thundered against the relentless corruption of French colonialism; and three final chapters about the radical cause's taking on true political coherence.

Initially this contention pitted the nationalists (chiefly the Viet Nam Quoc Dan Dang, patterned after the Kuo Min Tang of China) against a proto-Marxist youth group, Thanh Nien, which metamorphosed into Trotskyist and Stalinist groups in the 1930s, finally to be captured by the latter. All streams were militantly anticolonial; but the Stalinists considered the others mere bourgeois revolutionaries, individualistic, non- or anticollectivist, and unwilling or unable to break completely with past value systems.

A common perception of academics viewing Vietnamese history, particularly those who come to it from a China background, is that accidents of history cheated Vietnam out of a vigorous intellectual tradition with national focus; that the literate class in Vietnam was always far smaller proportionally than in China; and that romanization of the Vietnamese language and other factors reduced Vietnamese access to China in ways that it did not in Japan and Korea. It is precisely this perspective that Tai seeks to challenge. Her second major thesis (as I read it) is that the spirit of the Vietnamese revolution was not chiefly a matter of borrowed westernism but was authentically Vietnamese, rooted deep in its culture. A third point is that this early radicalism was very much a young people's enterprise and, to a large extent, a young Vietnamese women's enterprise. (For those of us who have lived long in Vietnam this work reminds us just how really tough, mentally and physically, are Vietnam's women).

My chief quarrel with Professor Tai is that she tends to treat the Vietnamese revolution—indeed, Vietnam's political history in general—as a coherent entity and sharp-edged, as a clear progression of events, when, it seems to me, it is a great muddy river of events full of backwaters and eddies. The Vietnamese revolution, in particular, I see as a messy shaking out of history whose meaning is still being sorted out. Tai also seems excessively harsh on Confucianism, which is, after all, one of the great intellectual streams of thought: it produced a governing system that lasted longer than any other, save Egypt.

This is a bittersweet book. One puts it down with feelings of admiration for those young Vietnamese radicals of the 1920s but also with a sense of tragic waste. There were then (it is clear now) many roads to revolution in Vietnam. Almost any one of them would have been better than the one taken. Perhaps it was inevitable, as Tai seems to conclude. In any event, this is a fine book, commended to all.

University of California, Berkeley DOUGLAS PIKE

INTERNATIONAL RELATIONS

The Water's Edge and Beyond: Defining the Limits to Domestic Influence on United States Middle East Policy. By Mitchell Geoffrey Bard. New Brunswick: Transaction Books, 1991. 313p. $32.95.

A long list of books is available on U.S. policy toward Israel since its establishment in 1948. By several criteria, this one is distinctive. Most notably, perhaps, its approach to the subject is original and imaginative. The author applies several extant theories of the American foreign policy process to a series of case studies in Israeli–American relations. At the end of each, he evaluates the degree to which each theory is (or is not) useful in accounting for decisions taken by policymakers in Washington affecting the State of Israel. His approach, therefore, brings to bear an unusual combination of theoretical and eclectic insights in an investigation of the policymaking process.

Bard's study also stands out because he provides us with as objective and remarkably balanced an analysis of pro-Zionist lobbying activity and its impact upon American policy toward the Middle East as is possible for one of the most controversial and emotionally charged issues in American diplomacy. The author rejects the widely prevelant view that in most key respects, Washington's policy toward Israel has been "dictated" by the Zionist lobby. Time and again, he makes the point that a fundamental precondition for the lobby's success has been the underlying sympathy of the American people and their political leaders for the Israeli position—a factor that the Israeli government and its supporters in the United States have skillfully exploited. By contrast, this crucial reality has been lacking with respect to the "Arab lobby" (if, given its inherent disunity, this term can be meaningfully used to describe the Zionist lobby's main rival in the United States).

Another highly significant finding of Bard's investigation is that among several important variables contributing to the outcome of Zionist lobbying efforts, the ideological position of the president is crucial in determining the policy of the United States toward Israel. In his view, this is more influential than any other single factor. When, for example, the prior positions of the president and the pro-Zionist lobby happen to coincide, the latter can be expected to achieve a high level of success in attaining its stated goals. When they disagree, however, contrary to prevailing opinion about Zionist lobbying efforts, pro-Israeli groups are likely to have a relatively poor, or below-average, record in attaining their avowed goals. Bard also finds that as a rule, the Israeli cause tends to be supported more by Republican, than by Democratic, chief executives. In his view, this curious result can be accounted for largely by the fact that Democratic presidents and officials can and do take for granted widespread support for their party by Jewish voters.

Another distinctive contribution of Bard's study lies in focusing attention upon a phenomenon seldom dealt with in other investigations of the role of interest groups in the political process. This is the extent to which the president and his advisers often use pressure groups like the pro-Israel lobby to promote their own policy goals. Lobbying, Bard convincingly contends, involves something much more complex than merely a one-way attempt by interest groups to impose their views upon officials in Washington and public opinion. Much more frequently than even informed citizens of the foreign policy process are aware, for example, executive officials rely upon, or use, lobbies to achieve their own policy objectives.

Bard's study is clearly organized and interestingly written. The research relies upon both primary and secondary sources. Overall, he has given us an extremely valuable, informative, and provocative analysis of an important and recurrent issue in American foreign policy.

Louisiana State University CECIL V. CRABB, JR.

Command in Crisis: Four Case Studies. By Joseph F. Bouchard. New York: Columbia University Press, 1991. 325p. $45.00.

This is an important new addition to the literature on Cold War crisis decision making, even if such matters seem less pressing today in a world in which the chance of militarized disputes between great powers seems relatively remote. Bouchard is concerned with "the problems that can arise when using military force as a political instrument in crises" (p. ix). He focuses on a dependent variable of high policy relevance, namely, "the occurrence of inadvertant escalation in international crises" (pp. xiv, 188). What he really does in this book, however, is examine in detail the obverse side of the inadvertant escalation coin, that is, how the lid was kept on crises between the superpowers through various mechanisms of control.

In his effort to develop an explanation of this inadvertant escalation (or nonescalation) phenomenon, Bouchard adopts a comparative case study approach and examines the "interaction of military forces in crises and the impact of such interactions on crisis stability" (p. xiv). He selects four crises in which naval forces played a significant role: the 1958 Taiwan Strait Crisis, the 1962 Cuban Missile Crisis, and the 1967 and 1973 Arab–Israeli wars. Apart from this theoretical purpose in developing them, the case studies are useful summaries of events in these crises. The author also introduces new primary source material.

A significant limitation of the book is that it is difficult to generalize too far beyond these cases. Thus, one encounters problems extending the conclusions Bouchard draws from his detailed study of naval forces to ground, air, or joint forces that operate in very different contexts with different command-and-control mechanisms and levels of complexity. Moreover, his case studies are all about dyadic great power relations (i.e., U.S.–Soviet), so that his conclusions cannot be extended easily (and certainly not without caveat) to multipolar settings that also include small states (circumstances more characteristic of present-day world politics).

Bouchard observes that the U.S. Navy's institutional orientation is to put "peacetime missions" such as show

of force for political purposes as secondary to "wartime missions." Nevertheless, all four case studies illustrate use of naval forces for "political signaling or related political functions" quite apart from their wartime use (p. xxxi). In fact, it is peacetime use of the navy short of war that is the more common use of force, which accounts for much of the success the U.S. Navy has had politically in getting budgets to sustain its "naval presence" and "power projection" capabilities.

The author's conclusion that crises can be controlled politically—that "tactical-level military interaction normally will not escalate to war without a deliberate decision by national leaders"—is reassuring, at least for the two great power confrontations he examines. Certainly, the situation grows more complex as the number of powers involved in a crisis increases, which makes maintenance of political control more difficult. Moreover, the stability that obtained in U.S.–Soviet crises was also a result of having "tacit rules of crisis behavior and communications" that had become "well developed" over time (pp. 198, 222). Whether even the relatively simple dyadic structure of conflict that governed U.S.–Soviet Cold War relations would necessarily be so stable without these norms is a central question.

The need for maintaining political control over military forces is never more important than it is in crises. Bouchard observes, "In an acute crisis, in which strategic or tactical interactions between the two sides have become decoupled from political-level interactions, the security dilemma can trigger an escalatory spiral at the strategic or tactical levels of interaction" (p. 198). Beyond tacit rules of crisis behavior and communications, escalation-inhibiting factors Bouchard observed in each case included military "prudence," caution and restraint by commanders, national leaders' structuring the tactical environment to dampen military interactions, accurate and timely tactical intelligence, and "double-checking the accuracy of initial reports of military incidents" (ibid.).

Although Bouchard acknowledges that they may not always hold, he nevertheless concludes that "these factors tend to moderate the intensity of tactical-level interactions, prevent armed clashes from occurring, and produce disengagement rather than escalation when clashes do occur" (p. 198). This is an important conclusion, so long as one keeps in mind the danger of extending it to multipolar and other settings not considered by the author. Seeing crisis stability in part as a function of relations between tactical and political levels on each side, as well as relations between adversaries, is an important insight Bouchard develops.

Notwithstanding the positive conclusions from the four cases he studies, Bouchard still maintains that naval forces "have serious crisis stability problems"; in a "severe" crisis, for example, he observes that "tension between crisis management and naval strategy objectives would be acute" (p. 196). Indeed, as is well known, President Kennedy recognized this problem explicitly as he and Secretary of Defense McNamara tried to maintain control of naval forces in their confrontation with Soviet counterparts during the Cuban Missile Crisis. Add to this imperfect information and the uncertainty or Clausewitzian "fog" that hangs over the battle area, and meaningful control by the political leadership is elusive: "National leaders must expect that at least some things will go wrong when they employ military forces in crises" (p. 221).

The case studies examine the relation between decision making at tactical and political levels in what the author refers to as "stratified interaction" (pp. 36–56, 222). Understanding crisis decision making as being "stratified," Bouchard observes that "direct control" by political leaders was only "exercised selectively and to a limited degree"; instead, political leaders relied primarily "on mechanisms of indirect control' such as predetermined terms of reference that specified limits to guide decision making by tactical commanders (p. 221). Bouchard goes beyond such observations to argue that national leaders should "focus on communicating with the other side in order to avoid misperception" and other miscommunication, rather than trying "to seize direct control of tactical decisions"; indeed, to Bouchard, "the greatest danger of a crisis escalating to war may well arise from decision making at the political level of interaction, rather than from decision making at the tactical level of interaction" (ibid.).

The effectiveness of indirect mechanisms of control in crises between the Cold War superpowers was heavily dependent upon the professional character of military commanders—seeing their political subordination as being legitimate. Such an assumption about all the militaries of great and lesser powers in the complex, multipolar world politics of coming decades is difficult to sustain.

In the final analysis, the author is careful not to overextend the important arguments he makes. His conclusions from the structured comparative case studies are really hypotheses worth testing or examining under the different conditions and contexts that will likely apply in coming decades. After all, the relative simplicity and stability of bipolar relations was something of an aberration from the historically more common multipolar mode. We need to know more about crisis decision making in the latter context. But that is not a fault of this book, which stands on its own merits.

University of Colorado, Colorado Springs PAUL RICHARD VIOTTI

The Politics of Negotiation: America's Dealings with Allies, Adversaries, and Friends. By Linda P. Brady. Chapel Hill: University of North Carolina Press, 1991. 269p. $32.50 cloth, $12.95 paper.

Reviewers commonly choose a classic as a standard of comparison if they wish to point up shortcomings in a volume. I wish to use a classic—Fred Ikle's *How Nations Negotiate* (1964)—to identify the limited but, nonetheless, significant contributions made by Brady's monograph.

The longevity of Fred Ikle's masterpiece is attested by its continuing sales. Nevertheless, its concerns relate narrowly to the Cold War as seen from the perspective of the early 1960s. Scholars have welcomed several specialized but useful supplements. Linda Brady's book on how the United States negotiated military capabilities and operations in the 1980s is such a valuable supplement. It shares with Ikle's classic an admirable clarity of presentation and unwavering focus on a few, simply (but never simplistically) expressed factors and heuristics.

Brady's book is methodologically narrower than Ikle's. After an introduction, the book presents what is,

in effect, a matrix. Each cell offers the author's answer to one question about one case. Six sets of questions range from the predictable (What factors influenced the formulation of negotiating positions?) to the vital but often neglected (What additional action was required for implementation of the agreement?). The negotiations treated are wartime host nation support with West Germany in the early 1980s; the Dual Track long-range theater nuclear forces deployment negotiations of 1979; Mutual and Balanced Force Reductions (MBFR) 1973–89; Intermediate-Range Nuclear Forces (INF) 1980–87; logistical support for the Persian Gulf in the early 1980s; and NATO out-of-area activities in the early 1980s. Since these vary significantly in duration, internal complexity, and relation to other negotiations, the reader would be helped had the author given more attention to her constitution of a "case."

Scholars of contemporary international negotiation will not find much new regarding negotiating theory. Nor will close observers of the public record of these cases often be surprised by the facts presented. The contribution of the book derives from meticulously comparative treatment, where variations of context and process are itemized, and from which are drawn some hypotheses about future negotiations.

The book is honest to its title: without reservation it is a discussion of politics. Brady ignores economic models, even when they might underscore her conclusions. For instance, she observes in the Preface that the most difficult negotiations occur between friends. This claim begs for a baseline of comparison: most difficult to conclude? most inefficient in process? most ineffective in resolving the problems that motivated negotiations? most unlikely to reach mutually beneficial and lasting agreements?

Scholars whose interests transcend strictly political factors might object to a few features of the volume's organization and to a few of its theses. For instance, Brady divides the six cases into three main types, said to be characterized by dealings with *allies*, *adversaries*, and *friends* and *mixed relationships*. There is another perspective which maintains that all negotiations that actually get under way must either be responding to their participants' recognitions of mixed motive situations or be motivated solely by the pursuit of side-effects (in Ikle's terms). Even within her carefully maintained focus on politics, Brady demonstrates a near aversion to economic phenomena and to the economist's methods. This is surprising because at several points she relates decision calculi that explicitly incorporate anticipations of other parties' responses, and she often relates participants' sophisticated assessments of trade-offs.

As an experienced Pentagon insider, Brady's own policy analysis and writing must have sensitized her to often-conflicting requirements for advocacy of policy, on the one hand, and its ex post justification, on the other. That same practical experience must have conveyed the often overdetermined "logics" by which policies emerge. Her first question about each case is, "What prompted the United States . . . to enter into negotiations?" Typically, narrowly rational, means–ends thinking is ascribed. What she seems to report is an answer to a subtly different question, namely, "What is the best case that could have been made for the United States to enter into negotiations?" The possibility that there were a multiplicity of converging pressures and complementary (if not entirely reinforcing) reasons is often overlooked. At several points in the treatment of the MBFR and INF talks, she appears to accept as unproblematic the existence of a "U.S. position" and makes observations rather casually about shifts in that position.

Ikle aimed his comprehensive analysis at the improvement of negotiating practice. Brady's final chapter, "Implications for Policy," is quite thin. I think that the case analyses plus the author's experience in the Office of the Secretary of Defense empower her to hazard far more in the way of "implications," even if they were modestly labeled "suggestions." In only one of Brady's cases is the U.S.–Soviet competition not central. But her analytic framework—succinctly conveyed as six sets of questions—is broadly applicable. The analysis is germane for understanding post–Cold War negotiations.

Syracuse University JAMES P. BENNETT

Learning in U. S. and Soviet Foreign Policy. Edited by George W. Breslauer and Philip E. Tetlock. Boulder: Westview, 1991. 881p. $75.00 cloth, $29.95 paper.

This timely collection defines *learning* as a rigorous concept and applies it to return engagements with similar issues in American and Soviet foreign policy across the past 45 years. It defines a new level for studies of decision-making, adding an historical dimension to the Allison tradition of analyzing single-decision cycles. It is likely to define key lines of investigation in international relations during the 1990s.

Karl Deutsch pioneered the study of learning with *The Nerves of Government* (1966). In the late 1970s the National Science Foundation supported an initiative for a new, interdisciplinary field along these lines. Now, with additional decades of data, it is possible to place Deutsch's inquiry on a solid evidentiary foundation. The framework will be especially helpful to study how political leaders respond to, and shape, new and changing international realities.

The volume represents a major investment, with 19 contributors and chapters of unusually high quality. This partly reflects its sponsorship by the National Academy of Sciences and foundation support that afforded a distinguished advisory committee, discussions among participants (beginning in the spring of 1987), and two editors of unusual ability and commitment who challenged their contributors to produce an integrated volume.

The volume is divided into four parts. An initial section discusses conceptual issues (contributions by Tetlock and Haas being especially notable); three concluding chapters partially integrate the volume. Between, two sections offer a well-planned series of case studies by experts in each field: seven on learning in American policy toward arms control, détente, Western Europe, China, the Middle East, Third World intervention, and nuclear nonproliferation since World War II; then a parallel series of studies of Soviet policy. Leading scholars have written the chapters, which include extensive references and provide sufficient space to develop complex themes.

The editors devote the conceptual chapters primarily to definitional issues, apparently in an effort to achieve rigor and clarity about the nature of their inquiry. Several disagreements emerge: approximately, the con-

tributors divide into two camps. Some (like Tetlock) define learning broadly (and see more of it). Others (like Haas) would require an extraordinary set of changes, and substantial evidence, to infer that learning has occurred (i.e., as opposed to mere *adaptation*). As one of the editors (Breslauer) argues, a variety of definitions and different calibrations of thresholds probably matters little, so long as one is explicit about the dependent variable and processes under investigation.

But I would add a caution. Nonlearning (or mere adaptation) is not the same phenomenon as statistical randomness or an equivalent (an established baseline of governments); thus, Haas's null hypothesis is not a deep analysis. The sophisticated, surly, and cynical viewpoint that "governments don't learn (they merely *adapt*) because that is the *nature* of governments" would be as underspecified and unfortunate a causal explanation as Morgenthau's now-problematic claim that "objective laws" sustain the realist behavior of states.

Presenting definitions, Tetlock boldly argues that concepts of differentiation and integration from cognitive psychology are always correlates of policy learning. This hypothesis predicts that identical "Tetlock shifts" (beginner-to-expert) will occur in both international and domestic policies. When the editors conclude that both Soviet and American leaders "have learned that many indigenous processes in the Third World are out of the direct control of either superpower" (p. 13), one imagines a series of doctoral dissertations and books testing this theory against data drawn from domestic policy and implementation problems of the Great Society.

Can the theoretical apparatus of this book fully explain the end of the Cold War? Possibly not, because the authors' image of learning appears closer to learning in academic or scientific life. But by the beginning of the 1990s, one did not simply see reality more clearly, as a scientist. The nature of a socially constructed reality and its supporting emotional consensus changed for all participants—as if one were living inside a dream or a nightmare and awakened.

For example, viewing objective evidence, it was arguably crazier to have stopped the Cold War abruptly than to have started it. By the late 1980s, both sides had vast nuclear arsenals explicitly targeted at one another; 40 years of global conflict had shaped the relationship and given evidence to confirm the hostile images of ideological formulae; the American president had called the Soviets an "evil empire" and was engaged in the largest arms race buildup in history; both sides were engaged in open support for proxy armies (Nicaragua, Afghanistan) in one another's spheres of influence. But in the late 1940s, against a background of wartime cooperation, no objective evidence of such magnitude and persuasiveness was available to justify the beginning of the Cold War.

Thus, if we contrast the two end points, was it not profoundly irrational and dangerous for the Soviet leaders to call off the Cold War? Not just to reach an arms control agreement (this might be predicted by realism and many other theories) but to dissolve the Warsaw Pact? To allow the breakup of the entire Soviet state? To welcome Western investors with open arms? Emotions can either block or support new learning; and one suspects that at a gut level, Soviet leaders decided to *trust* the West. And if so, this deeper intervening shift in emotional consensus, more consequential than the "objective" evidence of American arms buildups, is a causal mechanism whose nature, amidst the discussion of cognitive changes, remains a mystery.

With Tetlock's definition of differentiation and integration, the reader is prepared to read a dazzling tour de force in the last chapter, a crisp, refined, detailed, and brilliantly integrated outline of the causal variables and theories of the previous eight hundred pages. Yet, although Breslauer's chapter (and Weber's) are thoughtful and rigorous, they only begin to mine the volume. We do not know the box score of existing theories. It is left to future researchers (or graduate students) to inventory the new boxes, subboxes, and arrows implicit in this de facto update of Deutsch. The volume is a wonderful start—and by its own conception, the field has more work ahead.

University of Toronto LLOYD S. ETHEREDGE

Systems in Crisis: New Imperatives of High Politics at Century's End. By Charles F. Doran. New York: Cambridge University Press, 1991. 294p. $49.50.

Charles F. Doran has written a wide-ranging and incisive book that argues persuasively that power cycle theory is a potent analytic tool of analysis for understanding international relations in general and the outbreak of major war in particular. The book is ambitious, and the result something of a triumph. The author constructs a powerful case for claiming that the key to understanding the behavior of great powers (especially the sort of irrational behavior that commonly precedes cataclysmic war) is the trauma associated with widening gaps between national interests (or roles) and power (relative, not absolute), trauma which is the consequence of the power cycle. He argues that when a number of such traumas occur simultaneously, as they must owing to systemic interdependencies, the probability of major war increases dramatically.

Doran builds his argument meticulously. In part 1, he lays out the power cycle model—its essential concepts, their operationalization, and the data used to measure them—and places it clearly in its metatheoretical context. (There is also a brief but useful mathematical appendix). Although firmly embedded in the power politics tradition, power cycle theory (unlike balance of power) is dynamic and focuses on long-term change itself. One may quibble with the way Doran chooses to measure power: for example, the absence of contextual factors. But his combining of economic and military variables is a reasonable compromise that avoids the biases that flow from overemphasizing one or the other. The author recognizes the greater difficulty in precisely measuring international role, with all of its normative baggage, but his elaboration of this inherently slippery concept through historical and case description is entirely satisfactory.

Part 2 makes a deceptively simple (and highly effective) shift from the state and dyadic levels to system-level analysis. Doran shows how structural disequilibrium flows from a situation in which a number of states simultaneously experience "critical points" in their power cycles (such points being defined as changes in the slope or direction of the cycle that negate foreign policy expectations). The author's wedding of empirical

data and historical analysis in making his case is unusually careful and illuminating. Indeed, his treatment of the causes of World Wars I and II illuminates geniune historical anomalies and shows the utility of political science for traditional historians.

Doran argues that such shifts are "perceived" by national leaders and publics, who are shocked by them and may respond in unpredictable ways. The link between structure and perception is necessary in order to avoid the charge of structural determinism and so prepare the reader for the prescriptive analysis to come. However, although the argument is plausible, there is tension between the claim that shifts are perceived and the central role played by surprise in the analysis. Moreover, the process by which perception takes place remains nebulous. Nevertheless, Doran ingeniously manages to open the national "black box" and to introduce and justify the splendid prescriptive essay that is to follow in part 3.

Parts 3 and 4 will assure that this book becomes required reading for graduate seminars in international relations theory. Having avoided the sort of frustrating determinism that characterizes long-cycle and hegemonic theories (which the author addresses), as well as the static bias of much of power theory, and having wisely ignored the putative "levels-of-analysis" problem, Doran examines at length the prerequisites of world order and the prospects for avoiding cataclysmic war. He then applies a similarly provocative analysis to contemporary world politics, especially to the implications of American and Soviet decline and Japanese and Chinese ascent (though from different points on their respective power cycles).

The examination of the recent past and future alternatives of the United States and the Soviet Union highlights the critical importance of linking system-level structural factors in order to make sense of international relations. Unfortunately, though imaginative, it also reveals the limits of power cycle theory in dealing with the domestic arena. These limits are especially evident in the (dated) discussion of probable future alternatives for the Soviet state and the causes of American behavior in Vietnam.

If Doran's effort to examine policy choices suffers from some of the drawbacks inherent in the power tradition, his willingness to tease normative implications concerning "justice" and "legitimate" behavior from the power cycle model constitutes an important contribution to the discipline. His claims regarding the relationship between power and justice and the obligations of leaders to design policies that will narrow status–power discrepancies in rising states are not uncontroversial. Nevertheless, the effort is an important and welcome step toward relinking empirical and normative analyses in international relations theory.

In short, whatever one's view of the statist and power politics tradition of which this book is a part, it is difficult not to be impressed and enthused by its breadth. It is among the most important theoretical and empirical additions to the causes-of-war literature of the past decade. More importantly, it suggests that there is life yet in what many observers believe is the increasingly moribund realist–neorealist tradition.

Iowa State University RICHARD W. MANSBACH

Moscow and the Middle East: Soviet Policy since the Invasion of Afghanistan. By Robert O. Freedman. New York: Cambridge University Press, 1991. 426p. $59.50.

Soviet Policy towards Syria since 1970. By Efraim Karsh. New York: St. Martin's, 1991. 235p. $59.95.

The USSR and Iraq: The Soviet Quest for Influence. By Oles M. Smolansky. Durham: Duke University Press, 1991. 347p. $55.00 cloth, $24.95 paper.

The political and economic changes associated with the tenure of Mikhail S. Gorbachev as president of the Soviet Union have had substantial impact on Soviet foreign policy. *New thinking* (as the reinterpretation of Soviet foreign interests and behavior is termed) has resulted in major arms control agreements and a substantial reduction of the Soviet military presence in Central Europe, the liberation of Soviet client–states in Eastern Europe, the withdrawal of Soviet troops from Afghanistan, a dramatic reduction in Soviet military support for erstwhile ally–clients throughout the Third World, and Soviet support for the U.S.-led military operation in the Persian Gulf. As this partial list indicates, Soviet policy in the Middle East has been affected by policy shifts as significant as those elsewhere throughout the Third World.

Three recently published book by established analysts of Soviet policy in the Middle East provide both background for, and an evaluation of, recent Soviet policy. Taken together, these books provide detailed coverage of key elements of Soviet policy throughout the region.

Over the past 20 years Robert O. Freedman has been among the most important and most prolific of Western writers on Soviet policy in the Middle East. His most recent *Moscow and the Middle East*, brings the story he first told in *Soviet Policy Toward the Middle East Since 1970* up to the Soviet withdrawal from Afghanistan in early 1989. As those familiar with his earlier writings would expect, his new study provides a detailed chronology of the development of Soviet policy in the key issue areas of importance throughout the region. Freedman weaves together a treatment of domestic developments within the Middle East, his interpretation of Soviet objectives, and the interaction of Soviet and other external actors to produce a comprehensive picture of Soviet policy toward the region during the 1980s. This book (like all of Freedman's work) is based on a wealth of documentation.

Freedman concludes that as of early 1989 when the Soviets withdrew their troops from Afghanistan, Gorbachev's policies toward the Middle East were characterized more by continuity than by dramatic changes associated with new thinking. The areas of continuity included (1) the Soviet plan for resolution of the Arab–Israeli conflict, (2) the Soviet unwillingness to use military force to challenge either Israeli or U.S. activities in the region, (3) the inability of the Soviet Union to effect reconciliation between Syria and the leaders of both the PLO and Iraq, (4) the policy of trying to balance between Iran and Iraq during the ongoing war, and (5) the Soviet disinformation campaign targeted against the United States (p. 323).

Freedman does find new elements of Soviet policy in the increased emphasis upon the role of the United Nations, the efforts to normalize relations with both Egypt and Israel, and the overall more flexible and active nature of Soviet policy in the region.

While Freedman provides the reader with a *tour d'horizon*, the other two authors provide detailed country studies. For almost two decades the Soviet relationship with Syria was a centerpiece in its overall policy toward the Middle East. In a perceptive analysis entitled *Soviet Policy Toward Syria since 1970*, Efraim Karsh examines the nature of Soviet policy toward Syria, including the basic objectives, the instruments employed in pursuing those objectives, and the costs and gains associated with that policy.

Karsh argues that the basic objective of Soviet policy remained essentially unchanged, namely, the establishment and maintenance of regional stability. Karsh begins by arguing that the Soviet–Syrian relationship has been neither one of patron and client nor one in which the smaller has been able to control the policies of the larger. He argues that although the Soviet Union has provided substantial military support to Syria, it has recognized the limitations of military power in the accomplishment of its objectives, especially during times of peace. After establishing the initial framework for his analysis, Karsh provides a chronological examination of the Soviet–Syrian relationship from approximately 1970 until the Gorbachev era. He notes the great concern among the Syrians for the negative implications of "new thinking" in the foreign policy area as it emerged under Gorbachev after 1985.

As Karsh argues, Gorbachev has recognized the legitimate interests of the West in the region and has emphasized the need for a negotiated settlement of the central issues that divide Israel from its Arab neighbors. The overriding objective of Soviet policy in the area from Brezhnev to Gorbachev has been the attainment and preservation of stability. While Gorbachev's approach to the attainment of these objectives has shifted, he has not abandoned the core goal of Soviet policy over the past two decades. This conclusion coincides quite well with the basic argument presented by Freedman.

Oles M. Smolansky's *The USSR and Iraq* is the first major study of Soviet–Iraqi relations to appear in English. Rather than providing a chronological survey of the evolution of relations, Smolansky focuses on specific aspects of that relationship. He does begin with a brief historical overview of relations from 1958 up to 1980 and the outbreak of the Iran–Iraq war. Smolansky argues that relations were characterized by both an overlap of short-term objectives and also by substantial disagreements on issues of major importance. Smolansky then examines several issues that exercise major influence on the relationship. During the first of these, the nationalization of the Iraqi oil industry in the early 1970s, the Soviet Union was strongly supportive, for Soviet and Iraqi interest tended to overlap. The second issue, the Kurdish question, resulted in periodic divergence between the Soviet Union and Iraq. However, by the mid-1970s and the culmination of the governmental suppression of the Kurdish movement, Moscow shifted its support back to the government of Saddam Hussein. The issue of the Iraqi communist party was one that also resulted in a periodic deterioration in Soviet–Iraqi relations. Though the Soviets, on occasion, virtually abandoned their communist allies, by the end of the 1970s when Saddam Hussein executed a substantial number of Iraqi communists, Soviet–Iraqi relations suffered seriously.

Besides issues of Iraqi domestic politics that exercised an influence on the relationship, issues concerning regional problems—Iraq's relations with Arab countries, as well as with Iran—were important. These issues culminated in the Iran–Iraq War of the 1980s, during which the Soviet Union greatly reduced its support for its Arab ally, either asserting a neutral position or on occasion even "tilting" in support of Iran.

The USSR and Iraq makes a significant contribution to our understanding of Soviet relations with an important Arab country. What emerges is a clear picture of a relationship characterized almost as much by divergence and conflict as it was by agreement on key issues of domestic and foreign policy. At no time could Iraq be viewed as a client state of Moscow. Despite the strengths of this book—and they are overriding in any overall assessment—one could wish that greater attention had been given to aspects of Soviet–Iraqi relations during the 1980s not directly associated with the war against Iran. Moreover, one might question the author's decision to exclude any attention to Iraq's role within the Arab–Israeli conflict and also the Soviet role in Iraqi–Syrian relations.

As all three of these well-written books demonstrate, Soviet policy throughout the Middle East has focused on efforts to stabilize conflict and protect Soviet interests. However, despite the infusion of massive resources, the Soviet Union generally was not able to impose its will on client states. In sum, the Soviet ability to affect events througout the region was very limited.

University of Illinois, Urbana–Champaign ROGER E. KANET

The Political Economy of Third World Intervention: Mines, Money, and U.S. Policy in the Congo Crisis. By David Gibbs. Chicago: University of Chicago Press, 1991. 264p. $29.95.

In a provocative study of U.S. foreign policy in the Congo crisis of 1960–65, David Gibbs provides a convincing reinterpretation of the motivations behind the interventionist policies of the Eisenhower and Kennedy administrations. Gibbs argues that foreign investors helped determine U.S. policy by influencing bureaucratic decision making in each administration. In the Eisenhower administration, key executive branch officials including the secretaries of State and Defense were close to business groups with investments in Katanga Province. According to Gibbs, the economic ties of Eisenhower officials to U.S. investors in Katanga conditioned their support for the attempted Katanga secession of 1960. In contrast, business groups close to the Kennedy administration were divided into pro-Katanga and anti-Katanga factions. The anti-Katanga faction consisted of business groups (e.g., Chase Manhattan Bank) who opposed the Katanga secession due to their economic competition with Belgian companies. These investors sought the establishment of a stable central government in order to facilitate increased U.S. investment in the Congo and to weaken Belgian economic interests concentrated in Katanga Province. Gibbs argues that the divisions within the Kennedy administration between pro-Katanga and anti-Katanga business factions explain the bureaucratic rivalry over whether to support the Katanga secession.

Drawing on a wealth of primary documents, Gibbs challenges many of the traditional claims made by scholars by U.S. foreign policy. He argues that U.S. policy

toward the Congo cannot be fully explained in realist or anticommunist terms. Traditional accounts of U.S. intervention in the Congo focus on Patrice Lumumba's communist ties as the primary factor motivating U.S. policy. Gibbs concludes that these accounts are misleading for two important reasons. First, the anticommunist argument fails to explain why the Eisenhower administration supported the Katanga secession, given its destabilizing effects on the Congo and the degree to which it motivated Lumumba to seek aid from the Soviet Union. The Eisenhower administration could have opposed Lumumba without supporting the Katanga secession. Second, there is ample evidence that Lumumba was not a communist and, in fact, sought aid from the United States and the United Nations several weeks before he accepted Soviet assistance. In fact, Lumumba received aid from the right-wing Liberal party of Belgium in addition to aid from the Belgian communist party (p. 99).

These factors lead Gibbs to advance a business conflict model to better explain U.S. policy toward the Congo. The model puts forward two propositions concerning U.S. foreign policy: that business groups are the most influential pressure groups in the foreign policy process and that business groups are divided by diverse sectoral economic interests. This interpretation challenges bureaucratic accounts of U.S. foreign policy which focus on the institutional dynamics of foreign policy decision making. The author provides an additional counterpoint to traditional pluralist and Marxist accounts of U.S. policy. Pluralists underestimate the importance of policymakers' informal business connections while Marxists exaggerate the unity of business groups in the political process.

However, Gibbs is careful to note that the business conflict model is not always the best model for explaining U.S. intervention. He insists that the model is insufficient in explaining the eventual decision of the Kennedy administration to oppose the Katanga secession. Here, the realist or statist model proves useful in accounting for the triumph of antisecessionist business interests close to the Kennedy government. Kennedy officials came to oppose the Katanga secession due to the pervasive fear that it would destabilize the country and lead to increased Soviet and Chinese influence in the region. This Cold War consideration allowed business interests opposed to the secession to win the arguments with their pro-Katanga rivals. In this particular instance, the business conflict and realist models are complementary in their ability to explain U.S. foreign policy toward the Congo.

This is a first-rate example of careful archival research and nuanced scholarship. Gibbs supports his claims with extensive primary documentation, much of it recently declassified. However, the theoretical framework could be enhanced by considering in more detail how the business conflict model might be compatible with other approaches to foreign policy. For example, what does the model have to say about the institutional variables which might influence whether or not one business group triumphs over another in the foreign policy process? Do business groups attain influence solely as a result of their informal connections to policymakers (as Gibbs suggests), or is such influence conditioned by the nature of executive–congressional interaction? Also, to what extent can domestic opposition groups influence U.S. policy by pressuring business and political elites to modify or alter their positions (as during the Vietnam War)? To his credit, Gibbs considers some of these questions in the last chapter of the book, suggesting both cases where the model could be usefully applied and cases where it would be inadequate.

Florida International University RONALD W. COX

The Lessons of History. By Michael Howard. New Haven: Yale University Press, 1991. 217p. $27.50.

In this book distinguished military historian Michael Howard collects a set of articles, essays, and lectures mostly written during the course of the tumultuous 1980s. The perspective in these urbane, thoughtful pieces is one that can at once beguile and frustrate political scientists. On the one hand, Howard is quick to note that historians "find it as difficult as anyone else to distinguish between the significant and the transitory" (p. 2) and urges "humility" in finding historical palliatives to apply to the present (p. 5). On the other hand, he often finds himself uttering generalizations of the most sweeping sort. Since, however, the generalizations emerge after a lifelong study of war and of society and since they are filtered through a mind that is reflective, agile, analytic, and careful, they require consideration and respect.

Howard's chief generalization concerns historical patterns of war, and many are likely to find his conclusions to be wildly optimistic. He expects that war will persist among "undeveloped" societies and suggests that civil war might still occur within both undeveloped and developed ones. Nevertheless, he believes it "quite possible that war in the sense of major, organised armed conflict between highly developed societies may not recur, and that a stable framework for international order will become firmly established" (p. 176). His more fleeting prognoses on economics (not his strong suit) suggest utter gloom: revolutionary socialism, he observes, "is disastrously inadequate for long-term economic development," while its alternative, free-market capitalism, creates "horrifying inequalities and widespread corruption, which in turn inhibit economic growth" (p. 5). Apparently, nothing works.

Howard's intriguing conclusion that major war may have become obsolete derives chiefly from a set of observations about the way people in the developed world have changed their attitudes toward war. At one time, he notes, they were organized into "warrior societies" in which warfare was seen to be "the noblest destiny of mankind." This was changed by industrialization, he argues, which "ultimately produces very unwarlike societies dedicated to material welfare rather than heroic achievement," with the result that "today everyone in developed societies belongs to 'the peace movement', even those who, in the name of stability, are most zealously building up their national armaments" (pp. 167, 175–76).

The main problem for this generalization (as Howard is fully aware) is that industrialization spoke with a forked tongue. If it encouraged some people to abandon the war spirit, it apparently propelled others to fall, if anything, more fully in love with the institution. Some of the finest pieces in the book, in fact, trace the persistence—even the rise—of a militaristic spirit that became wedded to a fierce and expansionistic nationalist

impetus as industrialization came to Europe in the nineteenth century. And of course, in the next century industrialized nations fought two of the greatest wars in history. The lessons of history, then, suggest that industrialization can inspire bellicose as much as pacifist tendencies.

Howard never really provides much of an explanation for how or why industrialization must inevitably lead to an antimilitary spirit. The horrors and holocausts that accompanied industrialization he simply attributes to the "growing pains" of industrial societies (p. 1).

For a man so keenly attuned to the force of ideas in history, Howard seems unnecessarily anxious to come up with a mechanistic, necessitarian explanation for attitude change. People in the developed world may have become disillusioned with war as they had become disillusioned with slavery. Because of unpleasant experiences with it and because of the machinations of skilled propagandists, they changed their minds about what they most valued and came increasingly to view war—at least war in the developed world—as immoral, uncivilized, disgusting, futile (particularly economically), and rather ridiculous. It is not at all clear they needed industrialization to come to that view.

University of Rochester JOHN MUELLER

Code of Peace: Ethics and Security in the World of the Warlord States. By Dorothy V. Jones. Chicago: University of Chicago Press, 1991. 208p. $24.95.

In *The Twenty Years' Crisis, 1919–1939* (1949), E. H. Carr argued that idealism and realism play vital interactive roles in the theory and practice of world politics. Scholarship in the postwar era has tended to ignore this interaction, suggesting that we live in a realist world in which ethical principles have little place. *Code of Peace* offers a lucid and persuasive challenge to this position that indirectly recalls Carr's influential study.

Code of Peace describes how a universal set of ethical principles has evolved in the twentieth century. This code expresses an image of an ideal world of peace, justice, and cooperation and serves as a standard against which the behavior of states can be measured and toward which their actions can be directed. Forged by leaders acutely aware of the appeal of short-term national gains, it is the product of their reflections on state relations and their attempts to develop ethical principles and viable institutions to guide these relations. States do not always respect this code, but they recognize it as the essential basis of a just and peaceful world.

Dorothy Jones's argument is presented in six chapters that take the reader from the turn of the century to 1989. The self-interested "world of warlord states" (p. 8) was shocked by World War I into developing institutions to promote peace. Although the most familiar of these, the League of Nations, is best remembered for its failures, her discussion of the much-neglected interwar period notes several successes. This "experimental" period was hampered by conflicting approaches to conflict resolution; an unshakable commitment to state sovereignty; and uncertainty about how to address revolutionary violence, secessionist movements, and human rights violations. But it produced a baseline against which later efforts could be measured; some of its institutions, such as the International Labor Organization, succeeded; and states gained experience in cooperation and problem solving. Unfortunately, its principal strategy for peace, which relied on collective security and public opinion, failed dramatically.

After World War II, elements of the interwar framework for peace were revised and refined. International conferences focused on identifying fundamental principles, as well as developing viable institutions. New states later extended these principles, most notably through the inclusion of an economic conception of social justice. The notion that states existed to serve the needs of people was expressed through conventions on human rights. Thus, over a period of 70 years, nine principles were worked out and repeatedly affirmed that reflect a universal desire for justice and peace. Two auxiliary principles, concerning distributive justice and protection of the environment, have recently received attention and may become part of the code. Jones concludes, "In the short term, the states' efforts may seem to make no difference at all, but in the long perspective of history, this code of international ethics stands as a possible bridge between the world the warlords have made and the world the warlords have dreamed" (p. 166).

Code of Peace was awarded the 1991 Lionel Gelber Prize. It is a model piece of scholarship that includes a clear and rigorously researched argument, several useful appendices (including a list of the 79 conventions and agreements that form the basis of Jones's code), and a bibliographic essay. It will be of special interest to those who study international law, organizations, and ethics but also offers insights into perennial debates over realism and idealism, independence and interdependence, and theory and practice. The text is suitable for teaching purposes and provides a good framework for discussing emerging agreements in areas such as development rights and environmental policies.

The book has only minor weaknesses. Jones's argument that the code of peace transcends cultural differences and is therefore universal is rather cursory. The extent to which the priority, meaning, and commitment given to these principles vary across cultural and other lines requires further attention. The state-centric focus of this study may limit its value in a world increasingly shaped by diverse nonstate actors. The contention that the code of peace is significantly more than the hollow rhetoric of states remains disputable. While it is noteworthy that states have expended great efforts to construct this code, a more cynical mind might emphasize the variety of violence manifest in the postwar era, attributing cooperation and the absence of war in some regions to self-interested cost–benefit calculations. Nonetheless, Jones's central claim that states have gradually created a universal code of ethical principles and a complex of supportive institutional arrangements is well argued. Her study stands as an excellent piece of scholarship that will reward readers throughout the field.

University of British Columbia RICHARD A. MATTHEW

Trappings of Power: Ballistic Missiles in the Third World. By Janne E. Nolan. Washington: Brookings Institution, 1991. 209p. $29.95 cloth, $10.95 paper.

The world watched transfixed as Saddam Hussein lobbed Scud missiles toward Israel and Saudi Arabia during the Gulf War. The military impact of the Iraqi

Scuds was small, but the political and psychological impact of Iraq's ability to throw inaccurate ballistic missiles at her neighbors was great. The issue of Third World ballistic missile proliferation has remained salient. The United States recently threatened to intercept a North Korean freighter suspected of carrying Scud–C missiles to the Middle East. In the wake of these events, Janne Nolan's balanced and informative book is an excellent introduction to the issue of ballistic missile proliferation, an issue that is having a major impact on the global distribution of power.

That Third World countries want ballistic missiles should come as no surprise. Nolan rightly notes, "Given an international system in which military prowess has been a leading determinant of national stature, the efforts of third world nations to acquire the most advanced weapons available in the international market should come as no surprise" (p. 16). Nor should it come as a surprise, she says, that Third World countries have been able to acquire ballistic missile technology. She outlines three strategies that Third World countries have pursued to acquire ballistic missiles.

Some countries have used the establishing of a space program as a launching pad for the development of a ballistic missile infrastructure. India received help in the development of its space industry from the United States, West Germany, France, and the Soviet Union. In pushing the development of space technology, those countries inadvertently pushed forward India's ballistic missile program.

Some countries have been pushed by the United States to develop an indigenous defense industry. The United States adopted this strategy in the early 1970s as part of the Nixon Doctrine, which urged U.S. allies to build their own defense industry to acquire the means to defend themselves. South Korea exemplifies this strategy, as it obtained most of its ballistic missile technology from the United States.

Finally, some countries, such as Iran and Iraq, have relied on external suppliers for the technologies needed for ballistic missiles. Here, the limits of the Military Technology Control Regime (MTCR) of 1987 are apparent. The industrialized nations have been unable to create an effective international regime to stem the flow of ballistic missiles to the Third World.

Those interested in international relations theory might find this book disappointing. Nolan at times alludes to important themes in international relations theory, but they are never fully developed. Though theory development is clearly not the book's goal, there is much data for those interested in the development of international relations theory. Nolan points to both domestic and international factors as explanations for the failure of stemming ballistic missile proliferation. One interesting area for research would be a look at the MTCR from the perspective of the rich literature on regimes: the reasons for the lack of an effective regime should be explored for this case.

In addition, Nolan's chapter on the bureaucratic dimension of U.S. policy toward arms transfers and ballistic missile proliferation is of particular interest for U.S. foreign policy specialists. Nolan aptly notes that the institutional structure responsible for these issues has lagged behind technological changes. Both intellectually and institutionally, U.S. policy toward this emerging issue remains fragmented without adequate coordinating mechanisms. That the bureaucratic apparatus within the American state has not changed despite the increased importance of this issue is consistent with the emerging literature in international relations that has focused on the importance of domestic political structures. Though Nolan does not make explicit note of this literature, her findings provide support for those who have stressed the importance of institutions for explaining foreign policy outcomes.

The author does not discuss another possible explanation for the lack of a coordinated policy toward ballistic missile proliferation: perhaps policymakers have judged ballistic missile proliferation not to be a problem at all. They may feel that the limited range of Third World ballistic missiles does not threaten U.S. interests. Or perhaps policymakers have judged that ballistic missile proliferation will not lead to increased conflict in the Third World. After all, U.S. policymakers have always stressed the deterrent and stabilizing effect of U.S. ballistic missiles and thus may see Third World proliferation through the same lens. Such a perspective is short-sighted, but it may partially explain why the industrialized nations have failed to pay adequate attention to a growing security threat.

Nolan shows that Third World ballistic missiles have had a limited military impact. For example, they are inaccurate and have a shorter range than many of the combat aircraft in the Third World. However, ballistic missiles do offer some advantages with their increased speed and ability to elude air defense. In addition, the lack of accuracy will not last forever (though Nolan does make clear that great technological obstacles make this a problem).

Policymakers must carefully identify the nature of the threat. With the Soviet threat receding, some U.S. policymakers have justified increased funding for the Strategic Defense Initiative on the basis of the Third World ballistic missile threat. However, at this time, China is the only developing nation that can pose a threat to the United States with ballistic missiles. Thus, U.S. policymakers must be careful to respond appropriately to this growing security concern, and must not take steps that would inadvertently exacerbate the threat. For example, the Pentagon has recently announced that it is considering reorienting U.S. nuclear targeting toward Third World countries. Unfortunately, this policy may serve as an impetus to nuclear and ballistic missile proliferation.

The author is careful to note that the threat from the proliferation of ballistic missiles may soon increase. Even inaccurate missiles present a serious problem if coupled with chemical or nuclear warheads. In this sense, Nolan's suggestion for increased attention to this issue and for certain institutional reforms to cope better with this problem are crucial. It is also clear that the United States cannot go at it alone and that multilateral solutions are essential. Nolan's analysis and prescriptions provide a good starting point.

University of North Carolina, Chapel Hill ERIC MLYN

Law and Morality in Israel's War with the PLO. By William V. O'Brien. New York: Routledge, 1991. 342p. $45.00 cloth, $19.95 paper.

It is an unfortunate but understandable reality that books on the Arab–Israeli conflict are often judged as

much by their consonance with the political views of partisans as by their intrinsic intellectual content. This will probably be true with William O'Brien's *Law and Morality in Israel's War with the PLO*, too—all the more because of the difficult and contentious ground it seeks to tread.

Thus, those who tend to emphasize the paramount importance of Israeli security will likely welcome this volume. O'Brien argues that ever since the initiation of armed attacks by the PLO in 1965, Israel has been engaged in a "just war" against an organization whose primary strategy has been one of "terrorist attacks" (pp. 3, 7, 320). Any attempt to ascertain the legality of Israel's action, he holds, must be set in the context not only of the formal content of international law but also a realistic appraisal of both the continuing nature of the conflict and the actual (as opposed to rhetorical) practice of other states in the international system. On this basis, he concludes that the actions of the Israel Defense Forces have been militarily discriminate and proportionate and hence in reasonable conformity with the laws of war (pp. 168–69, 210). The few exceptions are limited aspects of retaliatory policy in Lebanon and the conduct of the 1978 invasion, some Israeli shelling of Beirut and the Sabra–Shatila massacres of 1982, and the behavior of some soldiers since the start of the Intifada in the occupied territories.

By contrast, those who place greater value on Palestinian self-determination will be unconvinced. Their largest objection is likely to be the extent to which O'Brien reduces the conflict to one of PLO terrorism and Israeli response. The former term—ubiquitous throughout the volume—includes a great many acts carried out by groups outside the PLO (including groups the PLO has sought to suppress) and is also stretched to include such things as attacks on military personnel and vehicles in the occupied territories (p. 224). Moreover, by raising the wholly unrealistic specter of the destruction of Israel—and by implicitly or explicitly citing a standard of international practices that includes France's brutal counterinsurgency in Algeria; South Vietnam's treatment of captured Viet Cong; and the human rights practices of Saudi Arabia, Syria, and Iraq—O'Brien is able to defend the many thousands of civilian deaths exacted by Israeli attacks in Lebanon, the policy of beatings adopted to combat the Intifada (p. 258), official torture (p. 254), the closure of Palestinian schools (p. 249), and violations of the 1949 Geneva Conventions relating to prisoner-of-war status (pp. 197–201) and the deportation and collective punishment of civilian populations under belligerent occupation (pp. 259–260). In general, the sorts of charges leveled by Amnesty International, Human Rights Watch, al-Haq, B'Tselem, and the U.S. State Department receive far less attention than Israel's self-justifications. A number of factual errors could be pointed to, including the claim that the PLO initiated military action in 1965. (The action in question was attempted by a group not then part of the PLO.) O'Brien's treatment of Israel's decision to invade Lebanon in 1982 can be seen as seriously flawed, both in its treatment of the *causus belli* and in its failure to adequately address the extent to which that war was directed at installing a compliant government in Beirut. Finally, there is much that he simply does not deal with, ranging from the full activities of Israel's proxy "South Lebanon Army" to the legality of Israeli settlement activity in the occupied territories.

My own views are mixed. O'Brien's analysis is, indeed, clearly framed and thought-provoking. He is certainly correct in underlining the necessity of treating the conflict as a protracted and continuing one wherein individual events cannot be treated in isolation. Yet O'Brien commits precisely this error, focusing on sporadic acts of terrorism at the expense of a real understanding of the Palestinian Nationalist movement, and isolating the morality of Israel's military activities from the morality of its military occupation and opposition to Palestinian self-determination. This problem becomes particularly acute after 1977, with the accession to power in Israel of the hardline nationalist Likud party. Likud policies toward the PLO were clearly driven more by ideology (territorial claims to the West Bank, hence the need to suppress independent Palestinian political organization) than by concerns over the limited threat to Israeli security posed by Palestinian "armed struggle." Indeed, as Avner Yaniv has shown in the Lebanese case (*Dilemmas of Security*, 1987), Israel often used military force to preempt or undermine growing Palestinian moderation that might otherwise have led to a negotiating process and future political settlement involving territorial compromise.

For these reasons, O'Brien's study cannot really be seen as the "comprehensive and objective analysis" that its dustjacket proclaims. However, it does represent another contribution to the continuing legal, moral, and political debate regarding the Palestinian–Israeli conflict.

McGill University REX BRYNEN

On Internal War: American and Soviet Approaches to Third World Clients and Insurgents. By William E. Odom. Durham: Duke University Press, 1992. 271p. $29.95.

This book examines how the United States can best respond to internal wars backed by the former Soviet Union (and others) in key Third World countries and areas. The author, William Odom, selects three countries of concern to the United States—El Salvador, Guatemala, and the Philippines—as well as the Middle East and Southwest Asia, to determine what the United States can and should do in light of the actuality or threat of internal wars that will affect important American interests.

Odom's key contribution is to combine political development theory (with heavy emphasis on Samuel Huntington's work) with knowledge of counterinsurgency warfare (he was director of the National Security Agency and retired as a lieutenant general in the U.S. Army) to guide American policy with regard to internal warfare. He observes that the prospects for democracy in each of the countries he studies are not good. The economic, social, external, and cultural forces are just too weak to support democratic rule over the long term (although attempts at democracy are not precluded). Moreover, in the wake of the Cold War, Odom correctly sees instability rising, as the restraining impact of the superpowers on Third World conflicts diminishes.

Odom's key conclusions are both startling and persuasive. American assistance to governments fighting insurgencies does far more harm than good. Militarily, it saps the ability and incentive of the government to

defeat the insurgents on their own. Economically, it stands in the way of the government's establishing effective tax extraction measures. By way of example, Odom contrasts Guatemala's relatively successful efforts at suppressing an insurgency and bringing about economic development with El Salvador's often disastrous results. The key distinction for Odom was that Guatemala had to act with virtually no U.S. assistance, whereas El Salvador relied on massive American support.

Odom also challenges the conventional wisdom by arguing that external forces play a key role in internal wars. He makes the case that outside assistance (from the USSR, Nicaragua, and Cuba) has been crucial for the El Salvadoran and Guatemalan rebels and that outside support (from the USSR and others) has even played a significant role among the Filipino insurgents. He concludes that the most effective way for the United States to defeat internal wars is to strike at external sources of support. Odom acknowledges that this will often be politically impossible (and perhaps unwise, as well); but he makes a convincing case that if the United States is serious about defeating internal wars, it should either go to the source or stay out of the conflict entirely.

Odom concludes with a set of thoughtful policy recommendations. They include that the American government narrow its focus to specific areas where troops are likely to be engaged when planning force development and operations, reduce the size of counterinsurgency forces while improving their quality, develop small countercoup and rescue forces, redefine international law to allow the United States to cope with internal wars, and transfer principal responsibility for paramilitary forces from the CIA (where they do not receive adequate attention) to the Department of Defense.

There are some problems with this otherwise valuable book. Discussions of Soviet strategy in the Third World have a Cold War flavor that has been largely overtaken by events. The book suffers from repetitiveness (the phrase, *colonialism by ventriloquy*, is especially overdone), does not make the case why vital American interests are engaged by internal wars, and treats the Middle Eastern and Southwest Asian countries superficially. The informal and somewhat eclectic style of the book may put off some academics who seek a more rigorous scholarly approach. In addition, Odom does not adequately explain why outside support can be so vital to insurgents and yet so harmful to incumbent governments.

These quibbles aside, this is an important book. As Odom notes, even the collapse of the Soviet Union will not stop internal wars that affect American interests. Leninist precepts on seizing and holding power have not lost their relevance in the wake of the Cold War. Outside powers hostile to the United States (e.g., Cuba, North Korea, and Iran) may continue to play a role in assisting insurgencies. Most important, the notion that the United States can do more by doing less is a refreshing change from much of the burgeoning literature on "low-intensity conflicts" and warrants serious consideration. That this view comes from an individual who has combined a successful career in scholarship with firsthand knowledge of actually dealing with insurgencies adds a special potency to the analysis. Policymakers and scholars will do well to take heed.

Johns Hopkins University STEVEN R. DAVID

Joint Ventures in the People's Republic of China: The Control of Foreign Direct Investment under Socialism. By Margaret M. Pearson. Princeton: Princeton University Press, 1991. 335p. $39.50.

Beautiful Imperialist: China Perceives America, 1972–1990. By David Shambaugh. Princeton: Princeton University Press, 1991. 326p. $35.00.

Since its Open Door Policy of the late 1970s, China's role in the global political economy has concerned scholars from both inside and outside the mainland. In the West, in particular, China's new identity in the world has prompted China scholars to break from the traditional idiographic approach of area studies and utilize more general theories, concepts, and arguments of social science to help us better understand and explain developments in China and their repercussions for the world. Two recent attempts are David Shambaugh's *Beautiful Imperialist* and Margaret Pearson's *Joint Ventures in the People's Republic of China*. Shambaugh examines the impact of (elite) perceptions on (foreign policy) behavior, and Pearson investigates the ability of a socialist state to control the costs of foreign investment while absorbing its benefits. While both are commendable in their attempts to match theory with substance, they remain more grounded in the latter than the former.

David Shambaugh's *Beautiful Imperialist* presents the views, analyses, and prescriptions of China's "America watchers" from 1972 to the present. Three main questions motivate his study: (1) "What is the substance of [the America watchers'] articulate perceptions, what is the variation among them, and how have they evolved over time?" (2) "What are the key explanatory factors (political, diplomatic, intellectual, sociological) that shape the specific perceptions and the images that lie behind them?," and (3) How does perception affect policy? (p. 35). He draws on extensive published writings (both public and restricted) and lectures of China's America watchers, as well as his personal interviews with these analysts.

Shambaugh propounds the thesis that two schools of thought have emerged among China's America watchers: Marxist and non-Marxist. (A third, more traditional and nationalist group appears only in foreign policy analysis. The "hegemonist" school consistently accuses the United States, as well as the Soviet Union, of superpower chauvinism. This approach, says Shambaugh, originates from China's historical experiences during the "century of humiliation" and thus represents an historical, rather than ideological, legacy.) The Marxist group is distinguished by its rigid adherence to ideology, which insists on "deductive analysis": "Marx says X or Lenin says Y, ergo this must be the case in the United States. Thus, the Marxists proceed on this basis to marshal evidence to sustain the a priori theories" (p. 136). The non-Marxist group, however, is less ideologically motivated and more interested in interpreting current events "in a largely atheoretical manner that presents the facts to their readership" (p. 86). Regarding the U.S. economy, for example, Shambaugh adds that the non-Marxists "are not political economists but real economists whose evidence is more empirical and statistical" (p. 136).

By characterizing China's non-Marxist analysts as "atheoretical," "less ideological," "ad hoc," and "descriptive," Shambaugh suggests that they operate outside any normative, theoretical, or ideological bound-

aries. This presumed neutrality, therefore, indicates that they have no self-serving agenda like the Marxist analysts. Yet Shambaugh fails to notice that these so-called free thinkers accept the same units of analysis, methodological assumptions, and institutional framework of Western neoclassical economics and positivist realism. On economic issues, for example, the non-Marxists focus on interest rates, inflation, tax cuts and money supply, government regulations on business, deficit financing, and federal expenditures. They accept American intellectual conventions (i.e., inflation vs. unemployment, Democrats vs. Republicans) and do not question their validity or even relevance. On foreign policy, China's non-Marxists sound like typical realists, with their emphases on superpower politics, the arms race, mutual deterrence, and U.S. and Soviet interventionism in the Third World.

Divergent intellectual traditions in China warrant attention and identification. But just because China's non-Marxists are beginning to sound like Western technorationalists, they are not necessarily, therefore, "nonideological" and "real," as opposed to the "polemical unreality" of Marxist–Leninists. Shambaugh concludes by declaring that a "perception gap" continues to plague United States–China relations: "Both sides are imbued with severe cases of cognitive dissonance, wishful thinking, stereotypical imagery, misinformation, and misperception" (p. 302). Indeed, this condition goes both ways when scholars of either side hide under the protective belt of "objective rationality" and charge their counterparts with ideologically skewered analyses.

Equally important, this book fails to address the methodological weaknesses of individualistically based, cognitive analysis. For example, Shambaugh presumes that individual input (i.e., China's America watchers) leads to a collective outcome (Chinese foreign policy decisions). Aside from questioning the methodological validity of linearly extrapolating from the individual to the group, another issue arises: What accounts for the content and nature of individual input and its variations over time? Shambaugh himself attributes the rise of non-Marxist analysis in China to increased exposure to the United States and changing institutional demands. But additional questions nag at the reader: Why and how did these structural and institutional changes occur? Why did these changes assume one particular trajectory and not another? Such inquiries expose the liabilities of Shambaugh's premise that "behavior is principally a function of perception" (p. 3). Clearly, perception and behavior interact, but Shambaugh acknowledges only one side of the relationship. Until these ideological and methodological issues are redressed, cognitive studies of this variety provide limited explanations while offering, at the same time, rich sources of textual data and information.

In contrast, Pearson's study of Chinese state control of foreign direct investment demonstrates methodological rigor and avoids ideological self-congratulation. Using China as a case study, she tests the supposition suggested by some *dependencia* and other critics that "socialism in some form would overcome the problems experienced in capitalist developing countries and would provide a substantial increase in host bargaining power" (p. 13). She shows how the Chinese state has tried, through a variety of measures, to "contain" the negative effects of foreign investment (e.g., loss of sovereignty, loss of "Chineseness") while reaping its benefits (e.g., technology transfer, foreign currency, demonstration effects). Pearson accomplishes this by focusing on host country controls at the national, regional, and enterprise levels. Given its rich data base and meticulous research, this study offers a firstrate history of foreign investment in China from 1978 to 1988.

Pearson concludes that a socialist state does not necessarily exert greater control over its foreign direct investment sector; indeed, China's record is mixed. While the Chinese state was successful in maintaining some control in foreign direct investment sectors (e.g., enterprise welfare provisions, externally imposed economic regulations), it eventually ceded a certain level of authority to foreign management (e.g., obviating political study sessions and political secretaries in enterprises): "At the most basic level, some of the specific tools the government intended to use to control the negative effects of foreign investment were simply ineffective in the post-Mao environment" (p. 203). Equally important, China's liberalization process effected a change in the relations between the state and transnational corporations as Chinese economic reformers increasingly conceded that the overall economic benefits of foreign investment outweighed its potential social costs.

Pearson, however, remains curiously silent on a potentially significant contribution suggested by her research: If the nature of the host state (i.e., capitalist, socialist) matters little in its long-term ability to control or contain transnational corporations, then what does matter (if anything)? Pearson does not address this logical implication of her findings. Instead, she discusses the generalizability to other socialist states of her findings on China. But this discussion seems moot, given Pearson's own conclusions. If a continental socialist state as ideologically bound and economically sufficient as China's cannot resist the seduction of foreign capital, technology, and know-how, why should smaller, less well endowed socialist states like Vietnam and Tanzania be able to fare any better? Not surprisingly, Pearson remains pessimistic: "Any expectations that socialism will be adequate to allow these countries to control substantially the terms of investment while absorbing satisfactory amounts of such investment are unlikely to be met" (p. 223).

Furthermore, I disagree with some of Pearson's theoretical assumptions. She accepts too easily the double artificial divide between domestic politics and international economics. The ability of a host state to control foreign investment, Pearson writes, reflects "its domestic political and economic systems, on the one hand, and the international economy, on the other" (p. 9). Indeed, Pearson continues with this line of analysis to speculate on China's future foreign investment environment. "It is certainly possible," she states, "that problems in areas not specifically related to the 'open' policy—inflation, the social upheaval caused by modernization, or growing intolerance over 'bourgeois liberalization'—could spill over into the realm of foreign economic policy and provide an impetus to halt foreign investment" (p. 216). But where do these "domestic problems" come from? Why would an otherwise autarchic, totalitarian state like China's experience inflation, modernization problems, and fears over "bourgeois liberalization" were it not for linkages with the world economy? Such insistence on domestic–foreign and political–economic separations only muddles our understanding of the nature of global interaction.

These criticisms aside, though, both Shambaugh's and Pearson's works represent the "future wave" of China studies. They are to be lauded for striving to match theory with ethnography for a case study as perplexing and fascinating as China's.

Syracuse University L. H. M. LING

Righteous Realists: Political Realism, Responsible Power, and American Culture in the Nuclear Age. By Joel H. Rosenthal. Baton Rouge: Louisiana State University Press, 1991. 191p. $24.95.

Realism is being put to the test by sweeping transformations in the international society. Traditionalists are looking backward, reinterpreting historical approaches for guidance in a sea of change. Postrealists are looking forward, applying new theories culled from continental philosophy to understand the challenges of late modernity. And neorealists are waiting for the data to come in.

Joel Rosenthal, editor of the journal *Ethics and International Affairs*, is clearly in the first camp. In a very incisive manner he sets out to rescue realism from its critics, as well as from some misbegotten disciples. He does so by going back to its origins, not in the sense of some pristine past of pure realism but of a formative moment in international politics when a public debate among a cosmopolitan group of thinkers and leaders, including George Kennan, Hans Morgenthau, Reinhold Niebuhr, Dean Acheson, and Walter Lippmann, gave rise to an uniquely American brand of political realism. Trying to make sense and a new order out of the postwar flux, these prototypical realists tested the legacy of the U.S. foreign policy (moralistic and legalistic, isolationist and exceptionalist) against the European tradition of realpolitik (where national interest outweighed, but did not ignore, moral considerations). The result, according to Rosenthal, was the advocacy of a hybrid American realism that was philosophical yet practical, prudent but not passive, critical *and* consensual. In short, a "righteous realism" to lead the nation and to instruct the public in the new responsibilities and obligations of a superpower.

In structure and intent the book is a concise review of the lives and thoughts of these seminal American realists. Indeed, the strength of the book lies in its ability to strike a balance between historical context and theoretical content, allowing the author to elucidate the key ideas of realism, demarcate the limits of its influence on American foreign policy, and make the case for its renewal—all in less than two hundred highly readable pages. In the process, Rosenthal does the field of international relations a service by envisioning a more complex realism. The straw man so often constructed by realism's critics is dwarfed by Rosenthal's comparative analysis of how these self-conscious realists used philosophical, historical, and moral reasoning to deal with the pressing issues of the day. There was nothing neat or parsimonious about their theories: the constituent elements of protorealism (the inevitability of tragedy, the play of paradox, the intervention of contingency) made for a highly normative, yet pragmatic, system of thought. This made, realism, the author argues, rather than any competing worldview, the best vehicle for understanding the moral responsibilities and political ambiguities that came with America's new power.

These individual *Ur*-realists unquestionably left their mark on American foreign policy, yet their ideas as a school of thought never really took root in the halls of power, the public realm, or the universities. Rosenthal goes beyond a theoretical and historical approach to explain the limits of their influence by detailing the tension between their professional responsibilities and personal beliefs (and the not always successful attempt to reconcile them). In their pursuit of a public philosophy of realism, they were able to transcend their primary identities as academician (Morgenthau), theologian (Niebuhr), statesman (Acheson), or journalist (Lippmann). They did so without pandering to the public; and although all held at one time or another high positions in the establishment, they eschewed elitist prerogatives. Rosenthal never really makes it clear whether the status was one of choice or circumstance; but the righteous realists were most effective (and perhaps most comfortable) as outsiders, or in Rosenthal's words, "cultural critics."

These realists did run against an American tradition in that they were machiavellian to the extent that they openly recognized the paramountcy of power in both domestic and international affairs. They were not, however, willing to subordinate their principles to the pressing demands of the Prince or to the passing desires of the public. Perhaps they could best be described as "Odyssean" realists: early in their careers they had heard and survived the siren calls of power, wandered and been tested in the wilderness, and then returned to warn the public of grave dangers, of "technocratic illusions" (Niebuhr), "a new feudalism" (Morgenthau), and "the sophisticated mathematics of destruction in which we have been entangled" (Kennan) (pp. 118, 135, 160). Most of them challenged conventional wisdom with nuanced positions of opposition at critical moments in American politics: Niebuhr criticized area bombing in World War II, Lippmann took on McCarthyism in the 1950s, Morgenthau early on came out against the Vietnam War, and Kennan opposed the Reagan arms buildup in the 1980s.

The author puts great stock in historical exposition, intellectual tension, and moral struggle to explain the efforts of the early realists to overcome the isolationism and exceptionalism of American foreign policy. Yet, by leaving out the story of other influences and challenges, the book serves in a peculiar way to reinforce American parochialism. For instance, there is no mention of the important cross-fertilization of American with British realism. What of the claim made by E. H. Carr, the British father of modern realism, that his greatest intellectual debt was to Niebuhr? Or Morgenthau's alignment with Herbert Butterfield and Martin Wight against the "abstract rationalism" of American international relations theory. Can one ignore the role of the Rockefeller-funded Committees on the Theory of International Politics set up in the late 1950s on both sides of the ocean for the study of international theory?

In the end, Rosenthal's account of the fathers of American realism rings true but incomplete. A new appreciation of how these men profoundly influenced American politics is gained; but one is left wondering how these giants of critical and moral reasoning were so easily conquered by the Lilliputians: the game-theorists who made MAD make sense, the systems analysts who rationalized the Vietnam War, the vulgar realists who justified the second Cold War. For that purpose, a

sociology of institutional politics is needed, as well as a rhetorical study of how the language and the power of the early American realists, initially steeped in the theologicoethical lexicon of tragedy and providence, justice and order, was scrubbed clean and neutralized by a nascent social science seeking a value-free discourse, thereby handing the public philosophy of international relations back to the ideologues.

University of Massachusetts, Amherst JAMES DER DERIAN

Images and Arms Control: Perceptions of the Soviet Union in the Reagan Administration. By Keith L. Shimko. Ann Arbor: University of Michigan Press, 1992. 288p. $37.50.

Images and Arms Control, by Keith L. Shimko, is a pleasure to read. The book uses, and contributes to, a developing political–psychological framework increasingly referred to as "image theory." It examines important theoretical issues, raises provocative questions that should fuel debate in this theoretical orientation, and applies the framework in an explanation of a series of arms control issues during the Reagan administration.

The theoretical portion of the book should provoke questions and debates among readers interested in political psychology. It begins with a discussion of the concept of images in political psychology and explores the enemy image in detail. The author expands upon a simple one-dimensional assessment of this construct, which is an important task in the refinement of the framework. Shimko argues that subtypes of the enemy image can be developed by fleshing out the attributes of the image further. He suggests, for example, that discriminations among types of enemies are made in perceived distinctions between "dispositional" and "situational" motivational forces. Perceived enemy capability attributes are amplified by adding nuclear and conventional weaponry attributes to the superiority, equality, inferiority perceptions commonly associated with the enemy image.

This effort raises a host of interesting questions concerning the general images concept. One set of questions concerns the problem of conceptualizing an image in a form that is distinct from the actual "fit" of a particular state in the image. One can ask first whether the proposed additional perceived dispositional, situational, and capability characteristics are at a sufficiently high level of abstraction to be useful and cognitively efficient components of an image. If not, they are not likely to be elements of an enemy image at the abstract conceptual level. Instead, these attributes may be ad hoc operationalizations or potential operationalizing categories used by policymakers on a case-by-case basis and therefore crucial elements in furthering the analyst's ability to describe the policymaker's full picture of a state once it is categorized into an image. While Shimko suggests that these are elements of image subtypes, he uses them as indicators for delineating differences in perceptions of the USSR within the Reagan administration.

A second important question raised in this theoretical portion of this volume concerns the nagging question of whether it is theoretically wise to think of differences among policymakers with similar images in terms of subtypes of images or positions upon an image continuum. Shimko prefers the former, but psychological findings should be important considerations in a larger debate on this issue.

The empirical portion of this book should be of interest to all readers, whether they are attracted to the theoretical orientation or not and regardless of differences in methodological demands. It presents a careful, well-supported assessment of the variations in Reagan administration views of Soviet motivations, strategy, decision style, and negotiating behavior. Shimko uses content analysis to provide empirical support for his evaluation of five central figures in the administration: Caspar Weinberger, Richard Perle, George Shultz, Richard Burt, and (of course) Ronald Reagan. The content analysis technique and problems in the methodology are clearly addressed, and the author presents his data in a readable form.

Shimko finds important similarities in images of the Soviets among Weinberger and Perle, on one hand, and Shultz and Burt, on the other. They were shown to differ in terms of both the content and quality of their enemy images of the Soviets, with Shultz and Burt often appearing not only less hard-line but also less certain in their assessments of Soviet motivation and intentions. Reagan is found to contain elements of each camp's image, but is ultimately closer to Weinberger and Perle. The policymakers' use of these enemy images is compared in terms of content, cognitive style, interest, knowledge, and other patterns. Most interesting is Shimko's ability to specify which elements of the images were different. The impact of those differences is then shown in an analysis of the INF, SALT II and START, and SDI debates.

The author's discussion of the impact of images on policy preferences is particularly useful in addressing the problem of how precise an explanation of policy preferences and tactical choice one should expect from political psychology. Shimko shows that the concept can be used easily in explaining general policy preferences and disputes. He also does an admirable job of connecting small variations in the enemy image of the USSR with specific differences among policy advocates concerning not only arms control in principle but their positions on classes of weaponry. He does not make the mistake of demanding that a perceptual analysis explain the exact number of weapons selected, but he does push the concept as far as it will go.

Finally, this book explores the limits of image-based analysis. The author honestly notes and accepts positions that cannot be accounted for with images and suggests a variety of additional explanatory factors ranging from personality characteristics to national self-image. In the process, he may give Ronald Reagan credit for more cognitive wherewithal than he is due; but that too is a mark of the author's effort not to overstep the boundaries of what he can claim for his analysis.

Washington State University MARTHA COTTAM

The Diffusion of War: A Study of Opportunity and Willingness. By Randolph M. Siverson and Harvey Starr. Ann Arbor: University of Michigan Press, 1991. 141p. $29.95.

The question of why most interstate wars remain small and localized while a few become large and, more

rarely, global is the central concern of this volume. More specifically, the authors examine the degree to which geographical contiguity and alliance commitments affect the choices of states to join or not join an ongoing war. They also consider whether these factors have any impact on the timing of war-joining decisions. In a nutshell, the reported empirical findings support the following conclusions. States that are contiguous with, or allied to, states already at war are significantly more likely to join the war than those that are not. Moreover, among joiners, states with closer geographical and alliance ties to warring states are likely to join wars sooner than those with looser ties. These findings are based on an examination of the Singer–Small war data for the period 1816–1965. Geographical contiguity is measured in terms of three categories: land border, cross-water border (separated by 200 miles or less of water), and colonial border (which actually includes other types of dependencies such as occupied areas). The alliance data are essentially the Correlates of War set, and the authors employ the familiar three-part distinction (defense pacts, neutrality/nonaggression treaties, and ententes) found in these data in their analysis. With both measures, the relative strength of effect expected on war-joining accords with the order just given; for example, states that have a land border with a warring state are more likely to join an ongoing war than those with a cross-water border. In general, the empirical results do support these expectations.

If the authors did little more than report their empirical results, one might question the justification for the book when one or two journal articles could have conveyed the same information faster and cheaper. (Indeed, some of the results have appeared in articles.) The answer to this lies in the broader purpose of the authors, that is, to show by example how the scientific study of war should be conducted.

Many of those engaged in the scientific study of war have argued for some time that wars should not be viewed as events but, rather, as processes; that is, wars do not simply start and, after a period of time, stop but evolve through time in response to factors that continuously promote or constrain their size, magnitude, and duration. Although such a process orientation may seem obviously appropriate to the study of war, the vast majority of empirical studies of war done in the past have not been in accordance with it. For example, the many time series studies that have used nation-months of war or battle deaths in war as a dependent variable implicitly assume that the factors that account for the occurrence of war are the same as those which account for its evolution. Ignored in these studies is the strong likelihood that new, nonlinear forces are set in motion after the beginning of a war that determine its ultimate character.

According to this process conception of war, there is not *a* war puzzle but, rather, a series of connected war puzzles that require solutions in order to develop integrated, cumulative knowledge about war. A major virtue of *The Diffusion of War* is that the authors embrace this process conception of war and focus on one of the more important war puzzles, namely, Why do most wars remain small, while a few become very large?

Another desirable characteristic of the book is that the authors explore the opportunity and willingness framework more thoroughly than any previous study. According to this conceptual framework, in order for a war to occur, at least two states must have the *opportunity* and *willingness* to engage in combat with one another. Thus, it is useful to categorize the factors that affect the likelihood and character of war in terms of what impact they have upon perceived opportunity and willingness. The authors elaborate this framework and spell out some of its implications. A major contribution of this framework is that it reminds us that a war requires at least two decisions to fight, one to attack and a second to resist. This may seem obvious; but it contradicts the standard implementation of expected utility theory, since, based on objective conditions, the more desirable a war decision is for an attacker, the less desirable it is for a defender to resist. In short, we must explain not only why some states attack (or threaten to attack) other states but also why those other states resist these attacks or threats.

As promising as this conceptual framework is, it does contain some problems. It rests fundamentally upon an ends–means distinction that is hard to maintain even in the abstract. Opportunity engenders willingness and willingness creates opportunities, and the two may be so intertwined as to make a distinction between them impossible. This problem becomes much worse when we turn to the operational side. For example, does a common border affect the opportunity for war or the willingness to war or both? Since the correct answer to this question is most probably *both* and, lacking direct measures of opportunity and willingness, we have no empirical basis to apportion the effect of common border, we are left with an arbitrary assumption about the nature of its effect. This problem is not restricted to geographical factors, and it raises the question of whether opportunity and willingness can be empirically separated in a meaningful way.

The authors should also be credited with directing our attention to the interdependence of events with their focus on the diffusion of war. Almost all standard statistical inference procedures are based on the assumption of independence; and the authors rightly point out that much social behavior (e.g., war joining) violates this assumption, sometimes dramatically. The authors embrace a diffusion–contagion model but (to my disappointment) do not develop, estimate, and test probability models that incorporate interdependence.

A final important contribution of the book is its reinforcement of the "rediscovery" of geography. War studies (and quantitative international politics in general) have neglected the spatial factor in world affairs. Too often, this factor has been considered trivial, uninteresting, or constant (all of which it is not) in previous analyses; and the authors deserve praise for demonstrating its importance.

In contrast to these good points, the book does have some notable deficiencies. The empirical findings are thin in comparison to the expectations raised by the title and theoretical sections of the book. Many factors affect the decision to join a war, and the authors' focus on only two of these is regrettable. Their focus is further restricted by the fact that they are only concerned with whether and when states join wars and not with which side they join. No explicit models are derived or tested, and the methods used are not uniformly state-of-the-art. Finally, although the authors rely on the Correlates of War data, they take exception to a number of Singer–Small coding decisions. These are sufficiently numerous as to warrant a complete listing of the war data used.

In spite of these deficiencies, the book makes an important contribution to the study of war, largely because it points in the direction we need to go if we are to produce scientific, cumulative knowledge about the causes of war and conditions of peace.

State University of New York, Binghamton STUART A. BREMER

Myths of Empire: Domestic Politics and International Ambition. By Jack Snyder. Ithaca: Cornell University Press, 1991. 330p. $35.00.

It is often noted that our theories of the domestic sources of international conflict and security policy have never been as fully developed as systemic, bureaucratic, or individual-level theories. With the publication of Jack Snyder's *Myths of Empire*, this is no longer true. Snyder poses the question of how to explain the numerous cases of overexpansion in the industrial age. He constructs a modified rational choice explanation based on the parochial interests of key domestic and bureaucratic groups, the strategic myths they construct to rationalize their expansionist policies, the processes of coalition formation and logrolling among these groups, and the domestic structures and patterns of industrialization under which these dynamics are most likely to arise.

Earlier theories of imperialism or overexpansion focused on the interests of self-serving monopoly capitalists (Hobson and Lenin) or atavistic military–feudal elites (Schumpeter). These concentrated interests reaped the benefits of overexpansion, while the costs were diffused throughout society in the form of taxation. Snyder, building on theories of collective action and voting theory, argues that groups concentrated enough to benefit from overexpansion are too narrow to control state policy, whereas those broad enough to control policy lack the motivation for overexpansion because their interests are too diffuse to reap net benefits. Snyder argues that key internal groups—each with parochial interests in limited but different forms of imperial expansion, military buildups, or economic autarky—create logrolled coalitions that secure power at the cost of a more expansionist foreign policy than is desired by any single group, and a policy that cannot be supported by available resources.

But logrolled interests do not alone explain overexpansion or the stability of the ruling coalition behind it. Key elites justify their power and policies through strategic myths which suggest that security is possible only through external expansion. Snyder rejects the hypothesis that strategic ideas are derived from intellectually formative lessons from history and argues, instead, that they are constructed by actors to rationalize their preferred policies. But these elites often internalize their own rhetoric or become politically entrapped in it, which inhibits strategic learning and disengagement from increasingly costly expansionist policies.

These dynamics are most likely to arise in cartelized political systems dominated by distinct concentrated interests. Overexpansion is less likely and less extreme when it occurs in democratic systems (where interests are diffuse and where the absence of information monopolies works against strategic mythmaking) or in unitary oligarchies (which have encompassing interests in avoiding the costs of overexpansion). Overexpansion is less predictable in dictatorial systems in which there is no check on the preferences of the leader. Domestic structure is a function of the timing of industrialization, which correlates closely, Snyder argues, with the concentration of power in society and the concentration of group interests. Early industrialization (Britain and the United States) is associated with diffuse elite interests and mass democracy, late industrialization (Germany and Japan) with concentrated interests and a cartelized system, and "late late industrialization" (the Soviet Union) with a centralized system ruled by a unified elite with encompassing interests.

Snyder tests the theory against competing power-based realist and cognitive learning hypotheses for five important cases of overexpansion: Germany from Bismarck to Hitler, interwar Japan, Victorian Britain, and the Soviet Union and the United States during the Cold War. The research design is carefully constructed to allow cross-sectional comparisons across countries, longitudinal comparisons within each country over time, and comparisons of group and individual preferences and strategic mythmaking within a country at a given point in time. The individual case studies are strongly driven by the theory, thoroughly informed by the secondary historical literature, and for the most part quite persuasive.

Snyder's findings support his theory. The most cartelized political systems were the most overexpansionist, whereas democracies and unitary oligarchies were considerably less so. In addition, Germany and Japan were least expansionist during those periods when they were least cartelized, while democratic and unitary states were the most expansionist when concentrated interests were relatively strong. Snyder shows that strategic beliefs were not learned from history but, instead, constructed to rationalize domestic interests and that realist theory cannot account for observed behavior. He concedes, however, that external threats and opportunities influence internal coalition politics and suggests that a more complete theory needs to integrate power, interests, coalitions, and ideology at both the international and domestic levels.

Myths of Empire is the best-developed theory to date of the domestic sources of international conflict and security policy. The fact that it is confined to great power overexpansion in the industrial era raises questions, however, regarding the extent to which the theory can be generalized. Snyder does not pretend to explain overexpansion in the preindustrial era, and he also suggests that the coalition politics model probably does not fit small powers. Snyder's care in not overstating his case should be applauded, though the modification of the theory to apply to the different class, sectoral, and bureaucratic structures of preindustrial states and also of contemporary small states would be a fruitful area for future research.

Although not all overexpansion leads to war and not all wars grow out of overexpansion, Snyder's coalition politics theory is an important contribution to our understanding of the causes of war, particularly the underlying causes. But elite cost–benefit calculations for war may be different from those for expansion, in part because external threats may be more pressing and elites may not be able to pass the political and economic costs of war onto others or defer them to an uncertain future. Although Snyder's concept of self-delusion and political entrapment by earlier strategic rhetoric is undoubtedly

important here, decision-making variables and strategic bargaining with the adversary also needs to be included.

The generalizability of the theory may also be limited by its focus on overexpansion as the dependent variable. Although the German and Japanese cases may be rather obvious ones, the concept of *over*expansion is difficult to operationalize, particularly given uncertainties regarding the intentions of others and the consequences of one's own action. Moreover, realist explanations emphasizing state cost–benefit calculations may be more difficult to dismiss for less extreme cases of expansion, though Snyder's novel interpretation of Britain's limited expansion in the Crimean War case demonstrates the potential applicability of his theory to such cases.

But these are just quibbles. *Myths of Empire* is one of the most important books on international conflict to come out in several years. Snyder has constructed a reasonably parsimonious model of coalition politics supplemented, where necessary, by key variables from other levels. In so doing, he has taken a major step toward ending the theoretical impoverishment of the study of the domestic sources of international conflict.

Rutgers University JACK LEVY

Moscow, Germany, and the West from Khrushchev to Gorbachev. By Michael J. Sodaro. Ithaca: Cornell University Press, 1991. 423p. $49.95 cloth, $15.95 paper.

American international relations scholars and specialists on Soviet foreign policy tend to view events in the postwar period largely in terms of U.S.–Soviet relations. While that presumption is often valid, it can sometimes obscure important sources of Soviet behavior by treating it as a derivative of its U.S. policy. This is especially true in the case of Europe, where Soviet fears and hopes with respect to important countries such as Germany have their own logic. By taking the relative importance of the United States in Soviet–German relations as a variable, rather than as a given, Sodaro shows that the leadership and the secondary elite (academics, journalists, etc.) in the post-Stalin era have been divided on a very basic issue in Soviet policy toward Germany: Does the Federal Republic of Germany have any independence in charting its foreign policy course, or is it subordinate to American policy and influence? He argues (and demonstrates quite convincingly) that four widely ranging views on West Germany's role in East–West relations—maximalist, Europeanist, Atlanticist, and Americanist—existed throughout the period in the Soviet and East German leaderships and that the basic prevalence of Atlanticist views created a flexibility in Soviet policy that both made détente possible and limited its cooperative potential. The book also examines how the constraints of the Soviet–East German alliance played a role in the timing and extent of cooperative initiatives due to substantial differences in East German and Soviet interests and viewpoints.

Sodaro's primary concern is to demonstrate that Soviet and East German policy toward West Germany cannot be explained in terms of unitary actor approaches but must be evaluated in terms of domestic political structures and the varying policies and viewpoints within the leaderships. In chapter 1 he sets out four types of decision-making regimes (directive, conservative, centrist, and reformist) and four primary aspects of Soviet foreign policy activity (the scope of détente, degree of responsiveness, willingness to compromise with foreign governments, and openness to internal debate). Because of the internal dynamics of the different types of decision-making regimes, they tend to produce different forms of foreign policy activity. Ulbricht's directive regime assessed détente in very narrow terms, was unresponsive to changes or other states, was unwilling to compromise, and restricted debate. Gorbachev's reformist regime has exhibited the opposite tendencies on all these aspects, while Brezhnev's conservative regime, Khrushchev's centrist–reformist regime, and Honecker's centrist–conservative regimes occupied the middle ranges. Thus, Sodaro claims, variation in foreign policy between leaderships and within leaderships (early vs. late Brezhnev) can be explained only by the imperatives and course of domestic politics.

As such, this book is another important contribution to recent work dissatisfied with relegating Soviet and East European communist decision making to the "black box" simply because of the difficulty in obtaining hard data on politics and foreign policies. Most of the book explores the disagreements expressed by the primary and secondary leaderships and shows that at many points both the Soviet and East German leaderships entertained various policy options, with results due to both relatively constant national interests (primarily the need for West German aid, trade, and technology) and internal political images. For example, Sodaro shows that were it not for Khrushchev's fall in 1964, Soviet–West German détente would have taken an earlier and more comprehensive form, with less of the constraints of military competition that bedeviled policies in the late 1970s. Similarly, it is impossible to imagine a persistent Brezhnev leadership moving to a Gorbachev form of German relations, given their divergent domestic priorities and coalitions.

But this does not make for a *theory* of the role of domestic politics in Soviet and East German policies toward West Germany. It takes the different leaderships as basically given and demonstrates that they have different priorities that affect their foreign policies. Thus, it is not really a theory of domestic politics but of regime types. Evidence that different leaderships or regimes have different priorities and policies does not demonstrate the impact of domestic politics. Indeed, it is consistent with unitary actor models of foreign policy in which the preferences and strategies of the unitary actor change at a few points in time. The author does not use his evidence of debate on policy toward West Germany to test a theory of how disagreements within the five leaderships affected policy. Such a theory would have to show how the institutional structure or nature of power in a political system was likely to produce particular policies. Harry Gelman's *The Brezhnev Politburo and the Decline of Détente* (1984) is an example.

Indeed, the author at times falls victim to the unitary actor model himself in constructing explanations for puzzling policies. The decision to deploy the SS-20s in the late 1970s, when the Brezhnev leadership had committed itself to a strategy based upon the advantages of economic and political détente, seems irrational, given the likely effect on West Germany of an enhanced Soviet nuclear threat aimed directly at it. Rather than exploring how the contradictory imperatives of a diverse coali-

tional leadership might contribute to self-defeating policies, the author attributes the SS-20 decision to military modernization, the desire to promote U.S.–West German conflict and gain further political advantages from Soviet military power, and a calculated decision to keep the level of military confrontation in Europe high in order to prevent any warming in West German–East European relations (pp. 266–69). This analysis is plausible and may be correct; but it presents a consistent and largely unified leadership and explains Soviet policy by seeking to make the leadership's reasoning coherent.

In the end, these problems are due to the enormous task of sustaining a theoretical framework while documenting and analyzing a complex area of foreign policy in two closed societies for a period of nearly 20 years. As a result, the theoretical issues too often become submerged in the empirical chapters that focus on Soviet and East German official policies and elite debates. The research does sustain the author's primary contention. To understand what these two countries did outside their borders in Europe, one must understand what went on within the borders.

Harvard University CELESTE A. WALLANDER

Encountering the Dominant Player: U.S. Extended Deterrence Strategy in the Asia–Pacific. By William T. Tow. New York: Columbia University Press, 1991. 540p. $55.00.

In a well-written and well-researched book, the author presents an elaborate argument that the United States has too much projected NATO strategic concepts to the Asia–Pacific region. He concedes that South Korea may have been as geographically vulnerable to a Moscow-directed tank attack over the decades as West Germany but notes that the rest of the region has not been beset by threats requiring counterthreats of U.S. nuclear escalation. In light of this, according to Tow, the region has too often simply been used as an auxiliary to concerns about NATO (as with various versions of the "maritime strategy" and "horizontal escalation" and the playing of the "China card").

The author's argument might have been made more tightly, and presented more rapidly and effectively, in a journal article; his overarching logical structure is repeated quite often through the book and may thus inferentially be assigned somewhat more significance for U.S. policy planning than is warranted. Above all, the book (like almost all the books now on the shelf) is overtaken by events—by the collapse of the Soviet Union and by the U.S. "dominant player" role in liberating Kuwait and by the burgeoning fears of nuclear weapons proliferation to North Korea alongside the rapid evolution of the entire Korean situation.

Tow's analysis is basically that *other* security concerns, *or* economic concerns that do not translate into military security problems, have been more important than the deterrence of Soviet attacks as we have tried to optimize U.S. policy for the Pacific rim. The book draws in a great deal of detail and analysis on this region over the entire Cold War period and handles it in a very balanced and persuasive way. Tow also nicely sorts out a fair amount of what we think of as theory on international relations and American foreign policy. Nonetheless, it might be that the U.S. government has already long perceived that, except for Korea, it had no great need for extended nuclear deterrence in the Pacific. In that case, the book may at times be attacking a straw man.

University of Maryland GEORGE H. QUESTER

Securing Europe. By Richard H. Ullman. Princeton: Princeton University Press, 1991. 183p. $19.95.

Richard H. Ullman has boldly ventured a book on the future of European international politics, with the general thesis that, *pace* John Mearsheimer, we will not miss the Cold War. In his admirably lucid and brief book, Ullman argues that "the era Europe has entered will be qualitatively different from those it has known before" (p. 139). Although the international system remains anarchic, among the developed countries the causes of war have become muted and the incentives for peace have increased. Arrangements are within grasp that would render wars growing out of either aggression or arms races and the security dilemma extremely unlikely. Furthermore, the end of bipolarity and the Soviet threat mean that peace is divisible. Conflict in the Third World, Eastern Europe, and within the Soviet Union need not disturb the peace among the OECD countries. With Germany unified, all the countries of Western Europe are now satisfied with the status quo. The revolutionary changes within the Soviet Union and that country's abandonment of ideologically driven need for satellites to the East and fear of the West have rendered that country, too, little desirous for any change that might involve violence. Even without the unifying fear of the Soviet Union, the countries of Western Europe have nothing to fight about. Furthermore, existing institutions (NATO, the European Community) not only provide a framework for mutual economic and political benefit but also permit each state to see that the others are not about to threaten it. The fact that these countries are all liberal democracies contributes mightily to their good relations. The only remote danger Ullman sees is the possibility that other forms of government would appear, especially in Germany, as the result of a severe international depression (pp. 35, 41, 105).

The prospects for the Soviet Union and Eastern Europe are not so rosy—though even here Ullman is not as pessimistic as many other analysts. The ideas and structures of the Conference on Security and Cooperation in Europe and the West European Union could be used to construct a European Security Organization (ESO) that would include the United States and Canada. The United States would have a significant role in Europe, albeit not as unnaturally large a one as it played during the Cold War. Inspection, both formal and informal, could produce confidence and security. Promises of collective security would be credible, in part because there is little danger that a powerful aggressor will arise. One cannot, however, exclude the possibility of "an outbreak of warfare between Hungary and Romania or Greece and Turkey, or between two Yugoslav republics," in which case the ESO might find armed intervention excessively costly and settle for "walling off" the conflict instead (p. 68).

Because "only democracies share a commitment . . . to the rule of law and to the peaceful settlement of disputes," countries with other forms of government would be excluded from the ESO (p. 76). But (as Ullman

notes but might have discussed at greater length) excluding the Soviet Union or successor republics if it or they are undemocratic would create problems (p. 77). Together the developed countries could try to limit arms proliferation in, and terrorism from, the Third World and should maintain sufficient military force for some military interventions; but it would be neither necessary nor desirable for the ESO to try to police the Third World. Conflicts there, however unpleasant for the people concerned, are not likely to "spill over to engulf the advanced, industrialized states" (p. 135).

I find this optimism quite believable, but Ullman could have devoted more attention to things that might go wrong. First, an international depression could undermine democracies and create conflict. The role of Japan (excluded from this book) would need to be considered. Second, Ullman may be too sanguine about prospects for democracy in Eastern Europe and the Soviet Union. Third, keeping conflicts among these countries from spreading to Western Europe may be more difficult than Ullman expects, especially if Germany develops strong interests there. Indeed, the economic investment that everyone calls for, while perhaps reducing the chance of violence and war, would render more difficult a "walling off" strategy in the event its pacifying influence fails. Finally, the security dilemma may be troublesome in the absence of the Cold War, especially if each country begins to worry about becoming the target of pressure from a coalition of neighbors.

As Yogi Berra said, "Prediction is difficult, especially about the future." Time will tell whether Ullman's optimism is warranted. But he has made a strong case for it.

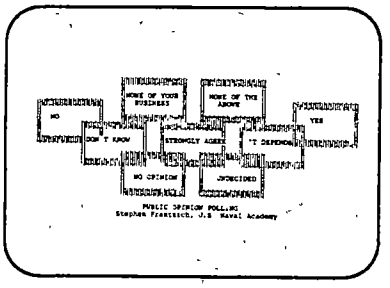
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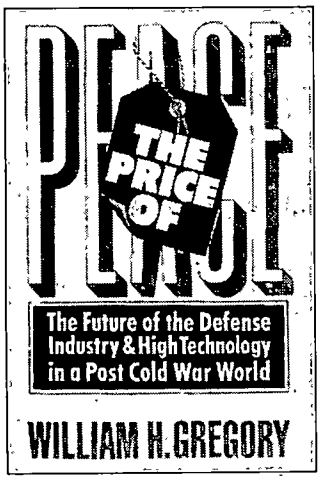
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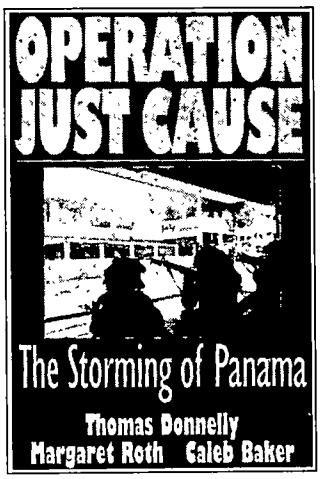
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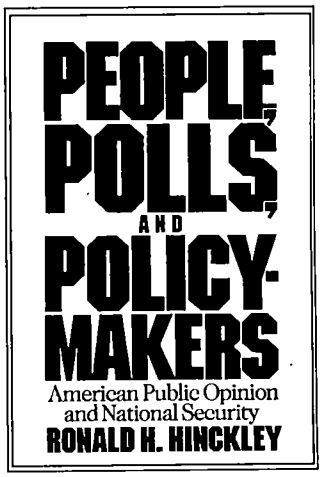
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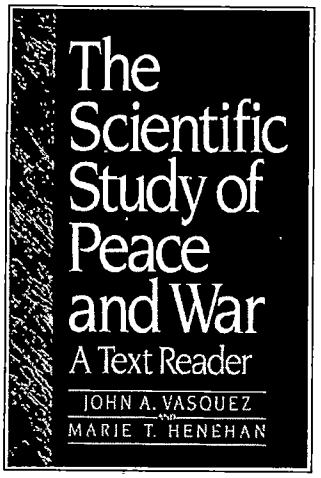
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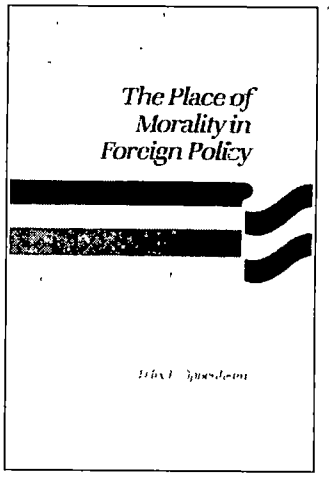
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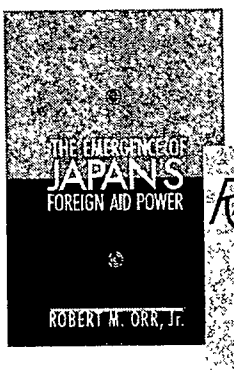
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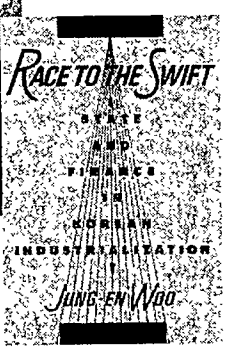
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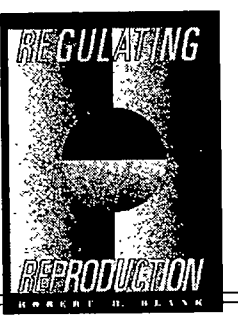
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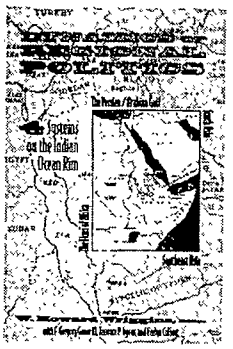


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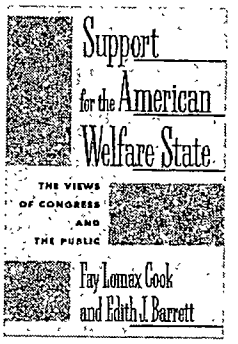
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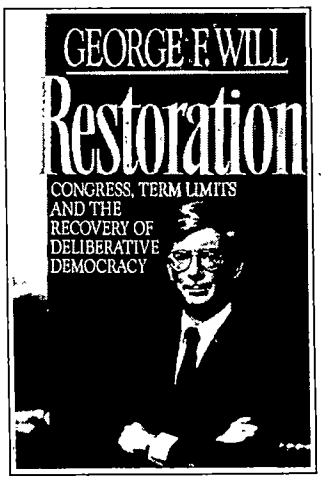
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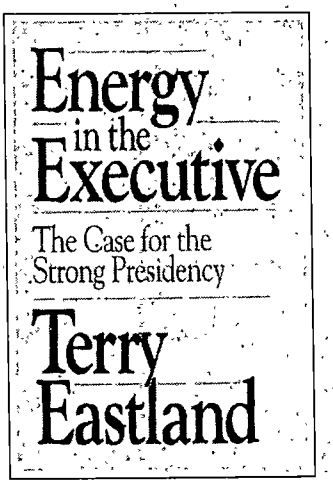
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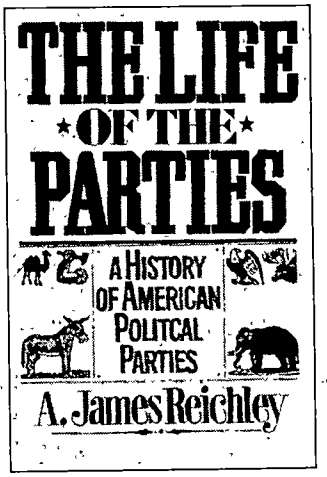
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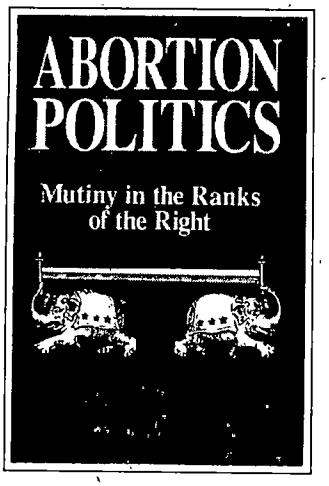
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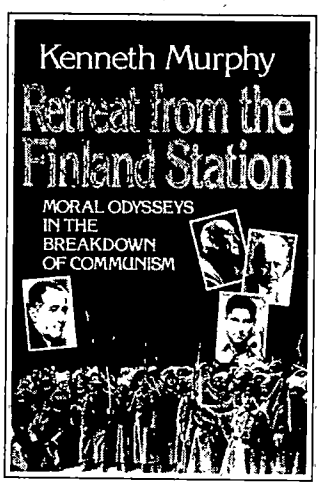
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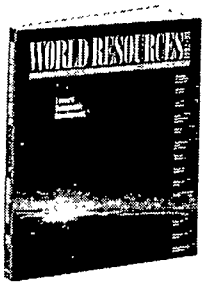
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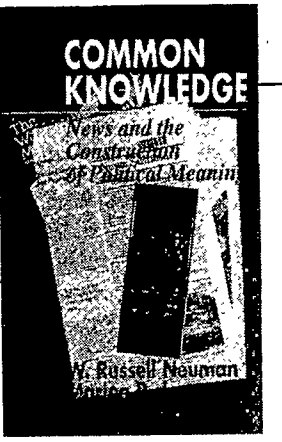
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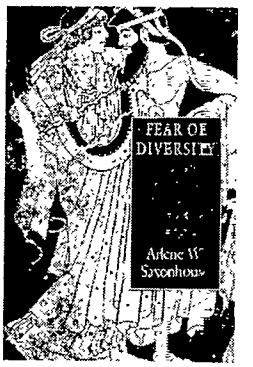
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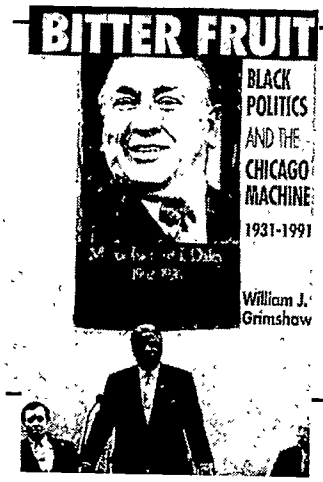
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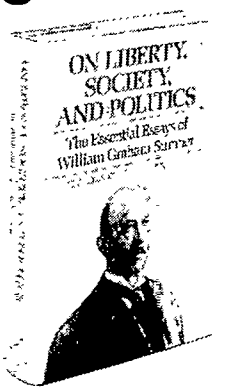
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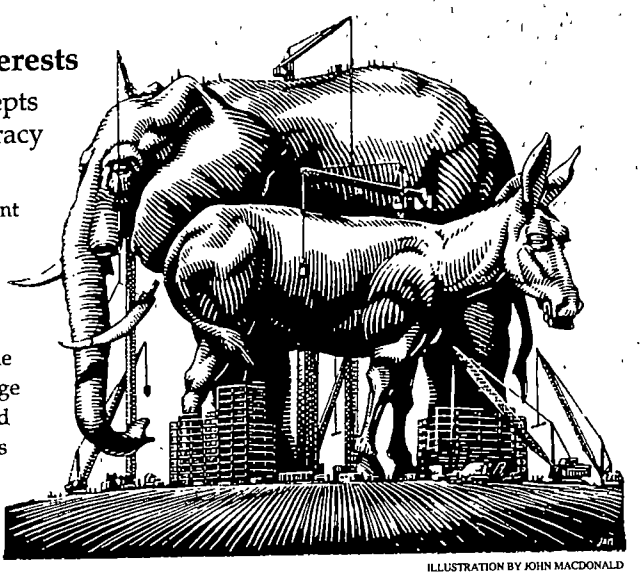


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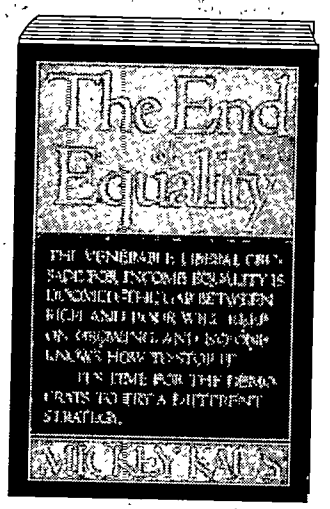
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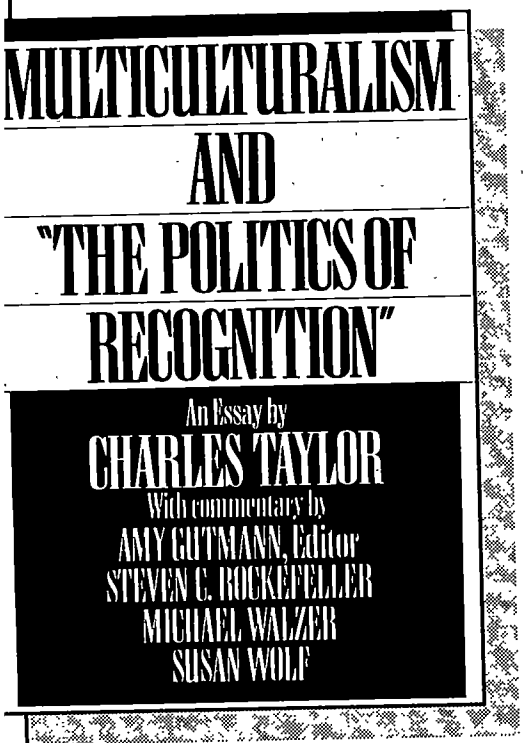
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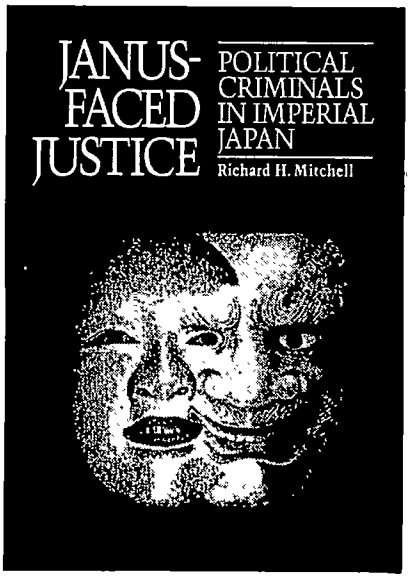
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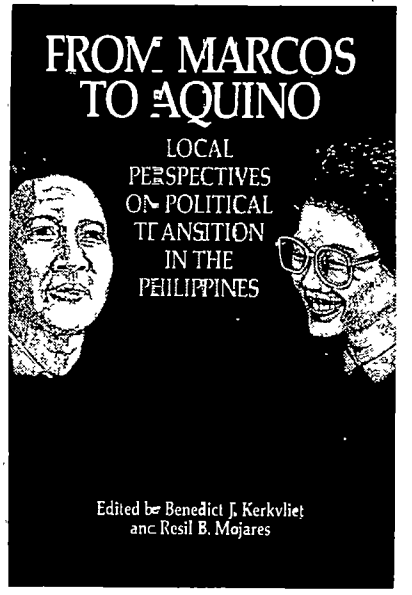
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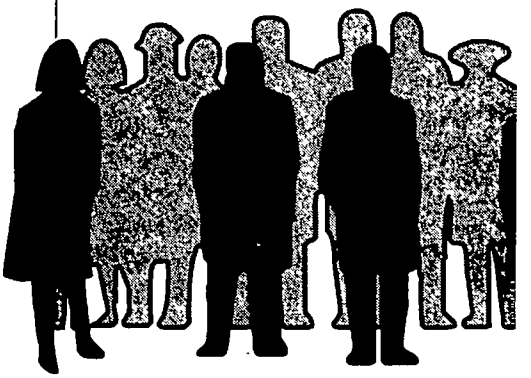
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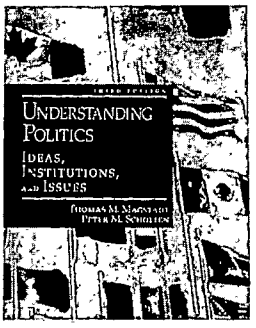
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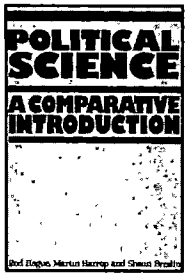
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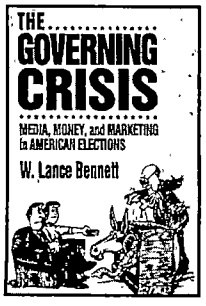
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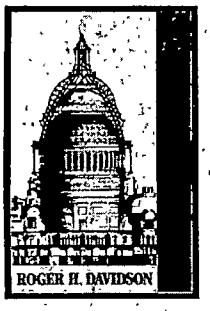
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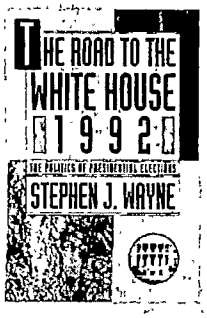
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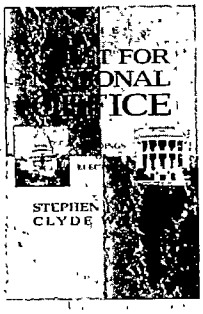


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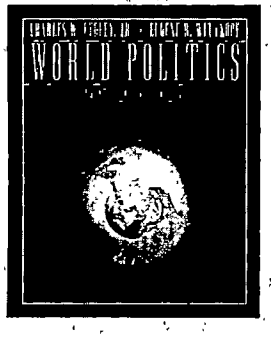
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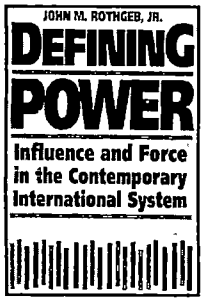
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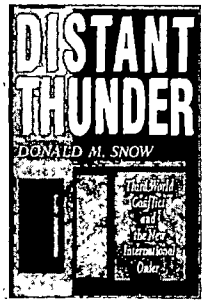
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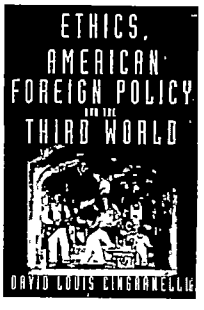
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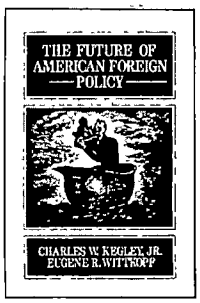
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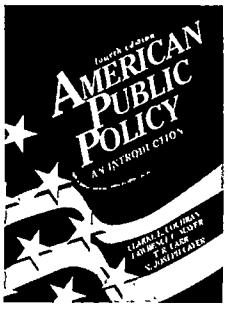
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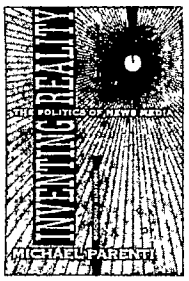
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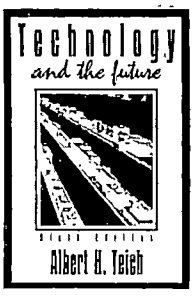
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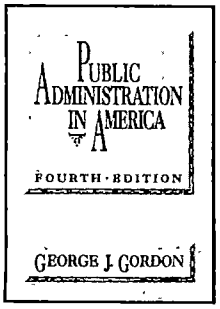
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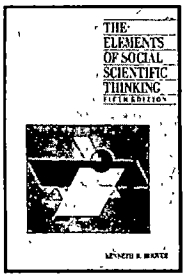
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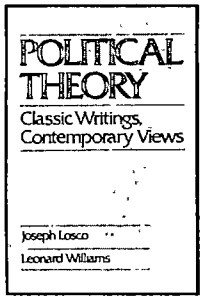


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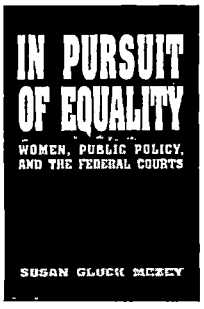


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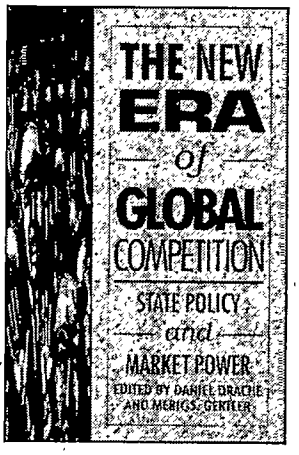
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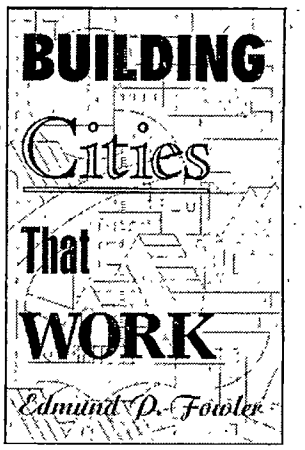
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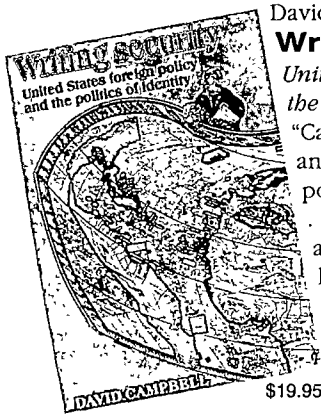
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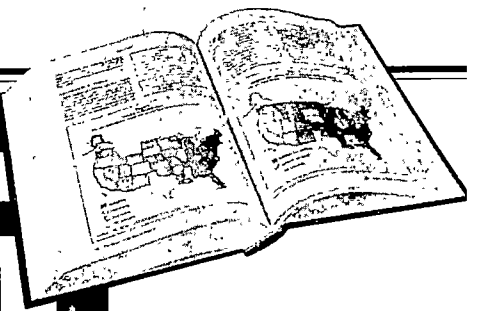
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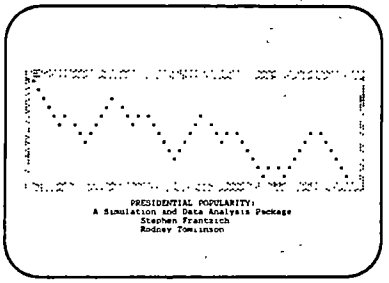
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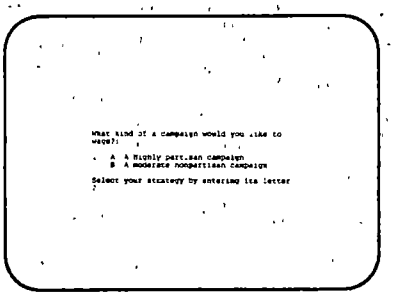
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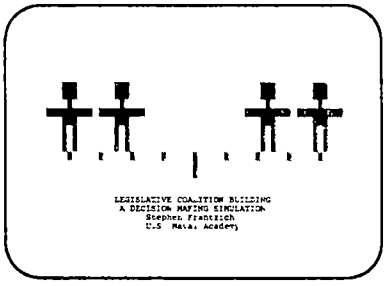
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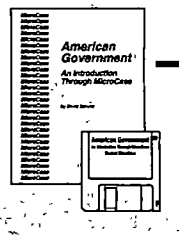
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IS THE CURRICULUM BIASED?

A STATEMENT OF THE NATIONAL ASSOCIATION OF SCHOLARS

American higher education is facing widespread demands to eliminate the allegedly "Eurocentric" and "patriarchal" bias of the curriculum. While the details vary from campus to campus, these demands tend to focus on four objectives:

- that the "canon" be revised to include more works by blacks, other ethnic minorities, and women
- that the "issues of race, gender, and class" be introduced into a greater variety of courses
- that more courses in women's studies and minority studies be developed
- that courses in women's studies and/or minority studies be required of all undergraduates

Various justifications are commonly proffered for making these changes. It is alleged that:

- works by minorities, women, and Third World authors have been excluded from the curriculum
- minority and female students feel alienated and their educational progress is retarded by being asked to study works primarily by white males
- in order to overcome their own prejudices, white males must become acquainted with the cultures and problems of minorities and with the perspectives and problems of women
- the traditional curriculum represents the hegemony of Western culture, covertly supports a status quo inherently oppressive of women and minorities, and is unfairly imposed on students from different cultures
- the traditional desiderata of truth, objectivity, and critical intelligence can be met only by adding the perspectives of women and minorities and by facing up to the new questions they raise
- an increasingly diverse society and interdependent world require that our citizens gain greater understanding of different cultures

The National Association of Scholars disputes the first five of these arguments and believes that the last entails something other than the changes being proposed.

First, any work, whether formerly neglected or widely known, should be added, retained, or removed from the curriculum on the basis of its conformance to generally applicable intellectual and aesthetic standards. A sound curriculum cannot be built by replacing those standards with the principle of proportional representation of authors, classified ethnically, biologically, or geographically.

Second, the idea that students will be discouraged by not encountering more works by members of their own race, sex, or ethnic group, even were it substantiated, would not justify adding inferior works. Such paternalism conveys a message opposite to the one desired.

Third, other cultures, minority subcultures, and social problems have long been studied in the liberal arts curriculum in such established disciplines as history, literature, comparative religion, economics, political science, anthropology, and sociology. But more important, mere acquaintance with differences does not guarantee tolerance, an ideal Western in origin and fostered by knowledge of what is common to us all.

Fourth, the idea that the traditional curriculum "excludes" the contributions of all but males of European descent is patently false. From their beginnings, Western art and science have drawn upon the achievements of non-Western societies and since have been absorbed and further enriched by peoples around the globe. That the liberal arts oppress minorities and women is yet more ludicrous. Even if the curriculum were confined to thought strictly European in origin, it would still present a rich variety of conflicting ideas, including the very concepts of equality and freedom from oppression invoked by those who would reorient the curriculum.

Fifth, while diversity of background is valuable to the discussion of issues to which those differences are germane, objectivity is in general not enhanced but subverted by the idea that people of different sexes, races, or ethnic backgrounds necessarily see things differently. The assertion that cognition is determined by group membership is itself an example of stereotypic thinking which undermines the possibility of a true community of discourse.

Sixth, the study of the traditions and achievements of other nations and of ethnic subcultures is important and should be encouraged. But this must proceed in a manner that is intellectually honest and does not serve as a pretext for inserting polemics into the curriculum. Furthermore, "multicultural education" should not take place at the expense of studies that transcend cultural differences: the truths of mathematics, the sciences, history, and so on, are not different for people of different races, sexes, or cultures, and for that reason alone their study is liberating. Nor should we further attenuate the study of the traditions of the West. Not only is knowledge of those traditions essential for any evaluation of our own institutions, it is increasingly relevant to our understanding of other nations, which, in striking testament to the universality of the values they embody, are rapidly adapting Western practices to their own situations.

The **National Association of Scholars** is in favor of ethnic studies, the study of non-Western cultures, and the study of the special problems of women and minorities in our society, but it opposes subordinating entire humanities and social science curricula to such studies and it views with alarm their growing politicization. Efforts purportedly made to introduce "other points of view" and "pluralism" often seem in fact designed to restrict attention to a narrow set of issues, tendentiously defined. An examination of many women's studies and minority studies courses and programs discloses little study of other cultures and much excoriation of our society for its alleged oppression of women, blacks, and others. The banner of "cultural diversity" is apparently being raised by some whose paramount interest actually lies in attacking the West and its institutions.

We urge our colleagues to demand clear explanations and cogent arguments in support of the proposals being so rapidly brought before them, and to reject any that cannot be justified. The curriculum is and should be open to change, but we must rebut the false charges being made against existing disciplines. We must also reject the allegations of "racism" and "sexism" that are frequently leveled against honest critics of the new proposals, and which only have the effect of stifling much-needed debate.

For copies of this or other NAS statements, or for additional information about the **National Association of Scholars** and its activities, including its fellows program, research center, speakers bureau, search service, newsletter, state and regional affiliates, conferences, local events, and the quarterly *Academic Questions*, write to the National Association of Scholars, **575 Ewing Street, Princeton, NJ 08540**, or call **609-683-7878**.

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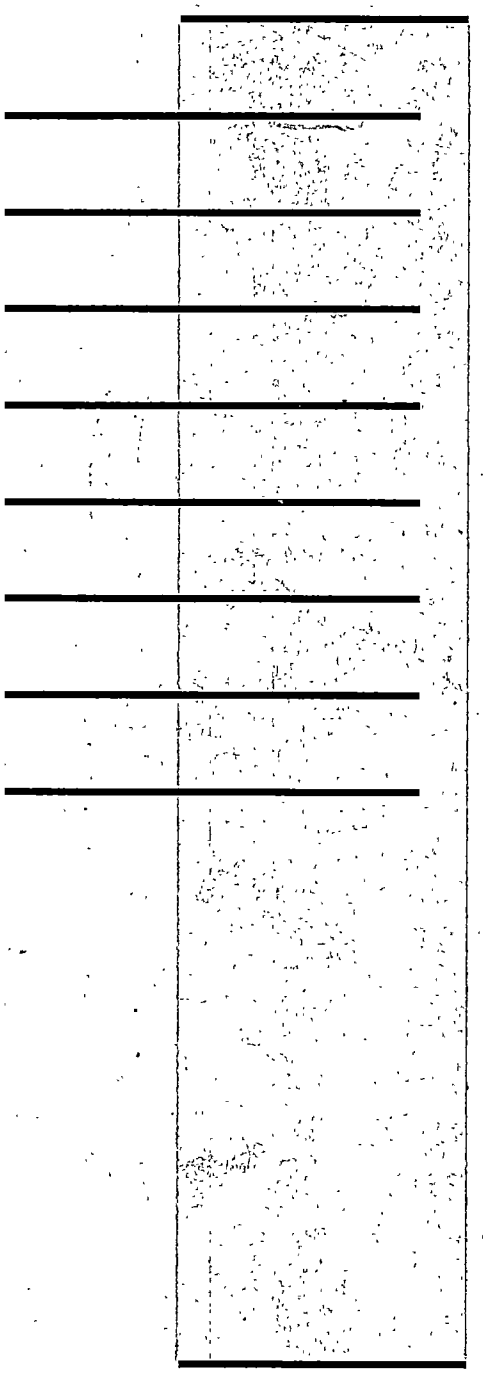
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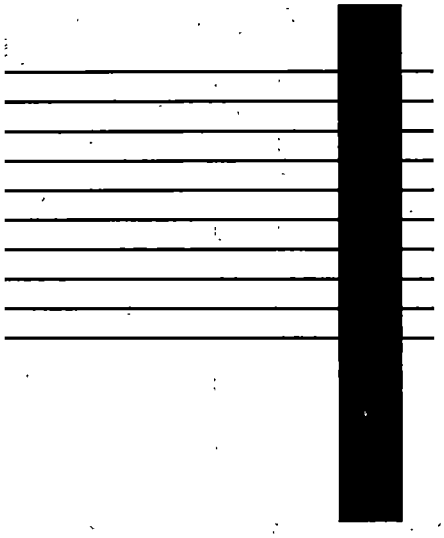
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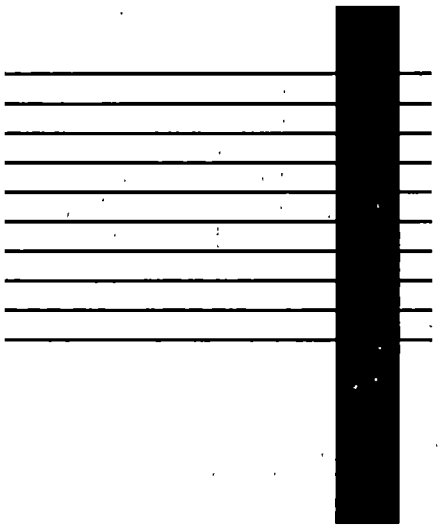
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ARTICLES

REDEFINING THE POLITICAL SYSTEM OF THE USSR: MASS SUPPORT FOR POLITICAL CHANGE

ADA W. FINIFTER *Michigan State University*
ELLEN MICKIEWICZ *Emory University and The Carter Center*

Using data from a national public opinion survey carried out in the Soviet Union during November and December 1989, we explore two attitudes relevant to the revolutionary changes there: (1) attitudes toward change and political democracy and (2) attitudes toward a core component of socialist ideology, the locus of responsibility for social well-being (the state or individuals?). These variables are unrelated, with the sample relatively evenly divided among the intersecting cells of a cross tabulation. While social conflict may be mitigated by the small sizes of absolutely opposing groups, consensus may also be hard to reach. Ethnicity, education, income, age, party membership, and life satisfaction have important effects on these attitudes. We discuss how attitude patterns in our data may be related to the disintegration of the Soviet Union and to problems faced by the independent successor states as they develop new institutions and foster new values.

In December 1991, the Soviet Union ceased to exist. A loose association, the Commonwealth of Independent States, represented the efforts of leaders of 11 newly independent states to retain a measure of continuity and to cushion the shock, both economic and political, of the sudden severing of ties. To many observers, the viability of the Commonwealth, without a decision-making body or regulatory and enforcement powers, appeared highly problematic even at its creation.

We report data on public attitudes toward change and political democracy, collected in seven of the former Soviet republics before the breakup of the Union. However, the questions we asked and the responses to them are not only closely related to the collapse of the former Soviet Union but are also very much a part of the post-Soviet future. All the newly independent states will have to address the attitudes of their populations to fledgling institutions and restructured values. All other things being equal, institutions and values that are supported by appropriate public attitudes will have higher prospects of survival. Difficulties in institutional development in areas where public support is low will be magnified. Each of the new states will, in short, have to confront the continuing public attitudes and beliefs with which they are beginning their political lives.

Further, our survey of attitudes toward the end of the life of the Soviet Union illuminates the profound impact of ethnicity on all the aspects of political change we studied. This variable is particularly important in the analysis that follows and underscores and prefigures the divisions that were later ratified. The depth of these divisions suggests not only a precarious outlook for integrative institutions, but also suggests difficulties for the large number of ethnic Russians living outside their home republic.

SUPPORT FOR CHANGE AND REFORM IN THE SOVIET UNION

In the historical experience of the last two centuries, movements toward democratization have often been accompanied by an enlarged role for the state as the protector of individual liberties and economic well-being. In these new democratic systems, the mass public was protected from arbitrary uses of power by institutionalized systems of state control that included both means for political responsibility and accountability to the public and the protection of social welfare; the latter typically involved governmental control over the provision of many social services. Many of the Western European democratic socialist movements stem from this sort of historical experience. In established democratic systems, as well, increasing political democracy has frequently meant an enlarged role for the state in the provision of social welfare; the historical experience of the United States since the New Deal period in increasing economic welfare and civil rights follows this pattern.

At present, there are important movements toward decreased state activity in many democratic nations. While these are frequently cited as part of a worldwide movement toward "privatization" that includes the democratic revolutions in the former Soviet Union and the nations of Eastern Europe, the differences between these political contexts are far more important than the similarities. The historical experience of the Soviet Union and the Soviet-type states of Eastern Europe is different in a fundamentally important way: the increased state control that occurred as the political regimes of these nations were formed was not part of a movement toward increased democracy but, rather, of a system of centralized control and repression.

In the Soviet Union (though the record of Gorbachev's perestroika is mixed), an important compo-

nent was a *smaller* role for the state. Shortly after his accession to power, Gorbachev took such steps as increasing centralization in the agriculture and food industry, restricting unearned income, and instituting the antialcohol campaign, all of which tended toward a greater role for the state. The course was then reversed, and the drive for pluralism began. "Informal" organizations increasingly entered the political arena, breaking state- and Party-run monopolies. The electoral process became, in 1989, genuinely (even if not fully) democratic. Media outlets were rapidly shorn of the tight constraints of censorship and uniformity, though tensions between editors and politicians increased markedly between late 1990 and the August 1991 putsch.

In the economic sphere, official rhetoric included support at the highest level for a market economy. Gorbachev's chief advisor, Alexander Yakovlev, considered the market as necessary for both socialist and capitalist outcomes. The ability to contract directly with foreign businesses was no longer the monopoly of the Moscow ministry; and the ministries were, in any case, in the process of downsizing. The notion of the joint venture, an arrangement to introduce foreign capital and techniques at all levels of the economy, became part of the message of perestroika. If the economy was not revolutionized until after the failure of the August 1991 coup, the policy pronouncements were, nevertheless, much in evidence earlier. Both political and economic reforms (with the political gaining much greater ground than the economic) chipped away at the power of the centralized state and the Party. The attempt of the opponents to regroup and reverse the course of reform may have convinced Gorbachev in 1990, but they ultimately failed in their bolder move the next August.

As noted, Gorbachev's pronouncements, particularly in the early stages of perestroika, were indeed ambiguous; and the tensions surrounding a move to a radically individualist political economy remained unresolved throughout his period of leadership. However, there can be no doubt that a major thrust of reform in the Gorbachev era was, *as compared to what went before,* toward decentralized government, more individual initiative, and greater individual responsibility. These ideas were frequently implied in discussions of "marketization" and greater individual "initiative" and "productivity." These are the ideas that we intended to capture in our survey with our question on state versus individual responsibility.

Because of Soviet history, our measures and our discussion of support for political change take the form of a focus on support for increasing political democracy (which is unexceptional) but also of support for greater responsibility for *individuals* in social welfare as opposed to the continuation of the state's playing the major role in this area. In this respect, our discussion is different from discussions of democratization that assume that this process will involve a larger role for the state. Given the historical and political context, this is a specification that seems to be necessary for theories of democratic development.

Who Supports Political Change and Reform?

Beliefs about the role of the state, group identifications, and personal self-interest are among the most prominent bases of political attitudes. In particular, the understandings held by a nation's citizenry about what role the state should play in the political and economic life of the nation, how far political dissent may be expressed publicly, how important order and stability are when these come into conflict with freedom, and other such basic aspects of the relationship between the public and its political leadership are major parts of the political culture of a nation. Typically, attitudes based on the prevailing political culture tend to remain stable unless cultural upheaval and discontinuity force people to take account of change (Eckstein 1988). Depending on the sort of change that is occurring and the fundamental motives that one believes drive political attitude formation, different hypotheses follow about the social sectors in which one expects support for change to develop most rapidly.

In change oriented from an authoritarian to a democratic form of government or a command to a market economy, attitudes toward change might be driven by beliefs about the role of the state and by personal self-interest. The nature of the change being undertaken is germane to the types of relationships that should be expected, as are the political and social contexts in which change occurs. In general, cultural theories of attitude formation suggest that attitudes supportive of democratization will develop most rapidly in the nontraditional sectors of the nation—that is, among the young and better-educated; in the cities, rather than in the rural areas; in the service, rather than the agricultural, sectors of the economy; and probably among men, as compared to women.

Other theories of attitude formation occasionally suggest contrary—or at least supplemental—hypotheses. For example, to support a market economy in the Soviet Union and its successor states represents a reversal of seventy years of ideological indoctrination. We would not expect people who were ideologically invested in the current system to be very supportive of this type of change, regardless of any "modern" demographic characteristics they might possess; at the least, their support would be tempered by the conflict. Similarly, the market economy and the economic dislocations unleashed by perestroika threaten the self-interest of some groups. Low-income people may be expected to want to retain the support and material benefits provided by the socialist state, regardless of any other "modern" characteristics they may possess, such as residence in large urban areas.

Our analysis takes into account the various theoretical problems and considerations we have discussed. First, we think attitudes toward economic and political change themselves must be clarified because the complexity of change suggests that these may not be unidimensional. Second, to the extent that we can identify a cluster of attitudes that are

TABLE 1

Attitudes on Various Aspects of Change and Reform (%)

QUESTION	REFORM POSITION	"IT DEPENDS"	TRADITIONAL POSITION	TOTAL	WEIGHTED N
Competitive elections (Q29)	95	—	5	100	(1,883)
Income differences (Q22b)	87	—	13	100	(1,910)
Free speech vs. order (Q22d)	58	—	42	100	(1,841)
Individual vs. state responsibility (Q22a)	49	—	51	100	(1,822)
Rapid vs. slow change (Q18)	45	17	38	100	(1,885)
Protest vs. traditional methods (Q22c)	36	—	64	100	(1,826)

Note: Ns are in parentheses. The total weighted N for the sample is 2,006. Missing data ranged from 5% to 9% in these questions. A middle position was available only in Q18.

oriented toward reform, we would expect greater agreement with such values in groups which belong to the "modern" sectors of society. Third, however, we do not believe support for democracy is based only in ideas or ideology; we also take into account how far groups can be expected to benefit from political and economic change. In groups that are part of the more traditional sectors, uniquely disadvantaged by change, or ideologically invested in the present system, we expect to find more resistance to modernizing and democratizing social, political, and economic change. Indeed, it is in such sectors that we expect to observe real resistance to reformist values. Thus, our analysis proceeds from assumptions rooted in both political culture theory and considerations of material and organizational self-interest.

The Political Context of the Survey

Largely unintended and unforeseen, what began in 1985 as perestroika resulted in revolutionary change. Glasnost, most observers concur, despite the tension and conflict over its scope, irreversibly altered the boundaries for the expression of opinions, although in the post-Soviet politics of the successor states (as before), there are wide differences from west to east. The political reforms (in which the monopoly of Communist Party power gave way first to splits in the Party itself and then to the presidential/parliamentary system amalgams in the now independent republics) continue to be implemented with varying degrees of rapidity and commitment. The Gorbachev administration's rhetoric of economic reform, first couched in conservative and ineffectual proposals, approached market principles, then reversed direction before the attempted coup. In some areas, such as the largest cities and the Baltic republics, reform proceeded far more rapidly than in the provincial heartland; and the increasing de facto decentralization of the country's political and economic systems has also been accompanied by severe economic deterioration and interethnic conflict. The main beneficiaries of the older system—the armed forces, the security organs, and the professional bureaucrats (the apparat) of the Communist Party and its remnants—have been least willing to see their power and resources eroded.

It is in the context of rapid and radical change that our survey of Soviet public opinion was designed and conducted. One of our aims was to explore the attitudes of our respondents to the tempo and components of political change. Our analysis is based on a national sample of interviews collected during late November and early December of 1989. These were true personal interviews and not the "drop off, pick up" questionnaires sometimes referred to as "face-to-face interviews" in the Soviet Union; this is the first large-scale true personal interview survey in that nation. The data were collected by the All-Union Center for Public Opinion Research, directed by T. I. Zaslavskaya. Further details on the sample and field procedures are given in Appendix A.

ATTITUDES TOWARD CHANGE: DIVISION AND IDEOLOGICAL COHESION

We focus here on six questions designed to measure attitudes about changes taking place in the Soviet Union at the time of our study. One question (Q18) is about the pace of change itself, and five more specific questions ask about change in the political and economic arenas: the perceived locus of responsibility (individual or collective) for individuals' social well-being (Q22a), the acceptability of differences in individual incomes and standards of living (Q22b), the acceptability of nonconventional forms of political expression like strikes and demonstrations (Q22c), the priority of free speech over public order (Q22d), and support for a competitive electoral system (Q29). The back-translation from Russian for these questions is given as Appendix B. (R) next to a response indicates that it was scored as "reformist" or "change-oriented." Except for Q18, which had a middle position ("In different situations, act differently"), all questions were dichotomies. The distributions on these questions highlight the substantial

disagreement that existed in the public on some issues that were important aspects of glasnost and perestroika (see Table 1).

As Table 1 makes clear, depending on which particular question one looks at, one can come to quite different conclusions about the level of support for change that existed prior to the dissolution of the USSR. To begin with, there was substantial disagreement on how fast change should be carried out. In the realm of political expression, practically no one objected to competitive elections. Nevertheless, apart from this very simple—albeit basic—measure of democratization, it was clear that old ideas were dying slowly; more subtle concepts about political rights were not universally approved. Two-fifths of our sample believed that public order was more important than free speech, and only slightly more than a third were willing to accept unconventional forms of political participation as a legitimate part of the political process.[1] Still, while a clear majority preferred to solve problems using conventional means, considering the strength of the wording of the question about protest, the amount of support for strikes, protests, and demonstrations was remarkable.[2] Nevertheless, the ambiguity and conflict over the extent of permissible political conflict signals the fragility of the current order.

Relatively few people were hard-core believers in strict income equality (about an eighth of our sample). Note that strict income equality has really not been a feature of official political doctrine. Stalin and all subsequent leaders asserted the rationale for wage differentials and there was a brief period of loosened reins on entrepreneurial activity during Lenin's New Economic Policy (NEP). However, as David Lane notes, "until the advent of Gorbachev's leadership, differentials were relatively low" (1990, 32); and there has been an official bias against too large a spread of income differences and against unearned income. Indeed, directives relating to the latter, even under Gorbachev, seriously impeded the development of individual economic initiative. It is also possible that the relatively low popularity of income equality in our sample may have been encouraged by the wording of our question, which accorded the *state* the function of providing opportunities for individuals to earn as much as possible. In retrospect, the question probably should have emphasized the exercise of individual initiative for making as much money as possible, rather than state sponsorship of these opportunities.

Other aspects of the economic "old order" are clearly more durable. Most striking, there was virtually an even division in the sample between individual and state responsibility for individual well-being. Since this is a central aspect of what change in the political and economic systems of the former republics is all about, the lack of consensus on this item suggests the potential for serious divisions in the former Soviet states.

It is also evident that there is not one cohesive ideology that ties together all of these ideas about political and economic reform, although we do find a cluster encompassing five of the six items. Correlations involving the question about state or individual responsibility for social well-being are particularly interesting. Table 2 presents the relationships among all six questions used as dependent variables. Many of these correlations are consistent with our expectations. For example, those who support free speech are also likely to approve of unconventional forms of political participation. However, being a "political democrat" or wanting rapid change does *not* necessarily mean that one is eager to abandon the socialist system by opposing a major role for government in caring for individuals. In fact, the reverse tends to be true. For example, people who approve of protest are *more* likely than those who disapprove to see individual well-being as a state, rather than an individual, responsibility. Moreover, those who want rapid change are *more* likely than those who want slow change to see individual well-being as a state responsibility. Indeed, the question on individual versus state responsibility, considering the "individual" response as the more change- or reform-oriented attitude, is negatively related to each of the other items under consideration. Although these correlations are modest, the data suggest that support for many aspects of change and political democracy is unrelated to the posture toward collective versus individual responsibility for the social welfare of citizens.

TABLE 2

Relationships among Attitudes toward Political Change

QUESTION	Q22A	Q22B	Q22C	Q22D	Q29
Rapid vs. slow change (Q18)	−.19	.18	.29	.25	.55
Individual vs. state responsibility (Q22a)	—	−.03	−.14	−.05	−.13
Income differences (Q22b)	—	—	.60	.55	.41
Protest vs. traditional methods (Q22c)	—	—	—	.76	.60
Free speech vs. order (Q22d)	—	—	—	—	.59
Competitive elections (Q29)	—	—	—	—	—

Note: Values are gammas.

At a theoretical level, these unexpected negative relationships demonstrate that the ideology of change must be understood in its specific political context. A 1980s American perspective that equates political democratization with a limited role for central governments (whether all-Union as in the former USSR or in the newly independent states) may be considerably off the mark from another geographic or temporal perspective. Nevertheless, it remains true that even in the specific case of "perestroika," our results are unexpected. Since perestroika was generally understood to include *both* political democracy and a move toward marketization and smaller cen-

tralized government, responses to the questions we used might have been expected to be unidimensional. It is clear that they are not. Perestroika was a complex, often contradictory cluster of ideas; and some aspects were clearly more popular than others. Furthermore, while there was strong support for political reform in the Soviet Union, this did not mean that all the ideas to which people had been socialized since the Russian Revolution would be equally changeable in the short term, even when new ideas that may appear inconsistent with some of them are introduced. The Soviet population had been exposed—subjected—to 70 years of official doctrine about the primacy of the state and collective approaches to problems. As Table 1 makes clear, this socialization was far from uniformly effective. But such ideas as people had developed about this subject were clearly not immediately changed into wholesale support for individualism and free enterprise even though there was a common recognition that the system had not worked efficiently and institutions supporting increased political and economic democracy had begun to develop. Clearly, political culture and socialization will inevitably provide a drag on attitudinal change.

That many advocates of change continue to support the socialist view that the state, rather than individuals themselves, is responsible for the well-being of the citizenry, suggests that contrary to much popular belief in the West, there has not been a wholesale abandonment of socialist principles. On the contrary, there may even be somewhat more support for these ideas among those who are the strongest believers in individual free expression and rapid change. What many Soviet citizens pressing for change may want is a more efficient and productive state, rather than a state shorn of social obligations. The Communist Party, as the engine of that obligated state, was rejected, but not the obligation itself; the institution failed, but not the norm. Indeed, the institution's failure may have been due largely to its unwillingness or inability to service the norm; and an important part of Gorbachev's failure as a leader was in his failure to distinguish the two.

The differential support for the various components of perestroika and the negative relationship between locus of responsibility and the other items suggest that a one-item measure of "support for perestroika" (such as appears in some reports of Soviet public opinion) would be quite difficult to interpret meaningfully. Further analysis confirms that Q22a, state versus individual responsibility for social welfare, should not be combined with the other items in a general measure of attitudes toward reform. As the negative relationships presaged, factor analysis suggests that Q22a is orthogonal to the other items; on the factor it dominates, all other items have negative loadings. Therefore, we combine items 18, 22b, 22c, 22d, and 29 into a scale of "support for political change." The alpha-reliability of this measure is .51. In addition, we analyze Q22a separately as a measure of "individual versus collective responsibility." While single items are generally less valid than multiitem indicators, in this case the idea we are measuring—attitudes toward individual versus state responsibility—is quite clear; and the single item has obvious content validity as a measure of that idea. We feel that the content validity and the inherent importance of the idea measured by this particular item warrant its use in our analysis.

For the scale of attitudes toward political change, we scored individuals +1 for each of the five items answered in the "reform," or change, direction, and −1 for each item answered in the "status quo," or traditional, direction. Thus, the scale ranged from −5 to +5. The distribution was heavily skewed toward approval for political reform. Almost 70% of the sample had net positive scores, almost 40% chose the reform-oriented response on three of the five items, and 14% were supportive of reform on all five items. In contrast, fewer than 1% opposed reform on four or five of the items, and fewer than 6% chose the traditional response on three items. Clearly, the battle over reform in the former Soviet Union was not between the reformers and those opposing all change but, rather, among those prepared to accept varying degrees of change.

For cross tabulation, the scale was divided into five categories: −2 through −5, −1 and 0, 1, 2–3, and 4–5. As expected from our individual item results, the relationship between the composite index of attitudes toward political change and the single-item measure of preference for state or individual responsibility for social well-being is negative, although rather weak. The statistics and the cross tabulation of the two variables are presented in Table 3. As we have already discussed, given that reform and restructuring in the Soviet Union were defined to include *both* political democracy and a move toward increased individual initiative, had public attitudes on these issues been similarly formulated, the relationship would have been positive, rather than negative. Instead, the negative correlation indicates that those who are more supportive of political change tend to favor state responsibility more than the opponents of change do and that those favoring individual responsibility tend to be less supportive of political change than those favoring continued state responsibility.

In order to clarify the degree of social and political conflict about these issues, it is instructive to consider the percentages of the total sample falling into each of the cells; and these are the figures shown in the body of Table 3 (the reader can calculate row or column percentages from the information given). Of the total sample, 29% are in the corner cells, representing the greatest degree of disagreement; 4% favor continuation of the socialist ideology of state responsibility *and* wants little or no political change (representing the extreme of those supporting the status quo); an additional 5% also favor little or no political change yet prefers a system favoring individual initiative. On the other hand, 12% favor a great deal of political change but prefer the socialist system of state responsibility, while 8% oppose state responsibility in favor

TABLE 3

Relationship between Support for Political Change and Locus of Responsibility for Social Well-Being (%)

SUPPORT FOR POLITICAL CHANGE	LOCUS OF RESPONSIBILITY FOR SOCIAL WELL-BEING	
	STATE	INDIVIDUALS
−5 to −2	4	5
−1, 0	11	11
1	10	12
2, 3	14	13
4, 5	12	8

Note: Weighted N = 1,785 (100%). Entries are percentages of the total sample in each cell. For the full table (with support for political change not collapsed), Kendall's tau $c = -.10$; gamma = $-.12$; $p \leq .02$.

of individual initiative and also want a great deal of political change. Only the last group really advocates a completely new system, and only the 4% mentioned earlier favor holding on to the status quo. While these totally conflicting groups are small, note that the remaining total cell percentages are relatively uniform, so that any group can substantially increase its size by including the group in the next closest cell. Thus, relatively evenly balanced coalitions of many different political complexions are possible.

As in most complex social processes, our data show that attitude change is uneven. New ideas are differentially accepted, and while a majority appear to favor rapid change, there is also significant conflict over the shape that change should take. Thus, some of the political difficulties and pressures from both right and left that ultimately brought down President Gorbachev—and continue to beset the newer leaders—are clearly illustrated in these data. We would suggest that in a society actively constructing new political and social institutions, with a mobilized public whose attitude structure supports division into relatively equally sized ideological groups, any political or economic institutional developments are liable to be uncertain until attitudes coalesce to a greater extent around both acceptable degrees of change and defining political and economic values.

SOCIAL CLEAVAGES

Up to now, we have discussed divisions in the total population of the former Soviet Union. Now, we move to a discussion of the major social cleavages attributable to a variety of social and demographic factors. Our analysis focuses separately on several major variables because we think it important to understand how particular variables operate to cause social cleavage in the process of change; at the end, we discuss regression equations that combine the most important of these variables.

Ethnicity

As the break-up of the Soviet Union and its reemergence as a number of individual states suggests, ethnicity was among the most important factors in considering support for the kinds of changes initiated by the Gorbachev administration. Our analysis includes the principal ethnicities that fell with sufficient frequency into our sample: Georgians, people from the Baltic states (in our case virtually all are Estonians), Central Asians (principally Uzbeks and Kazakhs),[3] Ukrainians, Belorussians, and Russians.

The differences among the nationalities in support for political change are shown in Table 4. While Russians, Ukrainians, Belorussians, and Balts did not differ very much from each other in the degree of support for political change, Georgians appear to be somewhat more favorable to change than the others, and Central Asians were considerably more resistant to change. In the post-Soviet configuration, the Georgians under Gamsakhurdia became outliers rejecting the notion of even weak Commonwealth ties, while many of the Central Asians have been able to prolong the previous system under new political labels.

The pattern of ethnic responses on the question of state versus individual responsibility for an individual's well-being (see Table 5) reflects the same contradictions that we have just discussed. For example, Georgians very strongly favored the socialist ideology of the state's taking responsibility for the individual; yet, of all ethnic groups, they were the most supportive of political change. The Georgians' response to the question of state versus individual responsibility

TABLE 4

Support for Political Change by Ethnicity (%)

SUPPORT FOR POLITICAL CHANGE	RUSSIANS	UKRAINIANS	BELORUSSIANS	BALTS	GEORGIANS	CENTRAL ASIANS
−5 to −2	7	5	8	3	2	24
−1, 0	20	21	20	19	18	33
1	21	24	23	24	15	20
2, 3	29	34	19	41	32	15
4, 5	23	16	30	13	33	8
Total	100	100	100	100	100	100
Weighted N	(1,119)	(275)	(70)	(42)	(53)	(208)

TABLE 5

Locus of Responsibility for Social Well-Being by Ethnicity (%)

LOCUS OF RESPONSIBILITY	RUSSIANS	UKRAINIANS	BELORUSSIANS	BALTS	GEORGIANS	CENTRAL ASIANS
State	51	54	50	41	77	43
Individuals	49	46	50	59	23	57
Total	100	100	100	100	100	100
Weighted N	(1,043)	(244)	(64)	(39)	(57)	(206)

may well have reflected their attitudes toward an independent Georgian republic, rather than a centralized Soviet state. Ronald Suny (1988), in discussing components of Georgian nationalism, emphasizes the ability of Georgian republic leaders to staff Party and state institutions with Georgians to the exclusion of other nationalities. Well before the Brezhnev policy of *korenizatsia* (nativization), the Georgians had consolidated their hold on the institutions of power and influence in their republic. Suny argues that even though an independent Menshevik state had existed briefly, the most "complete consolidation of the Georgians as a nation came in the first seven decades of Soviet power. . . . [and] the actual evolution of Soviet Georgia resulted in the emergence of a compact and conscious nation" (1988, 318). It is likely that this association of nationalism and the Georgian republic helped to produce the response that people would be better off if one's own (i.e., the Georgian) state provided leadership.

The second anomaly in attitudes toward individual versus state responsibility is exhibited by Central Asians. On the political change scale, they provide by far the strongest support for the status quo, consistent with their more traditional value structure and geographic and psychological distance from the centers of political change. Yet on this question, they differ much less from other ethnic groups (except the Georgians) and display a much greater willingness to place responsibility on the individual than their conservative posture on the political change scale would suggest. It is our view that although Central Asians may be as divided on this variable as the other ethnic groups, an alternative interpretation is more plausible. For some of the most traditional Central Asians the understanding of "individual" in this context may differ from that of other ethnicities. Specifically, it is the case that "individual" for the traditional ethnicities carries with it the sense not of the atomized individual, standing alone, but, rather, the individual "unit," which is, in fact, the traditional family. As Nancy Lubin wrote about Central Asia: "The extended family in pre-Revolutionary Turkestan was both a unit of consumption and production, an independent world unto itself" (1984, 209). Therefore, a response favoring the individual on this question may actually be a response favoring the continuation of the traditional patterns of social organization that have endured in Central Asia (even if in attenuated form on some dimensions) throughout the Soviet period, rather than an indication of support for a change in the current system.

As is well known, the moves for sovereignty, independence, or autonomy on the part of the constituent republics of the former Soviet Union put pressure on the substantial Russian "minorities" (who are often disproportionately found in positions of power and influence) in the non-Russian republics. Some are returning to the Russian Federation; but it is an unlikely solution, considering that at the time of our survey, some 24 million Russians lived outside this republic (Guboglo 1990). This potentially creates situations in which these individuals are exposed to a variety of experiences and peoples that may affect their attitudes in ways not likely for Russians in Russia. Indeed, our data show that the context of the republics strongly affects attitudes toward political change. Thus, we have also explored the impact of republic-of-residence for Russians living outside Russia.

Russians in Central Asia—both Uzbekistan and Kazakhstan—are much less likely to favor political change than are their counterparts in the Russian Federation. Of Russians resident in Russia, 52% score between 2 and 5 on our political change scale. However, only 38% of Russians resident in Kazakhstan and 36% in Uzbekistan were this supportive of change—closer to the 30% of Kazakhs and 23% of Uzbeks (in their native republics) than to their ethnic compatriots, Russians in Russia.

Ukraine is the only other non-Russian republic in which we have a sufficient number of Russians for analysis. Russians living here were *more* supportive of change than those at home: 66% score between 2 and 5 on the scale. Interestingly, while Ukrainians were more supportive of change than the general norm for the sample (51% of those living in Ukraine score from 2 to 5), they were not as supportive as Russians living in their republic. However, our respondents in Ukraine were drawn entirely from the more Russified area of that republic. There were no sampling sites in the western part of Ukraine, where Ukrainian nationalism is most pronounced. Hence, while our sample in Ukraine does not reflect well that much more volatile context among Ukrainians themselves, the attitudes of minority Russians in Ukraine on issues of political change might be less affected by such intrarepublic regional differences. Indeed, in the 1991 vote in the Ukraine on the subject of independence, though there were differences between Rus-

sians and Ukrainians, the divisions were far less than had been anticipated.

We find similar results on the influence of the local context for our question on individual versus state responsibility for social well-being. Among Russians in Russia, 50% choose *individual*. However, in Kazakhstan, where 66% of the titular nationality chooses this alternative, Russians choosing it increase to 58%. In Uzbekistan, where 55% of Uzbeks choose *individual*, relatively close to the Russian "norm" for this question, Russians are very slightly greater in their support for the individual, with 52% choosing this response. In Ukraine, where only 45% of the Ukrainians choose this alternative, only 37% of Russians do so.

In Central Asia, the Brezhnev policy of "affirmative action" (promoting people of the indigenous nationality and permitting preferential treatment) resulted in nepotism in which tribal loyalties and command over resources were expanded virtually into fiefdoms (Lubin 1984; Radio Liberty 1982). Russians in these and other republics perceived not only the erosion of their previously privileged status but also reverse discrimination (Karklins 1987). The resistance to change on the part of both the Central Asians and the Russians who live there may arise from values they share, even though the two communities tend to live separately and follow different career paths (Lubin 1984; Olcott 1987). On the other hand, the sources of resistance to change may have been different for the two ethnic communities, arising from the Central Asians' preference for the Brezhnev regime's complacent outlook on local arrangements, both legal and illegal, and, for the Russians, a general unease with what they perceived to be a continuing decline in their status, especially given the high birth-rate differentials and increasing proportions of the titular nationalities (Olcott 1990). Instances of discrimination against Russians in non-Russian republics broke into the press well before the current clamor about language, independence, and citizenship laws (Mikhailova 1989; Novikova, Ovsyannikov, and Rotman 1989). These issues, added to severe economic deprivation and conflict over status and scarce resources, have exacerbated the problem. For Russians in Central Asia, Moscow does not offer much solace: on political ideology, these respondents remained closer to the Uzbeks and Kazakhs than to their fellow Russians in Russia.

That Russians outside Russia often share a general propensity to accept or reject current political ideas in a fashion similar to that of the titular nationality in those republics (and different from Russians in Russia) is also subject to a more general theoretical explanation. The political attitudes of individuals who change their residential locations tend to be influenced by the political and social context into which they move; such individuals tend to change their attitudes in the direction of conformity with their new neighbors. This phenomenon has been found in diverse research, including studies of Americans moving within the United States (Brown 1981, 1988) and Americans moving to other countries (Finifter and Finifter 1989). The addition of this general contextual effect to the real and perceived grievances of Russians in Central Asia may sharpen even further the attitude differences between Russians in the home republic and Russians in the other republics. If, in the aftermath of the development of the Commonwealth of Independent States, pressures for the removal of Russians back to their home republic increase to intolerable levels, assimilation in Russia may be extremely problematic on political grounds, quite apart from the economic difficulties involved.

In the Baltic area, the situation of the Russian minority is different. The Baltic peoples, who took the lead in pressing for political change within the Soviet Union and were the first to declare independence, do not differ significantly from Russians in Estonia on support for political change. Perhaps surprisingly, on the item about strikes and demonstrations (Q22c), the posture of the Estonians hardly differs from the negative stance of the Central Asians in rejecting this form of political protest; and they are considerably more conservative than Russians living in their republic. Within Estonia, 45% of Russians, but only 18% of Estonians, think strikes and protests are acceptable methods of political participation ($p = <.001$, tau b = $-.26$, gamma = $-.57$).

We think this difference in the degree of approval of protest between Russians and Estonians living in Estonia occurs because of the particular socioeconomic location of Russians in that republic. Because the design of economic development in Estonia and Latvia during the Soviet period involved large inmigrations of blue-collar workers, Russians are now concentrated heavily in the industrial labor force of those more industrialized Baltic republics. There were substantial differences in social class between the ethnically Baltic Popular Fronts and Interfront, the Russian response. In Estonia (the Baltic site for our survey), the use of strikes was more likely to be a Russian, oppositionist, protest instrument than an expression of tolerance for political opposition within the ethnic Estonian community. Consequently, it is the Russians, rather than the ethnic Estonians, who account for most of the approval for protest that occurs in this republic.

Age

We expected age to be negatively related to support for reform efforts geared to social and political modernization and democratization. Virtually by definition, youth is part of the "modern" sector of society, and youth groups are frequently among those active in movements for social and political change; conversely, the relationship between aging and resistance to change is a phenomenon widely observed. In the case of the Soviet Union, the propensity of youth to accept change more readily than older people is probably accentuated by the effect of the Stalin period. A "generational effect" has been very widely discussed in terms of elite recruitment and mass

attitudes (Bahry 1987; Bialer 1986; Hough 1988). Analysts working with data from the Soviet Interview Project (SIP)[4] have drawn a distinction between the generation whose formative experiences occurred during the Stalin regime and the generation that might be termed *post-Stalin*. The latter—defined as those born in 1941 or later—is clearly deviant from the political conformity of previous generations, even when other relevant variables are held constant (Bahry 1987; Silver 1987). It is argued that this is a "generational" difference and not merely a reflection of the younger age of the post-Stalin generation.

A more recent discussion of the effects of age on propensity to accept change—in particular the reforms introduced by the Gorbachev administration—is that of T. I. Zaslavskaya (1990). In her discussion of the relationship between age and the likelihood of support for reform, she distinguishes two age groups in 1990 most likely to support reform: those over 55 and those under 35. Among the former, she finds those who, as she put it, had a "swallow of freedom" during the reformist Khrushchev years.

In our study, age does have a moderate effect on the support for political change (see Table 6). Since the definition of the post-Stalin generation used by the SIP study encompasses most of our sample—and, indeed, most of the population of the former Soviet Union—we cannot replicate all the generational categories used in the earlier work. However, the strong negative effects of membership in the 60-and-over age cohort do suggest that those who lived for a substantial period of time in the Stalinist Soviet Union are indeed resistant to liberal political reform. All our analyses show that people aged 60 and over are consistently and significantly more negative toward political reform than any other age group. Of those sixty or older, 53%, as compared with only 20% of those under age 60, have zero or negative scores on the scale of support for political change, confirming the political conformity of this group found in the SIP data. By 1991, however, this older group was retiring or being forced into retirement by the Gorbachev program of cuts in state and Party bureaucracies, thus limiting its political impact.

In addition to the marked effect in the oldest age group, our data also show that support for change declines rather consistently with increasing age. As Zaslavskaya observes, for those who were galvanized by the opening to liberalizing reform of the Khrushchev era (though the policies were often contradictory and unevenly implemented), the Brezhnev period represented a closing of that window and the ushering in of an increasingly dreary and immobile system. Thus, among respondents in their fifties or somewhat older, one finds both the initiators of the new Gorbachev reform movement and those who lived under the Stalinist dictatorship. The numbers of the group singled out by Zaslavskaya as favoring political reform, must, therefore, though their influence is very great at present, be offset by the greater part of the cohort. On the other hand, as noted, those who are over 60 are much more opposed to political change.

TABLE 6

Support for Political Change by Age (%)

SUPPORT FOR POLITICAL CHANGE	16–25	25–29	30–39	40–59	60 AND OVER
−5 through −2	4	9	7	8	17
−1, 0	15	19	17	24	36
1	23	14	21	24	22
2, 3	34	29	33	25	18
4, 5	24	29	22	19	7
Total	100	100	100	100	100
Weighted N	(375)	(257)	(385)	(641)	(289)

Note: Tau $b = -.18$; gamma $= -.23$.

Consistent with these findings, age is also negatively associated with support for individual responsibility for individual well-being, although this effect of age is not as strong as its dampening effect on support for political change (Table 7). However, the relationship is notable for its monotonicity. Clearly, a single cross-sectional survey cannot differentiate between generational and life-cycle effects: the data can be read to suggest either a very regular propensity for people to increase in their support for a collective approach as they age (possibly reflecting a wish to be protected from market forces as individual frailties or other personal difficulties associated with age increase) or a generational effect representing the quite steady erosion over time of the impact of socialization on the collectivist ethos. In regression analyses that we shall report, age is consistently the most or second most important of all variables.

Membership in the Communist Party

From the inception of the Soviet state, the Communist Party was accorded the "vanguard" role, leading and structuring policy options and setting policy direction. For over seven decades, all political discourse in all mass media repeated and reinforced this

TABLE 7

Locus of Responsibility for Social Well-Being by Age (%)

LOCUS OF RESPONSIBILITY	16–25	25–29	30–39	40–59	60 AND OVER
State	43	49	51	53	58
Individuals	57	51	49	47	42
Total	100	100	100	100	100
Weighted N	(353)	(242)	(349)	(594)	(279)

Note: Tau $c = -.11$; gamma $= -.14$.

notion. Part of the breakdown of the system was the repeal of Article 6 of the Soviet Constitution, which mandated the monopoly of political power by the Communist Party of the Soviet Union; however, this happened relatively late in the process, and our survey was conducted shortly before the event.

If the Party ideology about its vanguard role were valid, it would follow that Party members would differ from the non-Party population with respect to the critical issues of political reform. Given the movement toward glasnost and perestroika, if they were playing a "vanguard" role, Party members would be more supportive of political democratization. We find, instead, that Party membership is not at all related to support for political change. More importantly, of course, this means that Party membership did not predispose to *rejection* of political change. Overall, Party people are as divided about political reform as is the rest of the population. That they do not display a distinctive and coherent position on policies of such import, suggests that at least toward the end of the life of the Soviet Union, the Party was far from the monolithic instrument of Bolshevik tradition.

That Party members did not differ from nonmembers in attitudes toward political change suggests (as noted) that on these issues the vanguard role of the Party was to a large extent ephemeral. But it is also the case that at the time our survey was in the field, the Gorbachev reforms had not penetrated very deeply into the Party itself, that is, to the rank-and-file politically and geographically distant from the Moscow center. Gorbachev's advocacy of a reformed role for Party members (opening their operations to the scrutiny of glasnost, mobilizing non-Party people for greater participation in the workplace, and taking the lead in enlisting support for the complex of political reforms) did not reach very far below the higher echelons of power.

For example, in surveys carried out by the Academy of Social Sciences of the Party's Central Committee in 1986, 1987, and 1988, among the low-level Party officials who ran grassroots Party organizations, it was found that the reform process had "touched only the highest strata" of the Party. Very few had noticed any increase in the Party's attention to public opinion in the workplace, and an analysis of local Party summaries of operations showed little change from past practices (Toshchenko 1990). In fact, Party members failed actively to involve themselves or to change decisively—or even noticeably—their style and content of work, while the "informal organizations" and the competing political parties in process of formation increasingly structured the debate ("Pochemu otstaet Partia?" 1989).

Our findings on support for individual versus collective responsibility underline the conservative posture of Party members. Apart from age, education, and occupational prestige, Communist party membership is among the more influential variables in its association with the traditional collective ideology. Of Party members, 62% prefer the state to take responsibility for individual well-being, as compared to only 49% of non-Party people. Given this propensity for continuation of the ideological status quo, it is not surprising that Communist Party members as a whole are not in the forefront of those advocating more rapid change and greater political freedom to advocate change.

Education

A large literature indicates that more highly educated individuals are more tolerant of political deviance and more likely to engage in political participation. This suggests that the highly educated in the Soviet Union will favor reform, at least insofar as it concerns political democratization. Consistent with this, we find a positive relationship between education and support for political change (tau b = .17, gamma = .25). We also find that education has an equally strong impact on level of support for individual versus collective responsibility but that, interestingly, it also increases support for collectivism. Only 35% of those with higher education favor individual responsibility for social well-being, as compared to 51% of those with secondary education, and 58% of those with eight or fewer years of education (tau = −.15, gamma = −.26). Moreover, education is important: it lags behind only age and Central Asian ethnicity in its predictive power on support for political change and is the most important variable in predicting attitudes toward preference for state or individual responsibility for citizens' well-being. In regression analyses, the opposite signs for the relationships of education to our two dependent variables are maintained even after a large number of other variables are controlled. It is noteworthy that the relationship of education to support for collectivism is not consistent with the SIP data: among Soviet émigrés, Silver (1987) reports that the more highly educated are more likely to favor private/individual rights and solutions to problems than collective/state rights and solutions.[5]

Our interpretation of these differential relationships follows our theoretical discussion. Many observers have noted that the principal beneficiaries of the political changes instituted in the last few years were precisely the highly educated. Mobilizing this stratum for rapid political and economic change required the distribution to them of valued advantages. They were the recipients of both the material and the psychic gratification associated with glasnost: the foreign trips and goods and the ability to form new associations and acquire previously restricted information. What they apparently learned from this advantage in terms of their political attitudes was a strong propensity to favor the political freedoms represented in our scale of political change; but the changes they preferred were narrow and restricted to those that provided them with the benefits they sought—primarily the intellectual benefits that flow from political freedom.

Nevertheless, the issue should not be framed solely in terms of instrumental personal advantage. Many

of the most educated are precisely those who believe in a reasoned fashion that social justice requires the strong hand of the state. They are concerned that irrespective of the record of success of this particular system, the state does bear the onus for the social welfare of its citizens. In particular, as the provision of a safety net for the elderly, the young, the infirm, and returning veterans proves illusory, calls for a return to a greater role for the state in assisting the dependent population grow stronger. Many of those who search for models for the post-Soviet future look to countries where the role of the state is greater than it is in the United States. Thus, the well educated are favorably disposed to political change that benefits themselves but also resist certain changes that may be ideologically incompatible.

Income

Income plays a significant role in increasing support for political change, although its effect is considerably weaker than that of education. The reforms thus far are associated in the minds of many citizens of the former Soviet Union with a worsening economy, in which only a few groups have managed to derive material benefits (Levada and Sedov 1990). The few groups include those who run cooperatives and those who conduct illegal or partially illegal economic activity. In this context, it is not surprising that those with lower incomes are more likely to be fearful of, and opposed to, reform as compared to the wealthier, although the differences are not extremely large. In fact, the economic effects of the changes initiated by the Gorbachev leadership disproportionately burdened those with lower incomes.

Economic problems are compounded for older people on fixed incomes. A Soviet study shows that the majority of people who retired in the 1960s live on less than 50 rubles per month. This is considerably below even the unrealistically low poverty line of 70–75 rubles per month at the time of our survey. As the world now knows in great detail, many goods are in such short supply that only barter arrangements or radically inflated prices on unofficial or illegal markets can supply necessities. At the time of our survey, food purchases accounted for at least half of all expenditures of family income ("Prozhitochny minimum" 1990).

Issues of structural unemployment were already of concern to Soviet citizens well before explicit debates on a market economy broadened the scope of expected insecurity. The economies of the Central Asian republics were particularly affected: in the first six months of 1989, only some two hundred thousand people out of the three million unemployed in Central Asia were able to get work ("Bez raboty sredy morya raboty" 1989). The Gorbachev administration had indicated that the reforms on which it embarked would be accompanied by a "safety net" for the needy population. For example, all military demobilization was supposed to take place in synchronization with increased availability of housing and other necessities. However, the Soviet population saw little concrete preparation of such a safety net. Given widespread poverty, political and economic change may have represented a potential threat, rather than an opportunity. In this context, the higher the income, the more the individual would be able both to withstand the dislocations of change and to benefit from the increased freedom it represented.

Curiously, however, income had no impact on attitudes toward individual versus state responsibility for social well-being. While one might expect that those who are less able to care for themselves would favor a system in which the state took on this responsibility, we do not find this to be the case. This might well reflect an opinion held by the poor that the state has not been successful at this enterprise.

Gender

Men are significantly more likely than women to favor political change. This finding is consistent with much other research, in both the West and the Soviet Union, on the greater political conservatism of women, their lesser political interest and participation, and their stronger support for and trust of political authority (Jennings 1983; Jennings et al. 1990; Jennings and Niemi 1981; Shapiro and Mahajan 1986; and Carnaghan and Bahry 1990). However, gender is unrelated to position on state versus individual responsibility.[6]

In the former Soviet Union, the issue of gender is in part a cultural/ethnic one, in which aspirations and levels of mobilization of women differ radically from the Muslim East to the European West. We find that in its effect on support for political change, however, gender operates independently of the cultural factors associated with geographic region. Nevertheless, other factors create rather different environments for men and women. In spite of both roughly equal chances for educational attainment and high levels of work force participation (though by no means evenly distributed across occupations and disproportionately found in feminized occupations), women are less embedded in political life and more isolated than men. Leisure time is unequally distributed; and given the relatively low availability of appliances and other domestic aids, women, most of whom work in the national economy and also bear the burden of domestic obligations, have less free time than their male coworkers. Again, as elsewhere, our data show males as significantly more likely to express interest in politics than females, a major factor in receptivity to change. Forty-five percent of males, but only 32% of females, reported that they were "very interested" in politics (tau c = −.14, gamma = −.22).

Occupation

In discussions of support for political change in their country, Soviet analysts often referred to occupational sector and status. "Bureaucrats" or the "bureaucracy" were often cited as the principal sources

of obstruction to Gorbachev's reforms. Zaslavskaya (1990), for example, found the greatest resistance to change in the "directing apparat" (high-level bureaucrats), among people who work in the trade and communal services sector and among workers who are corrupt or severely downwardly mobile (*déclassé* is her term). She finds that certain others, whom she calls "quasi supporters," adopt an outward posture of support while disguising their real (negative) views toward political change; among these are people who work in the Communist Party apparatus, the state apparatus, and economic leaders.

On the distinctly supportive side, she finds those who have begun to participate in the new structure, such as employees and directors of cooperatives, leaseholders and others exercising new rights for individual production, workers who participate on the more flexible and remunerative group contract system, and finally, people who work on successful collective farms. Except for the last, all of these groups have already voted with their choice of job to accept change and depend on its continued expansion for their success. They are risk takers in a rapidly changing system.

In our survey, only those who work in the agricultural sector distinguished themselves by being less inclined to support political change than workers in any other branch of the economy.[7] This is consistent with findings in studies in many countries of the generally stronger conservatism of farmers and agricultural workers as compared to industrial workers. In Soviet society, agricultural workers were, by and large, not the risk takers whom Zaslavskaya describes. Even the notion of a "successful collective farm" is more often than not oxymoronic. The traditionalism of the agricultural sector and of rural life in general (as our findings about urban–rural differences in support for political change will show) was certainly a feature of Soviet political life, exacerbated by the isolation and disparities in development between that sector and others (Breslauer 1982; Hewett 1988).

The negative posture of the agricultural sector may reflect not only the traditional unwillingness of the least modern sector to change but also a certain skepticism about the reality and effects of reforms and of the ability of the agricultural sector to benefit from them as long as collective organizations survive and land is not fully privatized.

Urban or Rural Residence

We created a simple dichotomous measure in which cities of all sizes are classified as "urban," and rural and agricultural areas as "rural." Urban residents are greater supporters of political reform and democratization than are their rural counterparts (gamma = .28). This is consistent with numerous studies documenting the greater conservatism of those living in rural areas. Also (as we would expect), urban residents are more supportive of state responsibility for the social well-being of individuals, while rural residents are more likely to prefer a larger role for individuals (gamma = −.14).[8]

In terms of building support for the types of changes being undertaken in the former Soviet Union, these relationships reveal some of the difficulties when people do not see political reforms as unidimensional; that is, there is more support in the urban areas for political change in general, but this remains coupled with lingering support for state-oriented solutions to social and economic problems. In the rural areas, in contrast, there is less support for political change in general but more support for a movement toward greater individual responsibility.

Satisfaction with One's Life

Apart from education and age, life satisfaction is the only variable we have investigated whose effects survive simultaneous control for many other variables in multiple regressions for *both* of our dependent variables. For this reason—and because it plays an important role in some development literature—we included it in our analysis despite its different conceptual basis as compared to the social structural variables that constitute most of our independent variables.

Life satisfaction is often considered part of a cluster of cultural attitudes that are necessary for political democratization and economic change to take root (Inglehart 1990). At the aggregate national level, Inglehart demonstrates that life satisfaction is highly related to stability of democratic political institutions. In our Soviet data, however, higher satisfaction with one's own life resulted in *decreased* receptivity to change (tau b = −.12, gamma = −.17). That people who are satisfied with life want less change than the dissatisfied seems unremarkable. Reflecting on Inglehart's finding, however (which might be read to mean that the satisfied support democracy), we see that a different interpretation may be more appropriate: political change is less likely to come from the satisfied, whether they live in democratic or totalitarian systems.

On the other hand, we also find that individuals who were more satisfied with their own lives were more inclined to situate responsibility for social well-being with the *individual*, rather than the state (tau b = .10, gamma = .16). It is possible that people who were satisfied with their lives in the Soviet Union identified the source of their well-being in themselves or in their special abilities to exploit state programs, rather than more directly in any outputs they may have received from the state economy and social welfare programs that collectively represented the state-oriented ideology. The relationship between life satisfaction and preference for individualistic solutions is another (modest) indicator of the failure of state programs in the Soviet Union. Yet, interestingly, even in a state-run system, if one has found ways to cope and is therefore relatively satisfied, change is seen as less desirable.

TABLE 8

Regression Equations for Support for Political Change and Locus of Responsibility for Social Well-Being

	SUPPORT FOR POLITICAL CHANGE (POSITIVE = HIGH)			LOCUS OF RESPONSIBILITY (INDIVIDUAL = HIGH)		
INDEPENDENT VARIABLES	UNSTANDARDIZED COEFFICIENT	SE	BETA WEIGHT	UNSTANDARDIZED COEFFICIENT	SE	BETA WEIGHT
Central Asian ethnicity	−.491	(.065)	−.200	—	—	—
Age	−.192	(.028)	−.182	−.064	(.013)	−.139
Education	.183	(.038)	.127	−.106	(.018)	−.169
Gender (male high)	.452	(.117)	.098	—	—	—
Urban residence	.380	(.126)	.080	—	—	—
Life satisfaction	−.234	(.078)	−.077	.108	(.036)	.082
Income	.065	(.029)	.058	—	—	—
Georgian ethnicity	—	—	—	−.123	(.042)	−.079
Constant	1.485	(.403)		2.317	(.161)	
Multiple R		.397			.231	
R^2		.158			.053	
Adjusted R^2		.153			.050	
Significance of equation		<.001			<.001	

Note: All coefficients presented are significant at $p < .01$, except for income, which is significant at $p = .02$.

MULTIVARIATE ANALYSIS

A multiple regression equation (ordinary least squares) predicting support for political change confirmed that of the variables just considered, the most important were Central Asian ethnicity, age, gender, education, income, urban residence, and life satisfaction. (The multiple R is .40 and the adjusted R-squared is .15.) Table 8 gives the values for this equation. Working in the agricultural sector also has an impact but its coefficient is just short of significance, and the increase in the R-squared is insignificant.

For comparative purposes, Table 8 also presents an ordinary least squares regression for state versus individual responsibility for social well-being.[9] We are less successful with prediction of this variable (as would be expected with a one-item dichotomous measure). Including education, age, life satisfaction, and Georgian ethnicity (all of which make significant independent contributions to the equation), the multiple R is .23, and the adjusted R-squared is .05. Increasing age, higher education, and Georgian nationality increase the likelihood of preferring a state-oriented solution to social problems. Life satisfaction correlates positively with preference for an individualistic orientation. Occupational prestige and urban residence are other variables that are important to this measure but fall just shy of significance in the regression. A logistic regression of this question confirms education, age, Georgian ethnicity, and life satisfaction as the major influences.

As we have discussed, the regression coefficients show that both education and life satisfaction have opposite effects on our two dependent variables: education is associated with increased support for political change but decreased support for individualistic solutions, life satisfaction, with decreased support for political change but increased support for individualistic solutions. These changes of predictor variables' direction of influence on our two different measures of political reform demonstrate how the complexity of political change may lead the same people to react differently to its various aspects. Clearly, such issues need to be taken into account in developing measures of attitudes toward development, modernity, democratization, political change, and related complex concepts, and multi-item measures should be rigorously evaluated to be sure that all items are consistently related to each other.

CONCLUSION

Our findings about the multidimensionality of attitudes toward the complex of reforms introduced in the Gorbachev era have important political implications. That attitudes on these related subjects exist without significant associations with each other means that there is little systemic constraint on attitudinal lability. Major changes in support for particular aspects of political or economic reform can occur as change occurs in political or economic conditions.

Using our measure of support for modernizing political change, we find that its strongest proponents tend, though not without exception, to be the beneficiaries of those policies. Many of the supporters also fall into groups identified by cultural theory as "modernizing" groups. For example, the young have derived both psychic gratification from the policies and are looking at distinct career advantages. Most flexible in their outlook, and most able to change, they are the ones who are most likely to adapt successfully to a radically changed economic and political environment. They are better educated than their predecessors and better able to advance in

a situation of much greater uncertainty. The college-educated, however, may still support a strongly interventionist state when contrasted to wholly autonomous individual responsibility not only because of the relative advantages they might have derived from it but also because of an attachment to the notion that (an improved version of) the state might do better as a guarantor of social justice than would the unfettered operation of a market.

Those with higher income have greater insulation and can better withstand the sharp rise in prices. Those at the low end of the status scale (in terms of education and income) are unconvinced that the much-publicized projected safety net for the disadvantaged will offset increasingly burdensome dislocations. Males are also relatively more advantaged, empowered, mobilized, and able effectively to take advantage of political change and to benefit from it. All of these characteristics, as well as urban residence, are also associated with greater participation and mobilization.

Ethnicity remains a key factor in attitudes toward political change. But for the largest ethnic group, contextual factors are also important. There are substantial differences between Russians in their home republic, the Russian Federation, and the many millions of Russians who live, for the most part, as minorities in other republics.

Our research thus provides support for cultural theories of the role of mass attitudes in political change, insofar as this relates to the groups that will support modernizing social change; but our interpretations stress that support also arises out of particular material benefits realized by some groups from social change.

The ethnic and sociodemographic differences we observed as the Soviet Union was collapsing create serious problems for democratizing reform in the newly independent successor states. These divisions are an inevitable part of the legacy of the former Soviet republics and may well be magnified under conditions of extreme economic and political uncertainty. The traditional source of mobilization—and the source in control of many of the instruments of mobilization—had in the past been the Communist Party. But members of the Party were not performing that function as both their influence and cohesion dissipated. To mobilize and forge the coalitions necessary for developing the institutions (including effective political parties with an adequate base of popular support) and habits of change, the range of beneficiaries must be expanded. As our data show, support for both democratic change and change in the scope of state control is tentative. Unless there are timely outputs—both economic and political—that alleviate the serious material problems, legitimate the leadership, and confirm both the efficacy and utility of procedural and normative change, the process of institutional development will be substantially impaired.

APPENDIX A: THE SAMPLE

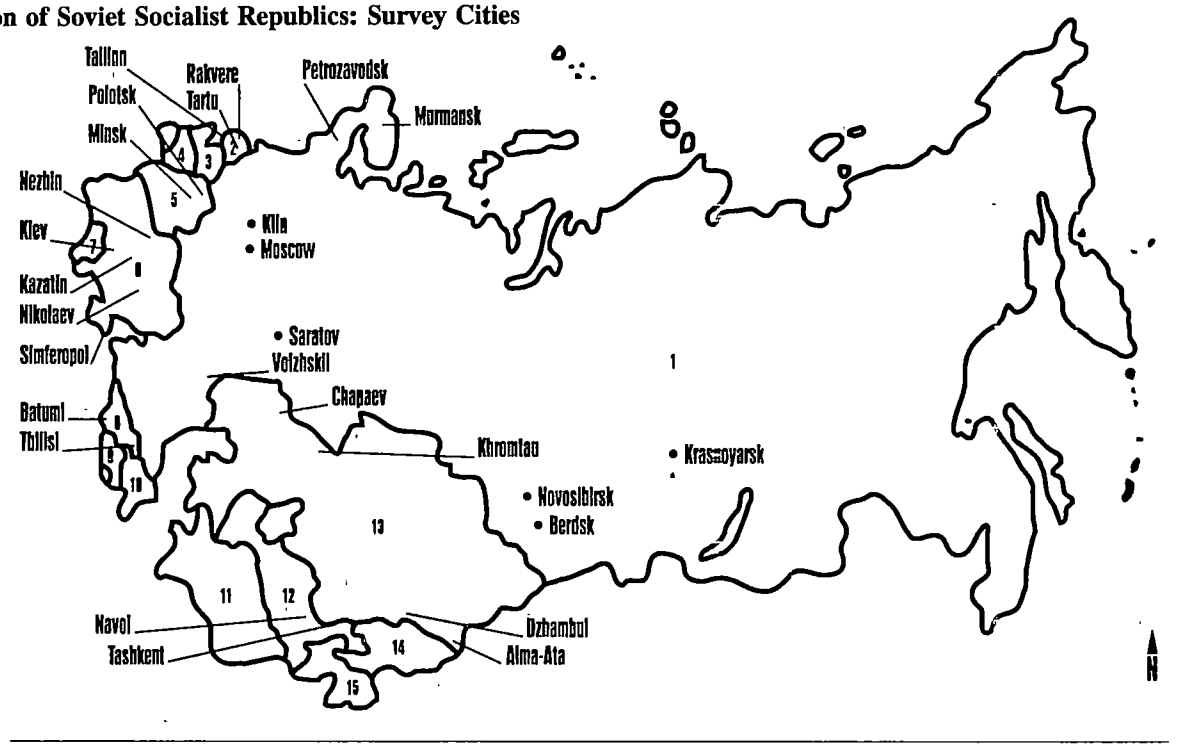
The sample was designed to represent the entire Soviet population, as well as the major nationality groups. The design was therefore clustered in seven of the 15 republics: Estonia, Belorussia, Georgia, Kazakhstan, Uzbekistan, Ukraine, and the Russian republic (RSFSR), which together contained 88% of the Soviet population. For the smaller republics, the sample uses clusters of republics, grouped according to cultural similarity and/or geographic proximity, to represent various regions of the nation. Thus, interviewing for the Baltic region of Estonia, Latvia, and Lithuania took place entirely in Estonia; interviewing for the region including Ukraine and Moldavia took place in Ukraine; interviewing for Georgia and Armenia took place in Georgia; and interviewing for Azerbaijan, Uzbekistan, Kirgizia, Tajikistan, and Turkmenia took place in Uzbekistan. Because of either large size or cultural dissimilarity, the Russian republic, Kazakhstan, and Belorussia were not grouped with other regions. Sampling within the very large Russian republic was further stratified by region, and separate samples were drawn from its five regions: East Siberia, West Siberia, the Northwest, Volga, and Moscow.

In order to achieve larger samples of important nationalities, we sampled disproportionately in Belorussia, Estonia, Georgia, and Kazakhstan. At the same time, achieved samples were somewhat smaller than planned in Ukraine and Uzbekistan. The final sample of 2,520 cases was therefore weighted to correct for the disproportional stratification. The weighted sample size is 2,006 and the number of cases in each regional area in the weighted sample accurately reflects the size of the population in each corresponding territorial region. All reported numbers of cases are based on the weighted sample size. The downsizing of the sample occurs because a weight of 1 was assigned to respondents in regions sampled with probability proportionate to size and oversampled groups were then downweighted against this standard. This procedure is preferable to upweighting the former group, because it does not bias tests of significance, based on the number of cases in the sample.

In each republic, interviewing took place in the capital, in one or more other midsized cities, and in some smaller rural and semirural areas. Given the lack of infrastructure for survey interviewing and difficulties of transportation, some interviewing sites were selected on the basis of geographic proximity to the twenty-six regional centers of the survey organization, availability of field supervisors and interviewers, and so on. Within these major sites, the design specified random selection within strata defined by age and sex, based on census information about the distributions of these characteristics within each regional cluster. In each sample location, names were chosen randomly from lists maintained in the government's population registration office. Name, sex, and age are noted on each individual's registration

FIGURE A-1

Union of Soviet Socialist Republics: Survey Cities



Note: Rural areas are not shown.

card, and interviewers sampled systematically to achieve the required numbers of cases in each stratum. The map presented as Figure A-1 shows the urban locations of the survey; the survey also included a large number of rural sites not shown on the map. This was the first Soviet survey to include such large numbers of rural and ethnic minority respondents. Since 34% of the Soviet population lives in rural areas, this is an important component of a representative sample. The interview was translated into several of the national languages (Estonian, Kazakh, etc.) and conducted, when appropriate, by conationals who were native speakers of those languages.

Since the sites themselves were not selected by strict probability methods, an exact estimate of sampling error is precluded; however, an educated guess is that it might be about 6%–7% for estimates based on the entire sample (basically doubling the sampling error for a random sample of this size). Given current practice even for samples that do not meet all the criteria for probability samples, we do occasionally report significance tests. We intend these as a general guide to the reliability of the data, rather than as rigorous tests of significance.

Even though we do not adjust the sample for any characteristics other than geographic region, it matches the Soviet population reasonably well on a variety of demographic characteristics (see Table A-1). We have the usual overrepresentation of well-educated people. It is therefore not surprising that the sample somewhat underrepresents the manufacturing sector and somewhat overrepresents people in education and science.

Prior to fieldwork, regional survey center directors came to Moscow for training on our instrument and procedures; and they, in turn, recruited and trained interviewers in their regions. Interviewers were paid almost twice the usual rate, so that the most experienced and best-trained could be hired. Armed with the names and addresses of sampled persons, interviewers returned up to three times to locate designated respondents. Virtually no interviewers had access to cars; and given the large size of the cities and apartment blocks and the usual difficulties of urban transportation, three callbacks in many cases represented a genuinely heroic effort. Following three unsuccessful callbacks, local procedure entailed substitution of a new name, a procedure also used in the United States by many commercial polls. Thus, the concept of a "response rate" does not seem to exist; nor, unfortunately, are systematic records kept of which respondents were substitutes.

Interviews lasted, on the average, about one-and-a-half hours. Each respondent was assured of confidentiality and anonymity; and, following the interviews, the names of respondents were separated

TABLE A-1

Population and Survey Sample Comparisons (%)

	POPULATION	SAMPLE
Age		
16–19	8.4	10.0
20–29	21.5	22.3
30–39	21.3	19.5
40–49	13.8	16.1
50–59	15.5	16.7
60+	19.6	15.3
Gender		
Male	45.7	46
Female	54.3	54
Marital status (men)		
Never married	20.6	21.9
Married	71.5	74.1
Widowed	2.6	1.1
Divorced	4.7	2.9
Marital status (women)		
Never married	14.0	18.0
Married	60.6	60.6
Widowed	17.4	11.3
Divorced	7.5	10.1
Education		
Primary or incomplete secondary	38.8	19.1
Secondary, general	30.5	23.9
Secondary, special	18.2	29.3[a]
Incomplete higher	1.7	4.8
Complete higher	10.8	22.8
Employed	61.9[b]	70.8
Occupational sector		
Indust., construction, transp., communication	48.8	40.6
Education, culture, science	13.6	17.9
Trade, public catering	8.1	5.8
Housing, service	4.0	6.6
Health care	5.9	5.3
Agriculture	18.3	18.7

Note: The sample data are weighted only by geographic region. Population data include persons aged 16 and over, except for education, for which the data given are for ages 15 and over. For the age distribution of the population, the size of the group aged 16–19 is estimated as 40% of population aged 10–19, which is the category used in the source document.

Sources: Population data are from *Narodnoe khozyaistvo SSSR v 1989g* [The economy of the USSR in 1989] (Moscow, 1990) and a USSR State Committee on Statistics press release of January 1990. We should like to thank Vladimir Treml for his assistance.

[a]Includes both secondary special and secondary professional–technical.

[b]Soviet sources cite 127.033 million persons employed in the officially defined national economy. To that we add an additional 1.5 million estimated to be employed in the private sector. Excluding the military, then, we estimate 61.9% of the population of 207.729 million (as of 1989) to be in the civilian labor force.

from their interviews. Validity tests included post card verification of interviews, callbacks on a small percentage of respondents, and name recognition questions that included a nonexistent television personality. Carbonless copy paper enabled both the Soviets and Americans to have an "original" of all interviews, and both sides conducted independent coding operations. The data used in this report are all based on the American coding operation and weighting procedures.

During the data collection, we visited a number of survey sites. In general, the dedication of the staff was extremely high, stimulated by a sense of the importance of the scientific enterprise and the conviction that representative public opinion data was an important political input in the democratization of the nation. It was our impression from informal conversations with many interviewers that there were very few outright refusals, so that any bias in the sample is introduced almost entirely by the sampling frame per se and the not-at-home factor. Although the sample does not in all important respects measure up to the highest Western standards, it is probably one of the best national samples achieved to date in the Soviet Union.

APPENDIX B: QUESTIONS USED FOR DEPENDENT VARIABLES

Q18. Some people think that to solve our most pressing problems it is necessary to make decisive and rapid changes, since any delay threatens to make things worse. (R)

Others, on the other hand, think that changes should be cautious and slow, since you can never be sure that they won't cause more harm than good. Which of these points of view are you more likely to agree with?

Q22. Below are some widespread but contradictory statements relating to problems of the development of our society. Which would you be most likely to agree with?

a. The state and government should be mainly responsible for the success and well-being of people.

   People should look out for themselves, decide for themselves what to do for success in life. (R)

b. The state should provide an opportunity for everyone to earn as much as he can, even if it leads to essential differences in people's standard of living and income. (R)

   The state should do everything to reduce differences in people's standard of living and income, even if they won't try to work harder and earn more.

c. Strikes, spontaneous demonstrations, political meetings and other forms of social protest are completely acceptable methods of mass conduct and an effective means for solving social problems. (R)

   These forms of protest are undesirable for society, they should be avoided in favor of more peaceful, traditional and organized methods of solving social conflicts.

d. To improve things in our country people should be given the opportunity to say what they want, even if it can lead to public disorder. (R)

Keeping the peace in society should be the main effort, even if it requires limiting freedom of expression.

Q29. In the coming elections for local soviets, should we elect deputies from among several candidates, as we mostly did in the spring elections? (R)
Or is it better to avoid the conflicts these elections generated and go back to the old system of voting?

Notes

The design of this study was a joint product of the authors, together with George Gerbner, of the University of Pennsylvania, and Boris Grushin and Nikolai Popov, both of whom were at the time affiliated with the All-Union Center for Public Opinion Research. Grushin is currently the director of Vox Populi, a private public opinion polling organization based in Moscow. We wish to express our gratitude for the cooperation and contributions of these individuals to the study design, as well as to T. I. Zaslavskaya, director of the All-Union Center, which carried out the field work. Funding for this project was provided by the John and Mary R. Markle Foundation, Rockefeller Family and Associates, International Research and Exchanges Board, Coca Cola Corporation, and American Express. We also wish to thank Michael Sweat for his capable assistance with data management and computer support and Paul R. Abramson of Michigan State University for his helpful comments on an earlier version of this paper. The two of us share equal responsibility for the work.

1. "Maintaining order in the nation" and "protecting free speech" are components of Inglehart's well-known measure of "materialist versus postmaterialist values" (see, e.g., Inglehart, 1990). Unfortunately for comparative purposes, that measure asks people to rank these and other values, so that direct comparisons with our measure are not generally possible. In addition, most of the published accounts using Inglehart's measure report only composite results for the overall scale, which includes many values not germane to our analysis.

2. Recent relevant European experience is worth noting. Support for unconventional means of political participation declined in both West Germany and the Netherlands between 1974 and 1979–81 (Jennings, et al. 1990).

3. For purposes of this discussion we include Kazakhstan in Central Asia. Rumer (1989) summarizes the distinctions between the four- and five-republic definitions of Central Asia.

4. Investigators working on this project collected data from 1970s émigrés from the USSR and generalize it to the western urban population of the Soviet Union.

5. While Silver used a three-item measure of state versus private control of medical care, heavy industry, and agriculture and our own measure is a single question about state versus individual responsibility for the "success and well-being of people," the core concepts involved in both measures seem similar. An intriguing possibility to explain the discrepancy in results is that the émigrés may be an ideologically self-selected group in which background characteristics relate differently to basic social and political attitudes. Since emigrants are generally thought to be less risk-averse than nonmobile individuals, it makes sense that the most advantaged among them would favor individualistic solutions to social problems. Among the nonmobile, on the other hand, socialization might increase support for regime norms (in this case, state-oriented solutions to social and political problems) among the best-educated.

6. Our finding is again at odds with the Soviet émigré project, since Carnaghan and Bahry report that women emigrants were considerably less supportive than men of state control. Again, it is possible that the difference is caused by emigrant versus resident status. Possibly, female emigrants are more resentful about the practical difficulties of life for women in the Soviet Union.

7. In our sample, the agricultural sector includes not only farmers but also animal husbandry and other technical and scientific specialists, machine operators, administrative and clerical personnel, and all others who support the farm sector, whether in state or collectivized agriculture.

8. We also used more refined measures of city size with no noticeable improvement in prediction. Very large cities in the more conservative central and eastern areas of the country are not as supportive of change as are smaller cities in the west, creating some curvilinearities in the relationships. Overall, therefore, because the largest differences are between the rural areas and cities of any size, the simple urban–rural dichotomy is more successful than ungrouped city size or any more refined grouped measure. For example, for political change, the gamma is .18 with unrecoded city size and .20 with a collapsed measure with six values. For Q22a, the analogous values are −.09 and −.10.

9. While logistic regression is preferred for dichotomous dependent variables, with very even distributions, such as the 49–51 split we have in this case, ordinary least squares generally yields the same results.

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ECONOMIC TRENDS AND GOVERNMENT SURVIVAL IN WEST EUROPEAN PARLIAMENTARY DEMOCRACIES

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In this study, I investigate the linkage between trends in key economic indicators (inflation, unemployment, and growth in gross domestic product) and government survival in 16 postwar European parliamentary democracies. The partial likelihood method, which allows for variation in indicator values over the lifetimes of individual governments, constitutes the basic analytic tool. The findings reveal overall causal roles for both inflation and unemployment, as well as important differences in these roles between socialist and bourgeois governments and between pre-oil crisis and post-oil crisis eras. Most significant, the introduction of these indicators to the analysis helps to resolve the debate between two rival explanations of governmental stability, the bargaining complexity hypothesis and the ideological diversity hypothesis, in favor of the latter.

The study of government stability in parliamentary regimes has been enlivened within the last decade by a debate over the fundamental nature of the phenomenon and how it should be investigated empirically. The standard approach, as in much of the social sciences, is the causal one; it relates the durations of governments to a variety of properties of those governments or their parliamentary environments, usually by means of regression analysis. Critics of the causal approach have argued, however, that it mischaracterizes the underlying process: governments do not fall because they lack properties normally associated with longevity, they fall when challenged too severely by events or developments occurring in the outside political, social, or economic environments (Browne, Frendreis, and Gleiber 1984, 1986, 1988). Since events of this sort are essentially random with respect to individual governments, they maintain that the task at hand is not to associate characteristics with government durability but to model the randomness of government durations.

The "events" theorists clearly have a point: to treat a government's duration solely as a function of a set of characteristics most or all of which are fixed by the time the government assumes office is excessively deterministic. Some allowance must be made for developments that occur during the government's lifetime. Yet to expunge causal factors from the analysis totally—to treat durations as purely a function of random outside events—seems even more unrealistic. If British governments normally remain in power longer than their Italian counterparts, for example, it is surely due in part to systematic differences between the two systems (less party-system fragmentation, more single-party majority governments, etc.), not simply because British governments face fewer or less difficult events.

These observations suggest that an optimal strategy for advancing research on this topic would be to combine the insights of both perspectives in some fashion. Since the events thesis of random collapses implies that government durations are distributed according to a negative exponential distribution (Browne, Frendreis, and Gleiber 1986), King and his colleagues (1990) recently proposed a sensible unification: a model that treats government durations as a function of a set of independent variables and an underlying exponential distribution. In their "unified" model, the systematic element in government durations is taken up by the independent variables; anything that cannot be explained by those variables is relegated to the exponential distribution. Estimated by the maximum likelihood method, the model possesses the important property of accounting for all significant intercountry differences in durations, thereby resurrecting comparative or cross-national analysis from the limbo to which the system-specific approach of the events theorists had banished it.

Although the unified model shifts the focus back onto the systematic factors (events become, in effect, part of the error term), it could be argued that it adheres too closely to the events approach in two respects. First, it accepts rather uncritically the appropriateness of an exponential specification, an assumption that Easton and I (Warwick and Easton 1992) found to be unwarranted.[1] Second (and more important for the purpose at hand), it implicitly endorses the events theorists' dichotomization of relevant factors into fixed characteristics and totally random events. The weakness with this categorization is that it omits a third major alternative: factors which vary in a nonrandom fashion over the lifetimes of governments. Foremost among these factors in democratic regimes is the changing state of the economy.

I shall attempt to relate evolving economic conditions—as measured by inflation, unemployment, and economic growth indicators—to government survival in West European parliamentary systems in a systematic empirical fashion. Although there have been numerous efforts to associate economic indicators with opinion poll standings or with electoral outcomes, attempts to establish a linkage with the sur-

vival of governments are relatively rare. A standard property of these attempts, moreover, is the characterization of economic conditions during each government by means of average indicator values (e.g. Budge and Keman 1990, 176–82; Robertson 1983b, 1983c), thereby rendering static an analysis that is intrinsically dynamic in nature.[2] There is no need for this compromise. Event history analysis—the broad class of techniques of which King and his colleagues' exponential model is an exemplification—provides ways to incorporate variables whose values change over the duration of individual cases directly into the analysis. I shall exploit this facility in order to determine whether—and under what conditions—changing economic circumstances bring about government terminations in parliamentary regimes, particularly when other relevant factors are taken into account. In so doing, I aim to fill a major gap in the empirical explanation of government stability in West European parliamentary contexts.

METHODS AND DATA

All event history models have in common the objective of explaining the "hazard rate" or rate of termination inherent in some phenomenon. Thus, a concern with durations may frame the theoretical issue; but it is typically the hazard rate that focuses the empirical analysis. (The two concepts are intimately linked, for high hazard rates mean short durations, and vice versa.) The hazard rate is generally taken to be a function of two components: (1) a set of independent variables or "covariates," which are related to the hazard rate in a log-linear fashion, and (2) a "baseline" hazard rate, which represents its underlying behavior. In mathematical form,

$$\lambda(t) = \exp(\mathbf{x}'\boldsymbol{\beta})\lambda_0(t),$$

where λ is the overall hazard rate, λ_0 is the baseline hazard rate, and x is a vector of covariates with associated coefficients $\boldsymbol{\beta}$.[3]

An important feature of this expression is that it allows the hazard rate to vary with duration time t. In fact, the assumptions one is willing to make concerning time dependency determine the type of model to be employed. An exponential model such as King and his colleagues' is defined by the relatively simple assumption that the baseline hazard is constant over the course of a case's existence; but a wide variety of more complex specifications are possible. Since there is evidence that complexity of this sort (known as "duration dependency") may characterize government termination rates (Warwick and Easton 1992), the data analysis undertaken here will employ Cox's (1972) partial likelihood (PL) technique, which, alone among event history methods, requires no specification of the baseline hazard function.[4]

Although the PL technique is itself complex, its essence can be conveyed in a relatively straightforward fashion. The basic idea is that the durations of all cases are ordered from shortest to longest, and then the covariate values for each case are compared with those for all cases that outlived it. If, say, the presence of covariate X leads to government terminations, one would expect that a case that collapsed after T days in office would have, in general, a higher X-value than the governments that survived beyond T days. When time-dependent covariates (i.e., covariates that take on different values over the lifetimes of individual cases) are involved, the PL technique compares, for the case that terminated at T, its X-value at that time with the X-values at the same point in the durations of all surviving cases. The amount of data involved in these comparisons is potentially very large; but the data collection task can be made easier if the values of time-dependent covariates are recorded for a limited number of time points, and intermediate values estimated, where appropriate, by means of linear interpolation. This is the strategy I shall follow.[5]

Apart from its capacity to handle time-dependent covariates, event history analysis provides a felicitous solution to a problem that has bedeviled previous work on government stability, namely, what to do with cases that end in a theoretically uninteresting way—especially governments that end with regular elections. These cases fit neither the events framework (since regular elections are not random events) nor the causal framework (since no properties of governments or their supporting parliamentary milieux are involved). Event history analysis simply "censors" these cases, that is, treats the true durations of these governments statistically as at least as long as the recorded durations. For the case of a government that ended with the expiration of its four-year legal mandate, this amounts to assuming that its duration, in the absence of this legal impediment, would have been not less than four years but may well have been longer.[6]

The censoring procedure may also be applied to other extraneous terminations, such as the death of a prime minister or a pro forma resignation required for technical reasons. Unfortunately, there are no statistical criteria for determining which circumstances merit this treatment, so the choice falls to the judgment of the researcher (Warwick and Easton 1992). In this study, cases are censored if their terminations (1) resulted from the death or illness of a prime minister, (2) were necessitated by legal or constitutional requirements, (3) had not occurred by the end of the observation period (31 December 1989), or (4) were brought about voluntarily within one year of elections scheduled before they took office. The last category is constructed, following King and his colleagues (1990), to allow for the possibility that governments might voluntarily dissolve themselves in advance of approaching elections in order to maximize their electoral advantage. As with the other categories, it is designed to capture situations where a government termination occurs even though its viability was not under challenge at the time.

One of the advantages of the censoring solution is

that it accommodates the common practice among researchers of treating governments that survive elections and continue on in office as having been "terminated" by those elections. This convention has been widely adopted because elections change the parliamentary party configuration and because they involve a different criterion for survival—popular success, rather than the maintenance of parliamentary support or coalitional unity. With censoring, no artificial truncation of durations is entailed in adopting this sensible rule. Other events defined in this study as marking the end of a government include parliamentary defeats, changes in the prime minister or the party composition of the government, and government resignations (regardless of the composition of the succeeding government).[7] As in the case of regular elections, the censoring mechanism is used to adjust for situations where the termination seems more formal or technical than real.

One area in which the design of this study differs from many of its predecessors is in the conceptualization of a government. A government is here deemed to be composed not only of the parties who assume cabinet portfolios but also of the parties who formally or openly declare their support for the cabinet as well. The motivation for the expansion in the definition of a government to include supporting parties is to capture the true, or effective, governing coalition. It is noteworthy in this regard that in two-thirds of the cases where such parties can be identified, their support is necessary to create a parliamentary majority. Nevertheless, any relationships that emerge using this definition will also be tested on other government definitions, including the cabinet-only version, to determine whether it affects the empirical findings to any significant degree.

In order to facilitate the sequencing of durations that underlies the PL method, duration has been recorded in days, rather than the more usual months. The values of the time-dependent covariates are, accordingly, also estimated to the nearest day. With respect to the rate of increase in gross domestic product, which measures overall economic growth, this estimation process simply involves attributing an annual value to each day of the year in question. For inflation and unemployment, however, better estimates can be obtained from monthly figures than from annual averages.[8] The method used here derives daily values of these indicators from linear interpolations of their December monthly values, which raises two basic questions: Why choose December values? and Are interpolated values based upon one month per year sufficiently accurate?

Although, in principle, any month could have been chosen as the basis for the interpolations, December values possess the important property of being very close to annual values. The coefficients of .92 and .97 generated when annual values of inflation and unemployment, respectively, are correlated across all countries with their corresponding December values suggest that the December values are rarely idiosyncratic. In addition, the slope coefficients of .93 and .92 produced when the annual values are regressed on the December values indicate that December is broadly representative of the year as a whole (a perfect match would be reflected in a coefficient of unity). The effect of interpolating December values is therefore to register the trends in the data at a level that is representative of the entire year, eliminating in the process any distortions due to seasonal fluctuations (which are especially likely to afflict unemployment rates).

The smoothening effect of interpolation also removes a certain degree of month-to-month fluctuation that is normally present in economic data. If these fluctuations represent random "noise," this smoothening confers a significant added benefit (especially since the PL model does not incorporate a random disturbance term). It is possible, however, that the erratic high values that may occur in any month are what induce government collapses. If so, the data would underestimate the phenomenon. To test for this possibility, inflation was coded in two versions: the actual monthly rate and a pseudo–monthly rate constructed from the interpolated daily values. When the relationships of the two variables with the rate of government termination were tested by means of the PL method, the pseudo–monthly rate proved to have a noticeably stronger effect.[9] This finding suggests that politicians react not to the bad figures of a single month but to underlying trends extending over a period of several months—trends more clearly evident in the interpolated values.

The data collected for this study cover 16 West European parliamentary democracies from the resumption of normal politics after World War II until the end of 1989.[10] Parliamentary party strengths, the cabinet compositions of governments, the existence of support parties, and the termination categories used for censoring are all derived primarily from *Keesing's Contemporary Archives* (1944–90). With respect to the possible presence of support parties, other sources, (e.g., Browne and Dreijmanis 1982; Strom 1990; Swaan 1973; and various country-specific works) were also consulted. For maximum comprehensiveness, the data set incorporates all governments in the relevant time frames, including noninvested governments, caretaker governments, and single-party—as well as coalition—governments. Where exclusions occur, they principally involve the time-dependent covariates. Thus, unemployment rates are not available for the French Fourth Republic or Iceland and are scanty at best in the early years for most other countries. In addition, in several countries the method of calculating rates or index numbers is not consistent across the entire postwar period, producing values that are noncomparable. When changes in rates are the object of investigation, the loss of cases is not great (since only values spanning a transition in method have to be excluded); but when the rates themselves are being examined, the entire earlier series must be excluded, reducing the number of cases appreciably. The situation with respect to the other two indicators (the consumer

price inflation rate and the gross domestic product growth rate) is not nearly so severe; but the consumer price data are less precise before 1964, when index values were given in whole numbers only.[11] Details on the sources of these data are given in Appendix A.

First, I shall examine the basic time trends in the economic indicators over the postwar era and test several versions of the indicators for their connection to rates of government termination, using the partial likelihood method. The analysis will then be broadened to incorporate the effects of other potential causal factors. As we shall see, economic indicators not only continue to demonstrate a causal role once other factors are brought into play but are instrumental in the evaluation of the relative merits of the bargaining environment and ideological diversity interpretations that have lately become major rivals in the explanation of government stability. Finally, I shall consider the success of governments of various ideological complexions (socialist, bourgeois, and mixed) in coping with inflation and unemployment; relate these records to their survival in office; and test the hypothesis that economic factors have had a greater impact on government stability in the more troubled economic climate prevalent in Europe since 1973.

ECONOMIC INDICATORS AND GOVERNMENT SURVIVAL

Before beginning the data analysis proper, it is useful to have in mind a general impression of the behavior over time of the three economic indicators: the consumer price inflation rate, the unemployment rate, and the rate of growth in the gross domestic product. In Figure 1, the time trends of these indicators are presented for the period 1950–89. These trend lines are based upon weighted averages of annual values of the indicators for the 16 countries of the sample, with the populations of the countries comprising the weights. The starting point chosen was 1950, because of the paucity of data before that time.

The trend lines pictured in Figure 1 indicate that after an initial bout of high inflation, both inflation and unemployment established and maintained relatively low levels throughout the 1950s and 1960s. In the 1970s, inflation rose to much higher levels and experienced two peaks (corresponding to the oil "shocks" of 1973–74 and 1979), then declined dramatically in the 1980s. The rise in unemployment in the 1970s was much more gradual, reaching an apex only in the mid-1980s before beginning a substantial decline. As for economic growth as measured by real increases in gross domestic product, it oscillates much more frequently and regularly than do either of these indicators but nonetheless reveals an overall tendency to decline over the period covered by the graph.

Although these trends contain few surprises, they do suggest that there is no simple trade-off between inflation and unemployment: both may be high or low at the same time. It is possible, therefore, that the two indicators have effects on government survival that are independent of each other. Conversely, there appears to be a tendency for higher levels of inflation and unemployment to be associated with lower gross domestic product growth rates and vice versa, suggesting that the latter may not convey new information about economic performance.

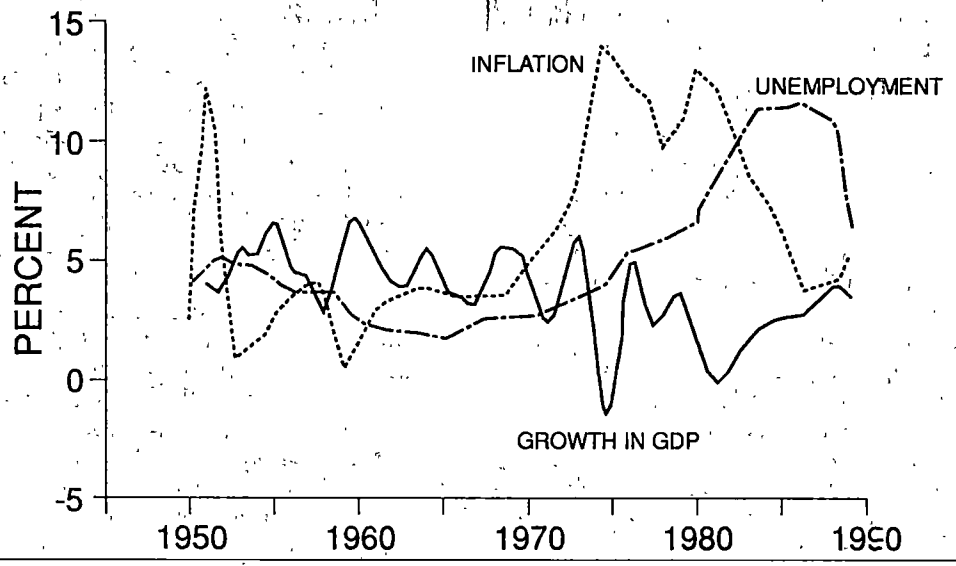
The hypothesis that economic trends are related to government survival receives a preliminary PL examination in Table 1. In interpreting the results listed in the table, it is important to bear in mind that since the model specification is log-linear, the effect of a one-unit change in a covariate on the rate of government termination is obtained by exponentiating its PL coefficient. For example, the coefficient of .06 attributed to the unemployment rate indicates that a 1% increase in unemployment is associated with an increase in the hazard rate of 6.2%, since exp(.06) = 1.062. It should also be noted that the procedures reported in Table 1 were performed with majority status (a dichotomous variable separating majority and minority governments) included as a control. This was done to prevent the possession of a parliamentary majority (or the lack of it) from masking the roles of economic covariates.[12]

The top of Table 1 explores the impact on government survival of four different measures of inflation: (1) the inflation rate, or percent increase in the consumer price index over the preceding year; (2) the change in the inflation rate over the preceding year; (3) the proportional change in the rate over the preceding year; and (4) the absolute value of the change over the preceding year. The last-mentioned category is included as a test of the hypothesis that economic change per se (regardless of whether favorable or unfavorable) brings about government instability. The results presented in Table 1 indicate that neither this nor any of the other proposed linkages finds support at the .05 level of significance (two-tailed tests), although that involving the inflation rate itself is sufficiently close that it should not be rejected out of hand. The direction of these relationships is as one would expect: high or increasing inflation rates are associated with higher hazard rates and thus a greater probability of government termination.

A very different picture emerges from the middle of the table, which contains results for the corresponding measures of unemployment. Because of the aforementioned noncomparability of data series, the unemployment rate itself is taken only from the most recent data series. The measures involving changes in unemployment rates may appropriately include earlier series based upon different counting methods, provided appropriate transitional situations are removed; this has the effect of increasing the number of cases modestly. The results indicate that the change in unemployment over the preceding twelve months is the most closely related to government survival, although all versions reveal relationships that are statistically significant or nearly so. Unemployment,

FIGURE 1

Average Annual Inflation, Unemployment, and GDP Growth Rates in 16 European Countries



Sources: Consumer prices from IMF; unemployment from UN; gross domestic product from OECD.

no matter how it is conceived, seems to be associated with higher rates of government termination.[13]

The final section of Table 1 shows the relationships with government termination of the analogous versions of growth in gross domestic product. Of the four versions, only the change in the growth rate from the preceding year shows any noteworthy effect. The negative sign associated with this effect indicates that increases in the growth rate of the economy are associated with a lowering in the rate of government termination, that is, with greater governmental stability. However, even this relationship falls well below the .05 level of significance.

The results reported in Table 1 provide some indication that economic trends (especially unemployment) are indeed associated with government survival in West European parliamentary democracies. This assessment accords with Robertson's (1983b, 1983c) earlier findings that the duration of governments is significantly related to average levels of

TABLE 1

Partial Likelihood Analyses of the Effects of Various Economic Indicators on Government Survival

COVARIATES	PARTIAL LIKELIHOOD COEFF.	T-VALUE	NUMBER OF CASES
Inflation			
Inflation rate (%)	.013 (.008)	1.73	305
Change in rate over prec. year	.024 (.018)	1.31	292
Prop. change over prec. year	−.003 (.006)	−.61	292
Abs. change over prec. year	.015 (.016)	.91	292
Unemployment			
Rate (%) (recent series)	.060 (.018)	3.35	193
Change in rate over prec. year (all series)	.235 (.055)	4.26	232
Prop. change over prec. year (all series)	.414 (.166)	2.50	232
Abs. change over prec. year (all series)	.124 (.065)	1.91	232
Gross domestic product			
GDP growth rate	−.021 (.028)	−.77	305
Change in rate over prec. year	−.036 (.024)	−1.48	296
Prop. change over prec. year	.001 (.001)	.77	296
Abs. change over prec. year	.011 (.035)	.32	296

Note: All coefficients are derived from partial likelihood analyses that include majority status as a control variable. Standard errors are in parentheses. Daily values for the inflation and unemployment covariates are interpolated from December monthly data; gross domestic product daily values are taken from annual figures.

unemployment—but not to inflation—in a more limited set of countries. The conclusion, of course, assumes all other factors equal; we cannot place confidence in it until the linkages have been shown to persist even after other relevant causal factors have been taken into account.

The analyses to follow will utilize the two indicators that showed the most promising results in Table 1: the inflation rate and the change in the unemployment rate. Not only is each of these measures more closely related to government survival than are the alternative measures of the same economic trait (controlling for majority status), but further analysis reveals that the presence of either of these measures in a PL model precludes a significant role for any of its alternatives. This suggests that the alternative indicators have relatively little to add to the statistical explanation of government survival.[14] This selection criterion is not foolproof; but in the absence of any political theory linking specific indicators to government survival, it is probably the most appropriate way to proceed at this stage.[15]

BARGAINING COMPLEXITY, IDEOLOGICAL DIVERSITY, AND THE ECONOMY

The goal of assessing the relative importance of economic conditions vis-à-vis other possible causes of government stability is complicated by the bifurcation of the latter along two very different interpretative lines: the *bargaining environment* thesis (King et al. 1990; Laver and Schofield 1990) and the *ideological diversity* thesis (Budge and Keman 1990; see also my earlier work, Warwick 1979). The bargaining environment interpretation views instability as the result of the absence of a stable core in a Euclidean ideological space—which, expressed less technically, means that coalition governments fall apart when at least one member believes that it can realize its policy goals more readily in a (viable) alternative coalition. A complex bargaining environment (one with many parties scattered throughout a large, multidimensional ideological space) increases the likelihood that these alternatives will exist. The ideological diversity interpretation, in contrast, sees government terminations as emanating principally from ideological or policy differences within governments, regardless of the availability of alternatives. The test of whether economic conditions matter for government survival therefore involves determining whether they continue to play a significant role once variables representing these two perspectives are introduced into the analysis; in the most optimistic scenario, this test will help to determine which perspective better fits the available evidence.

The main empirical support for the bargaining environment thesis comes from King and his colleagues' seven-covariate unified model. The four dichotomous covariates in that model indicate whether or not each government commanded a legislative majority, exercised its functions in a caretaker (nonpartisan) capacity, was the first to be formed after an election, and was obliged to undergo a formal vote of investiture. Bargaining complexity is conveyed by the model's three nondichotomous covariates: the number of failed formation attempts preceding the government's formation, the fractionalization (fragmentation) of the legislative party system, and its polarization as indicated by the proportion of seats held by "extremist" (antisystem) parties.

The theoretical role of these bargaining environment indicators varies considerably. A high number of formation attempts suggests a legislative situation rich in coalitional alternatives but gives no indication of the source of this condition. That source is more clearly identified with the system's fragmentation: other things being equal, more parties of significant size mean more viable alternative governments. Whether the presence of antisystem parties augments bargaining complexity is not so evident, however, given that these parties are typically excluded (by the prosystem parties, if not by themselves) from the business of bargaining over coalitions or participating in governments.

The ideological diversity thesis engages the debate precisely on this point. If antisystem parties are "noncoalitionable," as Laver and Schofield admit (1990, 200–201), their presence in force in a legislature should be associated with a lessening of coalitional alternatives and hence with greater government stability—the very opposite to what King and his colleagues found. Granted, a strong antisystem presence may be indicative of an ideologically divided and hence more complex party system. But the principal consequence of that polarization may be not a greater likelihood of attractive alternative coalitions, but rather the formation of governments from among prosystem parties that are themselves highly divided ideologically. These divisions may in turn predispose governments to early collapse. On this reasoning, one would expect ideological diversity within governments (provided it can be measured adequately) to displace the bargaining environment indicators in the explanation of government stability.

My previous research on government stability has concentrated on this measurement task (Warwick 1992[a]). The results of these efforts are three measures of ideological diversity, each significantly related to government stability. The first measure, which assesses the range of each government in a left–right dimension, is constructed from three "expert" assessments of party positions plus the European Manifestos Project's coding of postwar party platforms.[16] The other two measures assess the ideological range of governments along clerical–secular and regime support dimensions on the basis of Dodd's (1976) party scales. The question is, How well do these measures perform against the bargaining environment factors that constituted the core of the unified model, particularly when economic conditions are brought under consideration?

TABLE 2

Effects of Bargaining Complexity, Ideological Diversity, and the Economy on Government Survival

COVARIATES	MODEL 1	2	3	4	5	6
Majority status	−.69	−.65	−1.76	−1.43	−1.61	−1.90
	(.15)	(.21)	(.28)	(.32)	(.32)	(.29)
Investiture	.54	.59	—	.54	.60	.73
	(.15)	(.22)		(.22)	(.22)	(.21)
Postelection	−.66	−.49	—	−.36	−.30	−.36
	(.15)	(.20)		(.21)	(.21)	(.21)
Caretaker status	.54	.91	—	—	—	—
	(.47)	(.56)				
Formation attempts	.11	.15	—	.11	—	—
	(.05)	(.06)		(.06)		
Effective parties	.09	.07	—	—	—	—
	(.05)	(.08)				
Polarization	4.15	4.08	—	2.32	1.42	—
	(.64)	(.77)		(.96)	(1.06)	
Ideological diversity	—	—	—	.42	.54	—
				(.18)	(.18)	
Left/right	—	—	.33	—	—	.41
			(.11)			(.11)
Clerical/secular	—	—	.29	—	—	.19
			(.07)			(.07)
Antisystem/prosystem	—	—	.29	—	—	.16
			(.07)			(.07)
Returnability	—	—	—	.99	1.28	1.46
				(.52)	(.52)	(.50)
Inflation rate	—	—	—	—	.018	.038
					(.021)	(.020)
Change in unemp. rate	—	—	—	—	.286	.283
					(.070)	(.070)
Log-likelihood	−1120.5	−536.7	−548.8	−533.1	−531.2	−530.7
N	359	207	208	207	208	208

Note: Entries are partial likelihood coefficients. Standard errors are in parentheses.

The analysis of the interplay of these factors begins with a reestimation of the unified model on the present data set, using the PL method. The results are given in Table 2, Model 1, which follows Laver and Schofield's (1990) lead in measuring party system fragmentation with the more easily interpreted "effective number of parties" index (Laakso and Taagepera 1979), rather than with the fractionalization measure. In other respects, the covariate specification is the same. Given that the two models differ in all other significant features (the type of model, the countries sampled, the definition of key concepts), Model 1 mirrors the results reported by King and his colleagues remarkably well. Among bargaining environment indicators, the number of formation attempts shows about the same effect on government survival as in the earlier model and the polarization effect has become stronger. In light of the preceding discussion, it may seem surprising that the effective number of parties is not significantly related to government survival at the .05 level, but this is also true of the fractionalization measure in the unified model.

We have seen that the inflation and unemployment indicators are not available for all countries and years covered in the sample; the same holds for the European Manifestos Project data.[17] Accordingly, Model 2 reassesses the unified model on the smaller set of cases for which complete data can be obtained. Although coefficient values are affected to some extent by the reduction in cases, the key finding is that all covariates significantly related to government survival in Model 1 remain significant in Model 2. This result greatly facilitates the task of evaluating the two rival hypotheses.

The second of these hypotheses is given a preliminary test in Model 3, using the same subset of cases; the test shows that all three ideological diversity measures described earlier (together with majority status) are related to government survival significantly and in the expected direction.[18] The full effect of the concept will be assessed in subsequent models by summing these measures to form an "ideological diversity index," using the coefficients listed in Model 3 as weights. When substituted for its component measures in Model 3, this index receives a coefficient of 1.00.[19] What happens to this coefficient when the bargaining environment indicators are brought into the picture is, of course, another matter.

Before turning to that issue, there is a second aspect of the ideological diversity interpretation to be

considered. To the extent that the antisystem presence in a legislature reduces the range of possible governmental coalitions, the probability that any member of an outgoing government will participate in the government that replaces it is likely to increase. Where the antisystem presence is substantial, therefore, parties may be tempted to defect from an uncomfortably diverse coalition government in the expectation that they will probably have to be included in the government that replaces it. This propensity can be assessed for each system by determining its "returnability," that is, the proportion of government parties that return to power in the next government following a collapse or early termination.[20]

The two covariates reflective of the ideological diversity hypothesis—the ideological diversity index and returnability—are tested against the significant covariates of the unified model in Model 4. The results reported in this model indicate that ideological diversity and returnability do substantially reduce the roles of the bargaining environment indicators, with only polarization continuing to show a significant effect. The coefficient associated with the ideological diversity index, although still significant, is also lowered very noticeably from 1.00 to .42—a reflection of the variance it shares with polarization. Model 4 thus shows the extent to which the two sets of covariates overlap in their capacity to account for government survival but fails to eliminate either set decisively. The next step is to determine whether the two economic covariates—the inflation rate and the change in the unemployment rate—can achieve this end.

This issue is addressed in Model 5, which adds the inflation rate and the change in the unemployment rate to the significant covariates of Model 4.[21] The principal result engendered by the inclusion of the economic covariates is the reduction in the coefficient associated with polarization, which is now statistically insignificant. It is also notable that the effect produced by the inflation rate is also quite insignificant. These findings suggest that the two covariates overlap to some extent in their effects on government survival—in other words, that systems with a large antisystem presence also tend to have higher inflation rates and that both features correlate with government survival. The implication is that one of these covariates must be removed from the model specification.

From a theoretical standpoint, it is clear that polarization is the one to be discarded. As I have noted, polarization is a poor indicator of bargaining complexity because it registers the narrowing of coalitional alternatives conveyed by the presence of noncoalitionable parties—precisely the reverse of what was intended.[22] In terms of the ideological diversity hypothesis, polarization functions not as a direct cause but, rather, as a surrogate measure, standing in for the ideological diversity within governments and other features that lead to government instability. The results in Model 5 suggest what those other features are: the incentive to defect from governments engendered by high returnability rates and the higher inflation experienced by polarized systems in the present sample.

The effect of removing polarization from the specification is given in the final model of Table 2. The three ideological diversity measures are entered separately in Model 6 in order to demonstrate that each is related significantly to government survival. The model also shows a noticeably larger coefficient associated with the change in the unemployment rate than was evident previously (see Table 1), indicating that controlling for other relevant factors actually enhances the role of that factor. As for inflation, its coefficient has nearly tripled, although its statistical significance is not particularly high.

The PL analyses presented in Table 2 demonstrate that the two economic covariates identified earlier not only contribute to the statistical explanation of government survival but also decidedly tilt the balance in the contest between its two main theoretical interpretations. This conclusion, moreover, is not reached at a price in terms of statistical power: like King and his colleagues' model, Model 6 is capable of accounting for all intercountry differences in termination rates (see Appendix B for details). Nor is it dependent upon the inclusion of support parties in the definition of governments: the model emerges intact even when governments are defined to include cabinet members only; cabinet members plus formally allied parties; or cabinet members plus formal, declared, and undeclared supporting parties. Finally, it does not imply any dependency between the presence of ideological diversity and the impact of the economic covariates. In fact, tests performed to detect an interaction effect in Model 6 failed to turn up any significant tendency for economic conditions to have a greater effect at higher levels of ideological diversity.[23] Economic factors, it appears, affect homogeneous and diverse governments alike.

IDEOLOGIES AND ERAS

Although the role played by economic factors in government survival appears to be independent of the level of ideological diversity within governments, it need not follow that it is unrelated to their ideological *orientations*. It is a commonplace among political observers that left-wing governments concern themselves more with unemployment than inflation and that right-wing governments reverse that order of priorities. These preferences are often linked to the perceived economic interests of their supporters. High unemployment tends to be seen on the Left as a "human" cost of capitalism that disproportionately affects the worse-off and requires government intervention to rectify or at least alleviate; concerns expressed over inflation may appear, however, as a pretext for beating back union wage demands. The Right takes a less zero-sum view of the workings of capitalism; but it does find economic virtue in the creation and maintenance of a favorable business

climate, which typically involves price stability as well as moderation in tax and social spending levels. Thus, the expectation has been, at least until the rightward drift in economic policy of the 1980s, that left-wing governments will cater to their working-class base by pursuing high-employment policies even if it stimulates inflation, whereas right-wing governments will focus on their middle-class constituency's concern for price stability even if it risks jobs.

To what extent are these expectations justified? Table 3 addresses this question by presenting the records of socialist, bourgeois, and mixed governments on the two economic indicators most closely related to government survival: the inflation rate and the change in the unemployment rate. The three government types were created by defining a left–right ordering of parties for each system; dividing these orderings into socialist and nonsocialist components; and coding each government as composed wholly from the socialist component, wholly from the bourgeois component, or containing parties from both components. To assess the performance of the three types of government, mean values of the economic covariates were estimated for each government from interpolated daily values.[24] These means were then averaged across governments of each type.

Table 3 does, in fact, reveal broad tendencies that conform to my outlined expectations. Although inflation and changes in unemployment show considerable fluctuation over the lifespans of all types of governments (as indicated by the standard deviations), socialist governments clearly do experience higher mean levels of inflation than bourgeois governments. Conversely, bourgeois governments typically experience a greater tendency for unemployment rates to increase than do socialist governments. Mixed governments appear to represent the worst of both worlds: inflation rates rivaling those of socialist governments and unemployment growth matching that of bourgeois governments. Interestingly, these patterns hold up even for the final month of existence for each type of government, as the final row for each covariate reveals. This finding suggests that bourgeois governments do not fall when they lose their grip on inflation, nor socialist governments when unemployment escapes their control. Instead, both appear to terminate despite their continuing strong records in these domains.

This conclusion is reinforced by the results obtained when the full model is estimated separately for the three types of government. The estimated coefficients for the economic covariates in these analyses are given in Table 4. Models 7–9 present a pattern of findings that at first glance may appear surprising: changes in unemployment are significantly related to government survival among bourgeois and mixed governments only (Models 7–8), while inflation shows a significant effect solely among socialist governments (Model 9). These relationships cannot be due to the tendency for bourgeois governments to outperform socialist ones on inflation or to be outperformed by them on unemployment, because the models relate each government's economic record only to those of other governments of the same type. Thus, Model 7 associates higher termination rates with bourgeois governments that undergo greater increases in unemployment relative to other bourgeois governments—and the same goes for socialist governments with respect to inflation (Model 9). The most likely interpretation of these results is that bourgeois governments and socialist governments are perceived as being uniquely equipped to handle inflation and unemployment, respectively (a perception borne out by the evidence in Table 3) and are therefore vulnerable only to adverse developments in the other indicator. The finding that mixed governments are "punished" only for unemployment despite their poor records in both areas underscores the dominance of unemployment in the postwar era. It may be that inflation plays a significant role in socialist government terminations only because the socialists' credibility on unemployment is so high.[25]

A second dimension along which the effects of the economic factors on government survival may vary relates to the period during which governments held office. Few would dispute that the oil crisis of 1973 marked a critical watershed in Western economic history, the end of the long postwar economic boom. Certainly, both inflation and unemployment became much worse in the 1970s and 1980s, as Figure 1 indicates. This raises the question whether economic conditions have come to assume a more pronounced

TABLE 3

Inflation and Unemployment in Different Types of Governments

ECONOMIC INDICATOR	TYPE OF GOVERNMENT		
	BOURGEOIS	MIXED	SOCIALIST
Inflation			
Average monthly value	5.37 (95)	7.29 (92)	7.18 (40)
SD in monthly values	.95 (89)	.91 (89)	1.07 (40)
Final month value	5.37 (95)	7.18 (92)	7.39 (42)
Change in Unemployment			
Average monthly value	.24 (95)	.24 (92)	.03 (40)
SD in monthly values	.54 (89)	.36 (89)	.44 (40)
Final month value	.34 (95)	.22 (92)	−.02 (40)

Note: Main entries are mean values for each type of government, with the number of governments given in parentheses. Only governments with valid data for both economic covariates are included. Values for individual governments were derived by dividing government durations into 30-day intervals ("months") and estimating the annual rates of inflation and change in unemployment at the beginning of each interval (using December figures). Means and standard deviations were then calculated over the intervals and the values in the final interval noted.

TABLE 4

Economic Influences on Government Survival, by Type of Government

	MODEL (TYPE OF GOVERNMENT)		
COVARIATES	7 (BOURGEOIS)	8 (MIXED)	9 (SOCIALIST)
Inflation rate	.001 (.036)	.038 (.031)	.178 (.060)
Change in unemp. rate over prec. year	.248 (.098)	.760 (.167)	−.101 (.245)
N	91	78	41

Note: Entries are partial likelihood coefficients derived from reestimations of the full model (Model 6) for the appropriate subset of cases. Standard errors are given in parentheses.

role in government survival in the period since October 1973.

This issue is explored in Table 5, which shows the effects of the economic covariates when the full model is estimated separately for the two eras defined by the 1973 oil embargo. A comparison of Models 10 and 11 provides scant support for the hypothesis of a general increase in the impact of the economy on government survival: while the impact of changes in unemployment is much stronger after 1973, that of inflation shows a comparable decrease. This shift in emphasis, moreover, is not engendered by any increase in the proportion of bourgeois governments in the latter period.

The analyses reported in Table 5 indicate that the principal consequence of the altered economic conditions of the postboom period has been to bring the causal role of changes in unemployment to the fore. The relatively weak role played by inflation in the overall model (Model 6) may be attributed to this fact. This is not to say that inflation is irrelevant to government survival. If the analysis is confined to the 1960s and 1970s, when consumer prices showed the greater change, inflation displays a much more significant impact on survival than changes in unemployment.[26] But the stronger role in the postboom era clearly belongs to the rising rates of unemployment, the roller coaster ride in inflation rates notwithstanding.

TABLE 5

Economic Influences on Government Survival before and after 1973

	MODEL	
COVARIATES	10 (TERMINATIONS BEFORE NOVEMBER 1973)	11 (TERMINATIONS AFTER OCTOBER 1973)
Inflation rate	.146 (.072)	.039 (.029)
Change in unemp. rate from prec. year	.217 (.093)	.453 (.116)
N	105	103

Note: Entries are partial likelihood coefficients derived from reestimations of the full model (Model 6) for the appropriate period. Standard errors are given in parentheses.

DISCUSSION

A conceptualization of outside events and their impact on government survival very different from the events theorists' has guided my research. For one thing, the events under scrutiny are not the idiosyncratic occurrences—the forced landing of a terrorist controlled aircraft or the assassination of a prime minister—that occasionally topple governments but the more systematic and more fundamental economic trends that form a large part of their day-to-day concerns. For another, the interconnection between these trends and government survival is viewed as reciprocal, rather than one-way: governments affect economic conditions, as well as being affected by them.[27]

The influence governments have on their economies can be seen quite clearly in the pattern of relationships linking ideological orientation with success in containing inflation and unemployment. The evidence indicates that bourgeois governments, as expected, do experience lower inflation rates than socialist governments. Conversely, unemployment rates are relatively less likely to rise during socialist administrations. These associations do not establish a causal role for government policy, to be sure; but it is difficult to interpret them in any other terms. Less expected are the findings concerning the reverse link, which suggest that governments are vulnerable only on the economic indicator they are generally less adept at handling. Thus, socialist governments do not terminate when their unemployment records fall behind the norm for governments of that type (presumably because their bourgeois opponents do not provide a credible alternative on the issue); but they are at greater risk if their inflation performance is off the mark. For bourgeois and mixed governments, the immunity pertains to inflation and the vulnerability to unemployment.

The inflation rate and the change in the unemployment rate turn out not only to affect government survival but also to play a key role in the debate pitting bargaining environment and ideological diversity interpretations of the phenomenon against each other. Although the introduction of the ideological diversity within governments seriously undermines the causal role attributed to bargaining environment complexity, the economic covariates dispense a further blow by eliminating the significant effect of its one remaining indicator, the legislature's polarization, or antisystem party presence. In the ideological diversity interpretation, a strong representation for

antisystem parties in a legislature inhibits government survival both by necessitating the formation of ideologically diverse governments from among the prosystem parties and by increasing the likelihood that parties that defect from one government will have to be included in its successor. Model 6 provides evidence for both effects. In contrast, none of the three proposed bargaining environment measures is able to enter the model significantly; nor do they, taken individually or as a group, significantly improve its log-likelihood.

What perspective on government survival is suggested by the findings of this study? Perhaps the most basic feature of government survival uncovered here is the extent to which its immediate sources lie not with the larger parliamentary environment or with the outside world but with the governments themselves. This is indicated both by the importance of government characteristics such as ideological diversity and majority status as well as by the capacity governments have to influence the economic indicators that matter most for survival. The larger political and economic environments are not banished from the picture, however. Not only do parliamentary settings impose constraints on the composition of governments and global economic trends largely escaping their control, but the exact timing by which governments terminate under the pressure of ideological differences must depend, in part, on circumstances of an idiosyncratic nature. Whatever these circumstances may be, a heightened vulnerability to economic trends appears not to be involved. On the contrary, governments of all types, including the minimally diverse single-party governments, are at risk to adverse economic developments.

The mechanism by which these developments undermine government survival remains largely a matter of speculation at this point. Some idea of what might be involved can be gleaned, however, from the economic records associated with different types of termination. In particular, it is notable that governments that were defeated in parliament or resigned before the end of their mandate (excluding those that terminated in order to add a coalition member) had significantly higher mean levels of the two economic covariates than did other governments.[28] This suggests that impatience with government economic strategy or a desire not to be associated with economic failure encourages parties to defect from governing coalitions. A firmer verdict must await more detailed investigations.

Concerning the future, my findings contain both favorable and unfavorable signs for government stability in West Europe. On the favorable side is the finding that survival in office depends on changes in unemployment rates rather than on their actual levels. This result indicates that the higher overall levels of unemployment of the last 15 years may not be in themselves inimical to stability. Nevertheless, in view of the powerful impact of that factor (each 1% rise boosts the termination rate by about one-third in Model 6), any surge in unemployment consequent upon the restructuring of the European economy after 1992 could significantly undermine government durability. The days when the very survival of parliamentary regimes in West Europe was at issue may be long past, but the potential for future governmental instability should not be discounted.

APPENDIX A. DATA SOURCES FOR THE ECONOMIC COVARIATES

The December inflation values for the periods 1946–63, 1964–69, and 1970–89 are derived from consumer price data listed in the International Labor Organization (ILO) *International Labor Review* (1945–63) and *Bulletin of Labor Statistics* (1964–70) and the International Monetary Fund (IMF) *International Financial Statistics* (1970–90), respectively. For years in which only consumer price index values are given, the inflation rate was calculated from annual percentage changes in these values. Following the IMF practise, these rates are calculated only to the nearest whole number whenever the index values themselves are given in whole numbers. Annual values were taken from the IMF *International Financial Statistics Yearbook* (1979, 1983, 1989).

The primary source for the unemployment data is the *UN Monthly Bulletin of Statistics* (1947–90). For earlier years, this source was supplemented, where appropriate, by data from the *International Labor Review*. Because of gaps and inconsistencies in the UN data for Spain and the Netherlands (in the 1980s), data from the Organization for Economic Cooperation and Development (OECD) *Main Economic Indicators: Historical Statistics 1969–88* was substituted in those cases. An "earlier series" category is established whenever a serious discontinuity occurs in rates owing to changes in how they were calculated.

The gross domestic product figures are from the *OECD Economic Outlook* for the period from 1970 and the *OECD National Accounts*, vol. 1, *Main Aggregates, 1950–78* and *Main Aggregates, 1960–88* for the earlier years. An IMF version of this indicator was also coded, based upon the series reported in the *International Financial Statistics Yearbook* (1983–89); but it did not yield substantially different results and is not discussed in this study.

APPENDIX B. MODEL TESTS FOR INTERCOUNTRY DIFFERENCES

The tests involve determining whether the log likelihood improves significantly when a set of dummy variables representing individual countries is added to Model 6. If it does, the implication is that the original covariates are unable to account for all significant intercountry differences in survival rates. Because returnability is constant for individual systems, it is necessary to exclude two country dummies to avoid collinearity. Norway and Sweden were chosen because of their similarities in mean duration and other factors. For cabinet parties only, cabinet plus formally allied parties, and cabinet plus all supporting parties (three of the four definitions of a government) the increases in the log likelihoods generated by the addition of the country dummies are insignificant at the .05 level. (The χ^2 values for these increases are 12.3, 14.7, and 12.0, respectively, with $\chi^2 \geq 16.9$ required for significance at nine degrees of freedom.) The exception, the definition that includes support parties only when their support is openly declared ($\chi^2 = 18.9$), depends on one unusual circumstance: the obligation to treat the British Labour government of 1976–79 as three distinct governments because the Liberals agreed to support it for part of that period. The effect is to make Britain appear less stable than its covariate values and the model would indicate.

The country dummies can also be used to provide a test of the appropriateness of replacing polarization with ideological diversity. The test consists of adding the country dummies to Model 5, which includes both ideological diversity and polarization. The result is that the coefficients for both covariates increase modestly. However, the standard error for polarization increases very drastically; and the coefficient becomes even more insignificant. The t-value for ideological diversity, conversely, is not affected and the covariate remains highly significant. This suggests that polarization's role in government survival, unlike that of ideological diversity, may be an artifact of its high concentration in one or more countries.

Notes

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1. The determination of an appropriate distribution for government duration data (which is equivalent to determining the nature of the underlying trend in government survival net of all measured causal factors) is taken up in Warwick 1992(b), a companion piece to the present paper.

2. Elsewhere, Robertson (1983a) does relate changes in the rate of government collapse in various countries to changes in their unemployment and inflation rates. Individual governments, however, do not constitute the unit of analysis.

3. The log-linear specification is not strictly necessary but has the advantage of precluding negative predicted hazard rates. (A rate of termination cannot be less than zero.)

4. The partial likelihood (PL) method is introduced in Allison 1984 and developed in more detail in Tuma and Hannan 1984 and Blossfeld, Hamerle, and Mayer 1989. The technique does require the assumption that the hazard rates of any two cases be proportional over time; but further testing indicates that this assumption is met in these data (Warwick 1992[b]). It also assumes that any random errors are identical across cases, so that differences among cases are accounted for solely by differences in their covariate values (Tuma and Hannan 1984, 253–54). This assumption is normally violated in practise, but apparently without serious consequences.

5. If X_1 and X_2 are the values of covariate X at times t_1 and t_2, respectively, the interpolated value of X at a time T that falls between them would be

$$X_T = X_1 + \{(T - t_1)(X_2 - X_1)\}/(t_2 - t_1),$$

where all times are measured in days from some fixed date, such as the date the government was formed. The BMDP program 2L, which provides a highly convenient facility for interpolating time-dependent covariate values, was used to estimate all the PL models I report.

6. This is achieved statistically by estimating the contribution of these cases to the overall likelihood by means of the survivor function, which represents the probability of surviving to or beyond any given duration value, rather than with the density function.

7. This follows the practice of Browne, Gleiber, and Mashoba (1984) and King and his colleagues (1990), except in the case of resignations that are not accepted by a head of state. Such resignations are regarded as terminations in this study (although not in the others).

8. This assumption was checked by comparing the relationships between the termination rate and the inflation and unemployment indicators based on annual data with those produced using daily interpolations from December data (to be discussed). The December-based versions generally yielded the stronger relationships.

9. The simple (bivariate) PL coefficient for the actual monthly inflation rate is .056 (t = 3.29), while that for the pseudo-monthly rate is .064 (t = 3.56). This pattern holds up when the other covariates (to be discussed) are included in the analysis. In these tests, the actual monthly data attribute a given monthly rate to each day of that month. The interpolated data attribute the estimated value for the first day of a month to each day of that month. Because of the enormity of the coding task this analysis entails, it was performed only on the 208 cases for which complete data are available. The test is inappropriate for unemployment because actual monthly unemployment rates are contaminated by seasonal effects.

10. They are Austria, Belgium, Denmark, Finland, France (the Fourth Republic), Iceland, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal (from 1976), Spain (from 1979), Sweden, the United Kingdom, and West Germany.

11. The relative lack of precision in consumer price index numbers before 1964 is translated here into a lesser degree of precision in inflation rates, which are calculated only to the nearest whole number for that period. A comparison of results produced with and without the pre-1964 data indicates that the lack of precision of the earlier data affects relationships with government survival only slightly.

12. This could happen, for example, if economic downturns tend to produce majority governments. Since majority governments typically last longer than minority ones, unfavorable economic indicators might appear (spuriously) to be associated with longevity in office. It should also be noted that other factors may also be affecting the relationships reported in the table. Although uncontrolled here, they are taken into account in the subsequent analyses.

13. Using covariates based only upon the most recent unemployment series does not change these results very much: the PL coefficients for the change, proportional change, and absolute change versions are .309 (t = 3.66), .277 (t = 1.29), and .130 (t = 1.38), respectively. In addition, subsequent analyses based on these versions yield the same basic findings as those I shall report for the "all series" versions.

14. The same conclusion holds for the growth in gross domestic product: further testing indicates that its effect on the rate of termination is almost entirely due to its association with inflation and unemployment.

15. The criterion is not foolproof because any of the relationships reported in Table 1 could be suppressed by other factors. For this reason, the final model (6) developed in the next section was reestimated using the various alternative indicators of the economic covariates in the model. In all cases, the signs of the estimated coefficients were the same—and the significance levels lower—when the alternatives were employed.

16. The three sets of scales are Dodd's (1976) economic conflict scales (five-point version); Browne, Gleiber, and Mashoba's (1984) refinement of the Dodd scales; and socialist–bourgeois dichotomous scales based upon my earlier work (Warwick 1979). Each set of scales was subjected to a discriminant analysis using the European Manifestos Project data (Budge, Robertson, and Hearl 1986). The new, or "predicted," scale positions produced from these analyses, together with the first principal component of the European Manifestos Project data, formed the basis for four assessments of the left–right range of each government. Because these assessments intercorrelate so highly, they were subjected to another principal components analysis. The first principal component from this analysis, which accounts for over 95% of their common variance, constitutes the final measure of left–right diversity. Various other formulations, including those based on other source scales and those based on standard deviations rather than ranges, were also tested; this one proved to be the most successful.

17. The European Manifestos Project data exclude Finland, Iceland, Portugal, and Spain. Given the limited period of democratic rule in Portugal and Spain and the gaps in the unemployment data for Iceland and Finland, the number of cases lost through the use of European Manifestos Project data amounts to just 20.

18. This is also true when the entire set of cases for which these measures are available (n = 284) is used.

19. Although other methods of index construction could be utilized, this method was chosen because it leaves all other estimated parameters of the model unaltered.

20. Each system receives a returnability value based on its

record over the entire postwar period. It is assumed that this value reflects an underlying likelihood that is known to parliamentary actors throughout the period.

21. The postelection covariate falls somewhat below significance at the .05 level in Model 4 but was left in the analysis because its weaker role is partly due to the narrowing of the sample, as a comparison of Models 1 and 2 reveals. Had the reduced sample been more representative of the entire data set, it is a reasonable assumption that this covariate would have displayed a stronger and more significant effect.

22. This conclusion is supported by a correlation of .366 ($p < .01$) between polarization and returnability, indicating that the chances for a government party to return to power following an early government termination increase with the size of the antisystem presence in the legislature.

23. The tests consisted of adding interaction terms composed of the product of each economic covariate and the ideological diversity index to the model. Neither of these interaction terms, whether entered singly or together, proved to be significantly related to the termination rate.

24. The ideological orderings were based largely on Castles and Mair's (1984) left–right political scales, supplemented by the Browne, Gleiber, and Mashoba (1984) socioeconomic scales and by country-specific sources where appropriate. Mean economic indicator values for each government were derived by dividing the government's lifespan into intervals of 30 days, calculating the interpolated value at the start of each interval, and averaging these values over the intervals of the government's life. Thirty-day intervals were used in order to facilitate computations and to approximate the monthly intervals for which economic data are usually reported.

25. The patterns shown in Table 4 hold up when only majority governments (to control for the possibility that homogeneous minority governments may actually be dependent on parties of a different ideological orientation) and when only coalition governments are examined.

26. A reestimation of Model 6 for the 1960–79 period ($n = 91$) results in a PL coefficient of .087 ($t = 3.17$) for inflation and .177 ($t = 1.93$) for changes in unemployment.

27. This does not make the economic covariates endogeneous in the full model (Model 6). While their effects on survival do depend on the government's ideological orientation, they are independent of ideological diversity or any of the other covariates entered in the model. (Ideological orientation itself is not a direct cause of survival.) This is indicated by the finding that the introduction of the economic covariates into a model containing only the fixed covariates scarcely changes the coefficients associated with the latter. Thus, the economic covariates occur later in the temporal ordering than the fixed covariates; but they do not play the role of intervening variables.

28. Of the 208 cases utilized in the full model, the 95 that terminated in these circumstances had mean rates of inflation and change in unemployment of 7.3% and .4%, respectively. The corresponding values for the other cases are 5.9% and .0%. T-tests indicate that the differences between the two groups are significant at the .01 level in both instances.

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POLI: AN EXPERT SYSTEM MODEL OF U.S. FOREIGN POLICY BELIEF SYSTEMS

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One of the more fruitful systematic approaches to the study of foreign policy views nations as information processors and explains foreign policy as a function of information and processing thereon. Much of such processing depends on the prior beliefs of decision makers, but for a variety of reasons these systems of prior beliefs are very difficult to analyze using "standard" empirical and modeling techniques. Computational modeling, including expert system technology, provides an alternative methodology that allows such analysis. I present Policy Arguer (POLI), an expert system model of U.S. foreign policy making in Asia. POLI has been very successful in reproducing actual U.S. responses to events in Asia from the 1950s. More important, POLI recreates policy debate in the form of the arguments and counterarguments used to justify policy alternatives. The model explains foreign policy outputs in terms of a complete process from foreign policy beliefs, through debate, to choice.

Foreign policy decision making has been subject to formal analysis only in recent decades. Traditionally, scholars have taken a historical approach to the study of foreign policy, emphasizing descriptive detail and realism. Their investigations, often in the form of case studies, attempt to capture the rich complexity of actors and relations among actors. Formal studies, on the other hand, use different rules of argument and evidence, sacrificing the descriptive detail of the historical approach for much greater explanatory power. They represent the world with idealized models designed to capture in tight focus the important actors and relations among actors.

Despite important differences between formal and historical approaches, they also have similarities. Each attempts to understand the same set of real world processes of foreign policy decision making. Each constructs a representation of these processes according to organizing principles (theory or pretheory). Neither simply describes the world "as it is."

The Policy Arguer Project exploits the similarities between formal and traditional studies to build a bridge between them. Using the computational power of modern computers, formal models can achieve much of the rich complexity of historical representations, while maintaining analytic focus. Computational modeling techniques allow the analyst to raise the level of realism of formal models of foreign policy decision making. In the Policy Arguer (POLI), I apply computational modeling techniques to describe and explain particular foreign policy choices in response to world events, using U.S. decisions on policies toward Asia as my research domain. I shall focus on POLI I, which represents the U.S. foreign-policy-making system of the 1950s.

COGNITIVE APPROACHES TO FOREIGN POLICY ANALYSIS

The intellectual heritage of the POLI Project can be traced to the considerable work, both formal and traditional, on the role of *beliefs* in foreign policy decision making. Snyder, Bruck, and Sapin (1962) introduced the systematic study of foreign policy belief systems as an integral part of the decision-making frame of reference. They advocated the analysis of national behavior as a product of the *subjective* perception and interpretation of a situation by national decision makers, their expectations from various courses of action, and their choice processes. To use Holsti's classic description, a belief system is "a set of lenses through which information concerning the physical and social environment is received. It orients the individual to his environment, defining it for him and identifying for him its salient characteristics. . . . [And it] has the function of the establishment of goals and the ordering of preferences" (1962, 245). Belief systems, as cognitive structures that mediate experience for individuals, are central to the descriptively detailed analysis of decision making.

A number of foreign policy analysts have emphasized the importance of the *images*, embedded in larger belief structures, that policy makers have about the world and about other actors (Boulding 1956, 1959; Glad 1983; Glad and Taber 1991; Holsti 1962, 1967; Jervis 1970, 1976; Tetlock 1983). This work has primarily focused on the pathological impact of images on cognitive processing, particularly on perception and diagnosis. Through selective perception and distortion, decision makers bias incoming information about the world to conform with preconception. In addition, decision makers may develop rigid belief systems containing highly simplified images that

restrict flexibility in the diagnosis of world events and in the choice among perceived alternatives.

In contrast to the emphasis of most image research (that simplified belief structures degrade the quality of information processing), Nathan Leites (1951) and Alexander George (1969, 1979) focused on the necessity of relatively simple belief systems for cognitive functioning. They developed the *operational code* construct to describe these essential cognitive structures. Decision makers' operational codes consist of their fundamental beliefs about the dynamics of international relations and about the efficacy of human action, coupled with their simplifying images of salient international actors (as, e.g., friendly or unfriendly). These beliefs enable decision makers to process information and examine potential courses of action efficiently. Of course, a decision maker's operational code has a tremendous impact on the particular policies that will be advocated or opposed by that decision maker; and as such, it can be a useful analytic tool for the explanation of policy choice (Walker 1983).[1]

Belief structures of this sort may be formally modeled as *cognitive maps*, in which elements of belief and the relationships among these elements are graphically represented (Axelrod 1976). These structural representations of belief systems allow analysts to predict behavior from a decision maker's operational code. Unfortunately, unless they are highly simplified, cognitive maps quickly become too large and unwieldy to analyze using standard methods. Moreover, static representations of cognitive structures like operational codes or cognitive maps cannot deal effectively with important cognitive processes, including reasoning, counterarguing, and learning. For these and other reasons, the cognitive map approach has not been as productive as its early advocates anticipated. Indeed, many analysts of foreign policy decision making have moved away from static representations of cognitive structure toward models or descriptions of the *processes* involved (Bennett 1981; Cottam 1986; Hermann and Coate 1982; Holsti 1977; Janis and Mann 1977; Powell, Dyson, and Purkitt 1987; Purkitt and Dyson 1985).

COMPUTATIONAL MODELING IN INTERNATIONAL RELATIONS

For a formal modeler, the complexity inherent in the decision-making approach is daunting, requiring a modeling language capable of expressing its essential features.[2] Computational modeling, developed in *cognitive science* (roughly, the intersection of cognitive psychology, computer science, and the philosophy of mind) provides a set of tools designed for precisely this problem.[3] Unlike earlier formalisms, computational models can represent rich cognitive structures *and* the processes that link structure to decision. In the field of international politics, these techniques allow the formal analyst to return to the issues and theoretical approaches introduced by Snyder, Bruck, and Sapin (1962).

The application of computational modeling ideas in international relations has a sparse but surprisingly long history, beginning with work at the Massachusetts Institute of Technology in the late 1960s and through the 1970s (Alker, Bennett, and Mefford 1980; Alker and Christensen 1972; Alker and Greenberg 1971, 1976; Bennett and Alker 1977; Pool and Kessler 1969). Rather than attempting a comprehensive review (see Benfer, Brent, and Furbee 1991; Hudson 1991), I will briefly describe several projects in international relations that provided direct inspiration in the development of POLI.

Decision makers must generate meaningful representations of new information before that information can be used in reasoning, that is, they must interpret incoming information. Indeed, how they do this—and in particular the issue of perceptual bias in this process—is a major topic of belief system research, particularly for those who focus on images. Not surprisingly, it is also a central issue in most foreign policy computational modeling projects.

Building on the work of Schank and Abelson (1977), Carbonell (1978) developed a set of programs capable of quite complex interpretations of political events. The primary contribution of Carbonell's POLITICS for my purposes is the application of organized structures of prior beliefs in the interpretation of new events. In this approach, "understanding" is the process of relating external information to internally represented concepts and ideas. An object in the environment (say, North Korea) has meaning to a decision maker only if it can be mapped onto some set of internally represented (i.e., "known" or "believed") concepts (say, "communist" or "regional imperialist"). Decision makers with different internal belief representations will have different interpretations of an event involving North Korea. Although the particular cognitive organization used in POLITICS (ideologies, scripts, and context-dependent inference rules) differs somewhat from that used in POLI, the idea that complex representations of belief provide a fertile ground for event interpretation and the reasoning process is important in both models (see also Seitz 1987).

Thorson and Sylvan (1982) modeled the decision making of John F. Kennedy in the 1962 Cuban Missile Crisis. Kennedy's prior beliefs, on the basis of which he interprets and reacts to events during the crisis, are represented in JFK/CBA as a set of production rules, or IF→THEN statements. Though small by comparison with some other rule-based models, JFK/CBA contains a far more detailed theory of Kennedy's relevant beliefs than could be expressed intelligibly as a cognitive map. JFK/CBA recognizes 14 possible states of the world during the missile crisis, ranging from "Soviet missiles are removed from Cuba" to either the United States or the Soviet Union "initiates a war." U.S. and Soviet actions transform the current state into a new state. Kennedy's beliefs, embodied in the IF→THEN rules, are used to evaluate possible

courses of action, as well as the plausibility of possible Soviet reactions, eventually leading to a decision and a state transformation. A run of the model, beginning with the discovery of missile base construction in Cuba, produces a simulated trace that can be compared to the well-documented historical case. In this basic validity test, JFK/CBA fared very well. The simulation runs of the 1962 crisis, based on the computational representation of Kennedy's belief system, are remarkably similar to the actual historical trace.

JESSE, a computational model of Japanese energy policy making (Sylvan, Goel, and Chandrasekaran 1990) explicitly integrates cognitive and political factors. How, within political constraints, does the decision-making system interpret a situation and select a stored plan of action? These tasks—classification and plan selection—are accomplished in JESSE using a sophisticated combination of knowledge frames and rules. On the basis of prior knowledge, JESSE asks the user a series of pointed questions whose answers allow it to interpret an event according to its classification scheme. JESSE then chooses a plan of action from its repertoire of stored plans. For example, JESSE classified the energy situation following the 1979 Iranian revolution as (in simplified form) a serious threat to the availability of low-cost imported energy for Japan. From a large set of stored plans, JESSE then selected a number of policies to deal with this energy threat, including stockpiling energy and reducing demand.

JESSE and JFK/CBA demonstrate the usefulness of reducing the domain of analysis (see also Job and Johnson 1991). Knowledge-based, or *expert*, systems—a subset of computational models—gain considerable power by focusing on specific domains of knowledge, or belief. They process information on the basis of prior knowledge or beliefs about some problem domain, rather than relying on more general knowledge.[4] As we have seen, JESSE is designed to solve problems of Japanese energy security. It does very well in that domain but would be lost if presented with a problem from U.S. security policy. Like JESSE and JFK/CBA, POLI relies on knowledge and beliefs about a constrained domain, namely U.S. foreign policy making toward Asia. Following convention, I will call the specialized knowledge and beliefs of U.S. policymakers *expertise*—though this term does not necessarily connote accuracy or truth.

Anyone who takes seriously the role of prior beliefs in information processing must consider the question, Whence come prior beliefs? Thorsen and Sylvan (1982) and Carbonell (1978) emphasize the importance of *precedent* in the interpretation of new events (see also Alker and Christensen 1972; Bennett and Alker 1977; Tanaka 1984). Any decision maker's belief system will contain beliefs about "how the world works" in certain contexts; that is, beliefs about how the world worked in similar situations in the past. Considerable evidence from cognitive science suggests that analogies drawn from experience are key components in the interpretation process (Mefford 1985, 1987; Schrodt 1985). Case-based decision models rely heavily (often exclusively) on these stored examples for interpretation of and reasoning about new events. While POLI does not rely exclusively on historical analogies, some of its understanding of events does build on historical cases. For example, one interpretation of the 1950 North Korean invasion of South Korea, among several offered by POLI, rests on a perceived similarity to the behavior of Hitler after the 1938 Munich Conference. In this interpretation, the Korean invasion is understood by analogy with the 1938 case.

THE POLICY ARGUER

POLI is an expert system model of U.S. foreign policy decision making in which several competing views of the world drive the reasoning process.[5] POLI takes events, in the form "actor, verb, target, context" as input and recommends U.S. responses. For example, POLI I recommends 19 U.S. policies for the 1958 Taiwan Straits Crisis, including a direct military threat against China, an upgrade in military readiness, and a protest in the UN Security Council.

Following a typical expert system design, POLI has four components:

1. The *knowledge base* is POLI's representation of U.S. beliefs about policy toward Asia. It is composed of a set of paradigms, or shared belief systems, through time.
2. The *inference engine* controls the processing of information. In POLI, this is a forward chainer using fuzzy inferencing.[6]
3. *Working memory* is the space in which the inference engine compares input with the knowledge base, selects and uses pieces of knowledge to operate on the input, and produces output.
4. The *user interface* is the means of communication between the analyst and the model.

The key tasks in developing an expert system are the extraction and representation of the expert knowledge base that the model will use and the development of the inference engine, which controls how the expert knowledge is used.

Building POLI's Knowledge Base

Unlike experts in many other domains, U.S. foreign policy makers do not draw on a clear, consensual body of expert knowledge. Even in the 1950s, at the height of the Cold War, there was more than one way of thinking about the world. In general, multiple agents of different worldviews compete to affect policy outputs. These competing impulses arise from a set of quite different representations of the world. In fact, agents of different worldviews reason differently about the same input information and come to different conclusions.

For example, in the late 1940s there was a difference of opinion in the American policy making sys-

tem about policy toward China. Key beliefs of different worldviews led to quite different conclusions:

1. Communism is not an implacable enemy but only a temporary one due to mistaken U.S. policies (implying that the United States should change its policy so as not to tread on communist toes, including in China).
2. Soviet communism is fundamentally opposed to U.S. interests, but other brands of communism might not be (implying that the United States should encourage Titoism in emerging communist movements and not automatically oppose all communist movements, including Mao's agrarian movement in China).
3. Communism is monolithic and opposed to U.S. interests, but the communist menace is centered on Europe; if it is contained in Europe, communism will fall of its own dead weight (implying European containment, excluding China).
4. Communism is monolithic and opposed to U.S. interests, and its immediate goals are worldwide; the failure to contain communism anywhere will weaken resistance everywhere (implying global containment including in China).

POLI represents this type of situation with multiple competing worldviews.

Although U.S. foreign policy making is not consensual, neither is it completely idiosyncratic. Patterns, or clusters, of shared beliefs emerge in the system and gain varying degrees of acceptance among individual policymakers. Of course, at extreme discrimination these belief clusters dissolve into distinct idiosyncratic worldviews. But I do not take this approach here. Rather, I work at a level between the individual and the organization: the level of shared belief systems, or *paradigms*.

This project assumes that a limited number of paradigms drive U.S. foreign policy and that they provide a useful organizing device. In this view, the individual proponents of a particular shared belief system are less important than the position of their paradigm in the ongoing foreign policy debate.[7] Though the individuals make the decisions, they do so as agents of one or another paradigm. POLI is similar in this respect to Carbonell's (1978) POLITICS, in which ideologies and other shared belief structures govern the interpretation of political conflicts.

Focusing on the information-processing system at the level of competing paradigms avoids several problems associated with similar work at the individual level of analysis. Most important, since individual policymakers are often strategic political actors, their expressed beliefs change across audiences and political fortunes. Surely such "beliefs" are poor predictors of their actual positions.

This would be a particularly grave problem for the Policy Arguer Project, since POLI is largely built on the public statements of politicians. However, these statements are not used as indicators of an individual's beliefs; rather, I use them as indicators of important beliefs in the system of ongoing policy argument. Public statements are important in their own right, even if they are not sincere representations of the speaker's own view. If they are persuasive, public statements reflect important beliefs in the political system. Indeed, to the extent that leaders feel compelled to express disingenuous beliefs, they are responding to the pressure of strong paradigms in the political culture. Individuals who share important beliefs from the dominant paradigm will push the argument; opponents may be forced to go along. In this perspective, public statements are important indicators of the paradigms that are engaged in policy arguments, even though they may not represent the speaker's "true" beliefs.

Second, some individual-level projects in foreign policy decision making focus too heavily on one important leader. Except in relatively rare crisis situations, the decisions of one leader do not control U.S. policy. A model of U.S. foreign policy making needs to account for the disputes in the system, for the policy arguments. POLI focuses on these policy arguments, both between opposed paradigms and within a single paradigm. Indeed, the treatment of uncertainty and belief in POLI enables me to trace the amount of support within a paradigm and within the system for any given line of argument.

Data Collection. The paradigms used in POLI I are extracted from the *Congressional Record* for 1949–60, through a type of content analysis. The procedure was to (1) identify a set of potential paradigms described in several U.S. foreign policy texts (George and Smoke 1974; Harrison 1978; Jervis 1976; LaFeber 1972; Nathan and Oliver 1985); (2) distinguish a preliminary set of core beliefs for each paradigm, again using the texts; (3) do a pilot study in the *Congressional Record* for a two-year period; (4) modify the set of paradigms and their core propositions according to the findings of the pilot study; and (5) use the modified paradigm set as the basis for a complete content analysis through the decade.

The first four steps in this process identified the bare skeletons of three paradigms in the early 1950s: militant anticommunism (MAC), pragmatic anticommunism (PAC), and isolationism (ISO).[8] A coding plan for the content analysis through the decade was also developed on the basis of this experience. Briefly, this plan entails six steps:

5a. Identify all references in the *Congressional Record* index under the headings "Asia" and "the Far East."
5b. Find and classify each entry by type and date. They may be speeches, insertions of articles, or floor debates.
5c. Read each entry and convert the logic used onto a code sheet. A completed code sheet is essentially a propositional inventory for the entry.
5d. Determine whether each entry conforms to the core propositions of one of the paradigms and classify the entry accordingly. In the 1950s, the

current three-paradigm scheme was sufficient to classify all the entries.

5e. Organize the logic found in the entries for each paradigm according to the structure I shall describe.

5f. Assign a certainty factor (CF), ranging from 0 (indeterminate) to 1.0 (true), to each datum in each paradigm and an overall CF to each paradigm. These CFs, based on the relative frequency of each proposition in the *Congressional Record*, indicate the level of confidence POLI will have in the truth value of the propositions. The certainty factor for a proposition is equal to the proportion of entries that invoked it in the given paradigm and time period. Core rules have the highest CFs because they represent common lines of reasoning in the paradigm. Peripheral propositions are invoked less frequently, are controversial, and have lower CFs. For example, the proposition that the United States should use atomic force in response to an indirect threat (through the domino principle) appears in three of the 37 MAC entries (speeches, articles, or debates) in the 1949–52 period; its CF = 3/37 or .08. Similarly, the CFs for the paradigms, which represent their relative dominance in the decision-making system for the time period, are based on simple frequency counts. For each time period, the proportion of entries supporting a paradigm is assigned as its default CF.

Following this plan, the paradigms for the 1950s, divided into three time periods (1949–52, 1953–56, and 1957–60), were compiled. Each paradigm is a large, complex, frequently inconsistent system of beliefs. I can briefly characterize them, however, by describing their core components.

Militant Anti-Communism. Proponents of MAC argue that communism is a unitary and implacable enemy of freedom and the West. All of the major ills in the world are attributed to international communism under Soviet leadership. There is no room, in this view, for long-term rapprochement with the enemy. In addition, divisions within communism are unlikely. Thus, there is no reason to court disgruntled communist leaders to weaken the Soviet hold on the Communist movement. There is also a clear image of a world divided strategically into a center and a periphery. Those nations that are strategically central must be defended "at all costs." But because the strongest version of the domino theory is core to the paradigm, even strategically peripheral locations must be defended if threatened. Otherwise there is great danger that a series of "dominoes" will fall and threaten some intrinsically central location. A third set of core beliefs for the MAC paradigm revolve around the "bully metaphor" and derive from a counterfactual reconstruction of the appeasement of Hitler by the Allies at Munich. According to this view, Hitler could have been stopped had the Allies stood up to him. International aggressors, like bullies, are basically cowardly; they will back down from firm opposition. Thus, MAC generally prescribes prompt, decisive, unambiguous, and firm actions.

Pragmatic Anti-Communism. Proponents of PAC believe that totalitarianism is the enemy. Communism is not necessarily monolithic; thus, individual communist leaders like Marshall Tito of Yugoslavia may be split off from Moscow's influence. In addition, PAC argues that the spread of communist/totalitarian movements primarily stems from basic social and economic inequalities rather than from military pressure. The PAC paradigm sees a more subtle form of domino theory rooted in the vulnerability of struggling nations that have been recently decolonized. It follows that the best defense against encroaching totalitarianism is a remedy for these social and economic ills. The PAC paradigm advocates economic and material solutions to most problems, but it is willing to use military power when allies and interests are directly and militarily threatened.

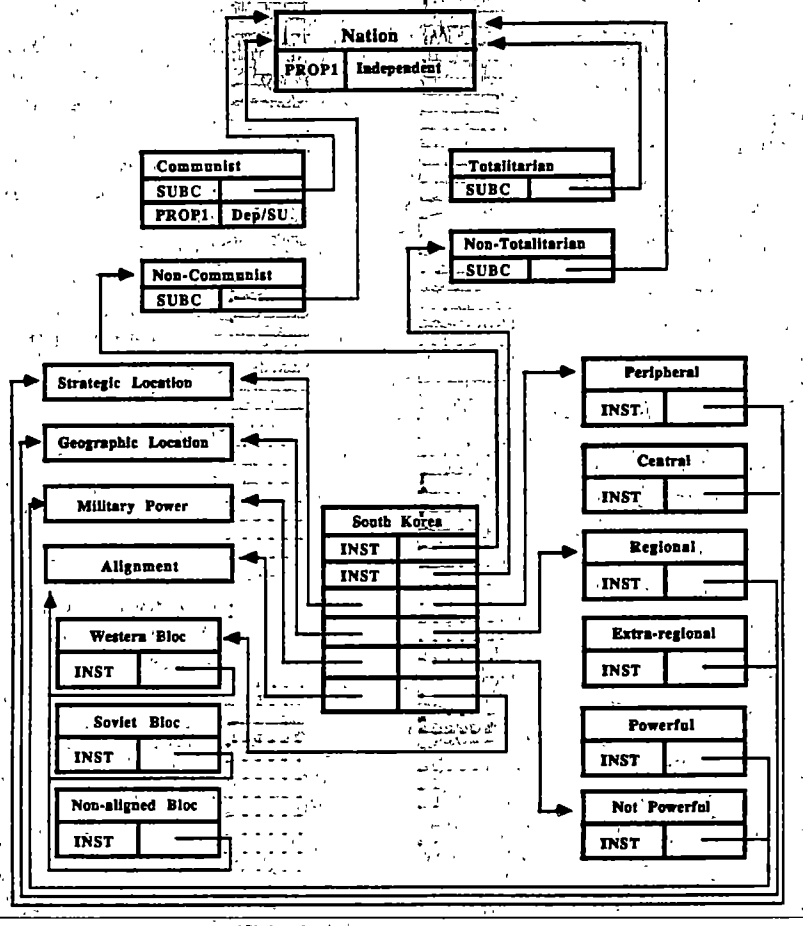
Isolationism. Isolationists argue that the United States should again withdraw from international politics. There are two basic components—one practical, the other moral. In practical terms, the ISO paradigm argues that the United States does not have the resources to maintain world leadership without degrading the quality of life at home. The concern of U.S. leaders should thus be internal. In addition, it would be imprudent to waste resources on a long struggle with communism. In moral terms, the United States can only maintain the ethical high ground by refusing to become involved in international adventures. Any appearance of complicity in colonialism, in particular, would damage U.S. integrity.

Paradigm Structure. Each paradigm in the knowledge base contains two types of knowledge. Procedural knowledge, in the form of IF→THEN production rules, codes how something is done ("IF communists invade South Korea, THEN send military forces"). Declarative knowledge describes important concepts and their relationships ("France is an ally"). These two types of knowledge in a paradigm serve distinct functions in the model: declarative knowledge is used to interpret input events, while procedural knowledge is used to reason from the interpreted events to conclusions.

The interpretive beliefs necessary to a paradigm (i.e., its declarative knowledge) are represented in POLI by a *frame system* with inheritance. One can think of frame systems of knowledge representation as hierarchical relational data bases. Each object in the system (in this case, each regional actor, class of actors, or property of actors) occupies a frame that may have any number of slots representing properties of the object. An object's properties can originate with itself or with other objects ("fillers") to which relational arrows point (i.e., objects "inherit" properties from classes to which they belong). In addition,

FIGURE 1

Militant Anticommunist (MAC) Frame System for South Korea



Note: PROP1 is the first in a list of properties of the object; SUBC means the object is a subclass of the category to which an arrow points; and INST means the object is an instance of the category to which an arrow points.

since this information is belief, rather than fact, each object's "degree of membership" in superclasses and the degree of confidence that the paradigm has in its assignment of characteristics must be considered. An empirically derived certainty factor is assigned to each element of belief in the frame.

Figure 1 gives an abbreviated example of interpretive beliefs as a frame system without CFs specified. Of course, this information is actually linked in a large frame system with all other regional actors, though it is difficult to present even two nations on one page clearly, let alone the entire system.

The *production rules* in a paradigm allow reasoning from an interpreted event to some policy output through inference chaining. These rules take the general form of IF→THEN propositions. Some take situations as their conditions ("IF an important enemy attacks an important friend"), while others take goals as their conditions ("IF you wish to oppose the attacker"). In addition, some give goals as their consequences ("THEN oppose the attacker"), while others give executable actions ("THEN attack the attacker"). The bulk of each paradigm consists of a long list of these propositions. Of course, like the declarative information in the frame system, procedural information is uncertain; each rule has an empirically derived CF that indexes the paradigm's confidence in the rule.

POLI'S INFERENCE ENGINE

The inference engine applies beliefs as represented in the knowledge base when POLI is confronted with an event. Processing begins when information about an event (e.g., North Korea attacks South Korea in 1950) is placed in working memory (through the user interface). The inference engine uses declarative beliefs to interpret the event, procedural beliefs to reason forward from its interpretation to a set of possible policies for responding to the event, and then selects a subset of these policies for implementation.

The Interpretation of Events

Suppose I present POLI with an event involving South Korea. The inference engine will search the paradigms in its knowledge base for declarative information about South Korea on which to base its interpretation. It will "understand" South Korea to be the collection of attributes assigned through the declarative information. Of course, like human experts, POLI is most efficient when it has a lot of interpretive information; but POLI does degrade gracefully, producing limited interpretations and inferences from partially understood events.

Consider POLI's interpretation of South Korea based on the abridged MAC frame system in Figure 1. Note first that South Korea is an instance of a noncommunist nation. Since noncommunist nations are a subclass of nations, South Korea will inherit the properties of nations (e.g., independence as an actor). South Korea is also listed as a nontotalitarian nation and has the properties Strategic Location = Peripheral, Geographic Location = Regional, Military Power = Not Powerful, and Alignment = West Bloc. In addition, each piece of declarative information has an associated CF, which the inference engine can use to gauge how much confidence it should place in an interpretive belief. The MAC paradigm might be very confident that South Korea is weak (CF high), but have little confidence in the inference that South Korea is strategically peripheral (CF low). Other information that the paradigm considers important is represented in the complete frame system (e.g., trade relations and the existence of an insurgency movement).

Similarly, each paradigm in POLI interprets the entire event, broken into its components: actor, target, verb, and context. When POLI is unable to interpret part of an event (say, the verb is unrecognized), it alerts the user to the problem and proceeds on the basis of the information it can interpret.

Inference Chaining and Fuzzy Reasoning

A paradigm recommends specific executable behaviors by reasoning through an inference chain from the interpreted event to the output policy recommendation. This inference process begins when the conditions of an IF→THEN rule match "facts" in working memory (initially, just the interpreted event). The rule "fires," and the consequences of the rule are added to the list of known facts. These inferred facts may enable other rules to fire; and, in a cascading effect, more beliefs may be inferred in response to the event, triggering still more rules.

Since I am interested in the diverse arguments that might emerge from these paradigms, there is little restraint in POLI on the growth of inference chains; I want all possible arguments to develop. To avoid infinite looping, no rules can appear twice in the same chain. Otherwise, inference chains grow as long as rules continue to fire. As you might imagine, POLI needs some mechanism for distinguishing among the hundreds of inference chains that can grow from a paradigm. In addition, the multiple paradigms in POLI produce competing outputs and arguments. POLI resolves conflicts of this sort through fuzzy reasoning based on CFs. The inference engine calculates a certainty factor for each of the competing conclusions, allowing POLI to distinguish strong from weak inference chains.

Reasoning in POLI is fuzzy in the technical sense of using fuzzy logic. Based on Zadeh's (1965) original theory of fuzzy sets, fuzzy logic provides a means of formally representing degrees of belief. The membership function of a fuzzy set maps to the [0, 1] segment of the real line, rather than to the two element set (0, 1), which allows a continuum of membership values (Cioffi-Revilla 1981; Negoita and Ralescu 1987; Seitz 1985; and my earlier work, Taber 1991). In fuzzy logic, this property is applied to capture the partial truth value of conditional statements and the natural vagueness of declarative concepts. The conditions in a production rule and the rule itself carry partial truth value. In Boolean logic, based on the theory of crisp sets, such conditions must be either true or false, leaving no room for degrees of truth. This would be a serious inflexibility in a belief system such as POLI. Decision makers clearly have uncertain—sometimes conflicting—beliefs; and they frequently use verbal hedges to qualify their beliefs. A descriptively accurate model of this process must allow reasoning from partial truth or vague meaning.

Surprisingly, given the importance of uncertainty in political beliefs, most of the major political computational models do not address the issue; the notable exceptions are JESSE (Sylvan, Goel, and Chandrasekaran 1990) and the Superpower Rivalry models (Seitz 1987). In the larger context of cognitive science, however, the manipulation of vague and subjective knowledge has been a major focus (Negoita and Ralescu 1987). Several formalisms exist for representing uncertainty, including probability theory and fuzzy set theory.

Where one wishes to express the vagueness of a natural language concept or belief, the approach based on fuzzy set theory enjoys an important advantage over a probabilistic approach (Cioffi-Revilla 1981; Seitz 1985). Consider the concept *powerful*. Its intuitive meaning concerns some vaguely defined quality that actors may have more or less of. But probability theory, based on the mathematics of crisp sets would force us to define some ideal or absolute meaning for the concept. To say, "China is quite powerful" would be to say that there is some fairly high probability that China has this ideal quality *in full*. This is quite different from the natural meaning of the phrase. Fuzzy set theory, on the other hand, focuses our attention on the relative *amounts* of this quality nations have. Now to say, "China is quite powerful" is to say that China, *relative to other actors* has a substantial amount of this quality. Indeed, we can assign a numerical value to the fuzzy variable *powerful* for China that will be understood in relation to the values assigned to other actors. In the POLI project these CFs

are empirically derived as part of the content analysis I have described. They are based upon the degree of support for a particular belief within the given paradigm.[9]

The production rules in POLI are fuzzy conditional statements in that certainty factors (CFs) are associated with the elements of the rules. Here I will simply illustrate the algorithm for fuzzy inferencing used in POLI (the derivation is presented in Taber 1991). Consider the rule "IF a nation has large oil reserves, THEN that nation is rich," which has a CF of .3. This rule means that the expert system has .3 confidence in the conclusion "That nation is rich" when it is certainly true that "that nation has large oil reserves" (i.e., when the CF for the premise is 1.0). When the system receives information that "Saudi Arabia has large oil reserves" with CF = .9, we can infer the information that "Saudi Arabia is rich" with a calculable degree of belief. Through an application of the compositional rule of inference, we compare the CF of the new information (.9) with the CF of the known relationship (.3), taking the smaller. The expert system would report the inference that Saudi Arabia is rich with CF = .3. Now if we input the information that "Bangladesh has large oil reserves" with CF = .01, the expert system would only have .01 confidence in the inference that Bangladesh is rich.

Taking a more complex example from POLI, the event "The Vietminh attack France at Dienbienphu in 1954" is interpreted (in part) by MAC as "unimportant enemy (.6) attacks (.9) important friend (.4)." In addition, the MAC paradigm includes the very uncertain production rule, "IF enemy attacks important friend, THEN drop an atomic bomb on the enemy (.08)."

Since in fuzzy logic, conjunctions of fuzzy elements carry the truth value of the minimum fuzzy element and disjunctions carry the truth value of the maximum fuzzy element, the condition of the rule in the example will have a global CF of .4, the minimum of the truth values for the three elements of the premise. But this rule has a very low CF (.08) associated with it, and this low CF must affect the conclusion. Using the compositional rule of fuzzy inference, the smaller of the rule CF and the premise CF is assigned to the inferred conclusion, resulting in a .08 level of confidence in the recommended action, "Drop an atomic bomb on the Vietminh."

These fuzzy algorithms make intuitive sense. *All* the elements must have some truth value for a conjunctive argument to hold; so a conjunctive argument is only as strong as its weakest link. Conversely, a disjunctive argument should be as strong as its strongest element, since *only one* of the elements need be true. Finally, in the context relevant to POLI, where rule CFs define the inferential relationship between premises and conclusions, one may view the compositional rule of fuzzy inference as a special case of conjunction. It is the logical conjunction "Premise is true (CF)" *and* "Rule is true (CF)." Both must have some truth value for the conclusion to have any truth value.

The processor must still select among the recommendations of all of the paradigms. Recall that POLI's inference engine produces three sets of uncertain recommendations—one for each paradigm. To the decision-making system, paradigms are more or less true descriptions of the world at any one time. They are informal theories about regional processes. One paradigm may dominate discussion among policymakers for a period because the decision-making system believes it to be a good representation of the world. Other paradigms still create arguments and inferences, but their individual arguments cannot be more persuasive than the paradigm as a whole. POLI limits the confidence it places in particular inferences of a paradigm to the confidence it places in the paradigm. Again, following the rule of fuzzy conjunction, POLI assigns to each recommendation of a paradigm the minimum of the paradigm's confidence in the recommendation and POLI's confidence in the paradigm itself.

Recall the Dienbienphu example that resulted in the inference "Drop an atomic bomb on the enemy" with CF = .08. Even if the paradigm that makes this recommendation is quite dominant (as MAC was in 1954), POLI would only accept this inference with .08 confidence—the CF of the inference *within* the MAC paradigm. Now consider the weaker isolationist paradigm (CF = .025). Even if it made an inference of which it was quite sure—"Remain neutral (.9)"—the inference would only be accepted by POLI (*outside* of the ISO paradigm) with a .025 CF.

An input event, then, produces several recommended policies from the paradigms, each with a CF attached, and each with supporting arguments. POLI selects a subset of these recommendations upon which to act, using the CFs as a guide. One selection mechanism in POLI generates a probability distribution over the CFs of all policies in the recommendation set and chooses policies stochastically. In the Dienbienphu example, though neither ISO's recommendation to remain neutral nor MAC's atomic policy would be very probable, there would still be a small chance of each. A second approach deterministically selects the recommendations with the highest CFs.

VALIDITY AND POLI AS AN ANALYTIC TOOL

Computational modeling techniques allow foreign policy analysts to increase the descriptive detail of their models without losing analytic focus. I shall demonstrate the descriptive complexity possible using the Policy Arguer as an analytic tool while also addressing the issue of validity.

In discussing an information-processing model like POLI, we must distinguish outcome from process validity, where outcome validity is the familiar notion of correspondence between the model's predictions and behavior from the real world system. Do the

outputs of the information-processing model—treated as a blackbox—match, to an acceptable level of statistical significance, the outputs of the empirical system? Process validity, on the other hand, concerns the correspondence between the mechanisms in the model and those in the real world system. If an important virtue of a computational model is that it is *not* a blackbox, we must demonstrate its reasonableness as a process model.

Outcome Validity

The outcome validity of POLI was checked using a chronology of Asian events in the 1950s, compiled by the Royal Institute of International Affairs, as a test data set ($N = 161$). For each event in the test data, the actor, target, and verb were identified, along with the U.S. responses. An average of slightly more than three responses per event were given in the test data. It was then straightforward to run each event through POLI and compare the recommended policies with the policies actually chosen by the United States. If the historically selected policy or policies were highly recommended by POLI, where a "high recommendation" was operationally defined as "having one of the top five CFs in the recommendation set," it was counted a success for the model.[10] If *all* of the actual U.S. responses identified in the test data set were not highly recommended by POLI, it was counted a failure for the model.

POLI did remarkably well in the decade of the 1950s, succeeding on 138 out of the 161 events. For just under 86% of the events in the test data set, POLI's highest recommendations included all of the policies chosen by the U.S. foreign-policy-making system. In addition, an analysis of the 23 failures shows that POLI came close for most of them. Either a chosen policy was just outside the highly recommended set and would still be a likely choice if the selection were stochastic or else the real world policy was too imperfect a match with POLI's recommendation. For example, POLI (based on MAC reasoning) highly recommends that the United States accuse China of imperialism in 1953 in response to China's attempt to increase trade with Great Britain. The actual policy chosen by the United States, as identified in the test data set, was "attempt to block increased trade." This is not a match, but one could argue that the intention is the same. In addition, an exact match in this example fell just outside the set of highly recommended policies. MAC pushes the policy "oppose increased trade between China and Great Britain," but only with .17 confidence; the cutoff for high recommendation was .282 in this case.

A potential problem exists with this analysis, however. The knowledge and beliefs that POLI uses to interpret events and reason to conclusions are extracted from speeches in Congress in the same time period as the events. Although the test data set is operationally independent of the *Congressional Record*, some of the speeches and articles respond directly to particular events that appear in the test data set. In other words, the logic and arguments on which POLI is based are, in part, the congressional responses to events in Asia. Because these events do appear in the test data set, there is a possibility of circularity.

To find out if this problem was serious, I used the knowledge base from the prior period to operate on events. Thus, I used the 1949–52 knowledge base for 1953–56 events, the 1953–56 knowledge base for 1957–60 events, and the 1957–60 knowledge base for 1961–63 events.[11] POLI remained very successful, even for this more stringent test. Fully 75% (84/112) of the actual U.S. responses to the events matched POLI's highest recommendations.

But how should we assess the success of POLI? Is 86% correspondence "good"? To gauge POLI's success, I will compare it with the performance of two null, or baseline, models: the *inertia model*, where existing policies are repeated in response to new events, and the *modal behavior model*, where frequently used policies are selected in response to new events. These null models are variants of the well-known organizational process model (Allison 1971), and we might expect them to do reasonably well. I will instantiate the two null models using the chronology of Asian events as described and compare POLI's performance with each.

But if I count successes and failures for the models at the descriptive level I have used for POLI, neither of the null models can approach POLI's performance. POLI matches actual U.S. responses to Asian events in the test data in much greater detail. In addition, neither of the null models can interpret events. The events must be categorized for them. POLI's task, which includes the interpretation of "raw" events and the detailed recommendation of policy responses, is considerably more comprehensive than either of the null models can attempt. Indeed, the fact that POLI does more theoretical work is already a powerful recommendation.

Table 1 presents the coding scheme used to reduce the level of description of the test data set. Quantifying event types and U.S. responses gives us a more typical events data set,[12] allowing a fairer test of the null models and a clearer comparison with POLI. Each of the 161 events in the test data set was placed in one of the eight categories, where plus signs denote a cooperative verb, minus signs, a conflictual verb. Thus, when Soviet leaders visited India in 1955, promising development assistance, it was a type 4 event: an enemy of the United States did something cooperative to a friend of the United States. Next, the U.S. response to each event was coded according to the ordinal scale in Table 1. Since this response is directed, we also code whether it is aimed at the actor in the event (1), the target in the event (2), or a third party (3). For example, the United States increased its development assistance to India to counter the 1955 Soviet offer. This response is coded a 2–2, the first digit indicating the position of the response on the scale, and the second, that it was directed at the target in the original event (India). In addition, the United States denounced the Soviet Union as impe-

TABLE 1

The Categories of Behavior

Event Types

1. Enemy + Enemy
2. Enemy − Enemy
3. Enemy + Friend
4. Enemy − Friend
5. Friend + Friend
6. Friend − Friend
7. Friend + Enemy
8. Friend − Enemy

U.S. Response Categories

1. Military Aid
2. Economic/Development Aid
3. Verbal Support
4. Positive Propaganda (e.g., symbolic support for nationalism)
5. Persuade, Negotiate
6. Negative Propaganda (e.g., name calling)
7. Verbal Condemnation (e.g., diplomatic protests)
8. Verbal Threat
9. Nonverbal Threat (e.g., movement of troops)
10. Direct Military Force

rialistic, which would be coded a 6–1: a 6 on the response scale and a 1 indicating that the response was directed at the actor in the event (the Soviet Union).

Since it gives POLI a major advantage in the test, I do not consider more than one U.S. response to each event in the test data set. Rather, I will only include the most extreme U.S. response. For each event, the U.S. response closest to an endpoint on the scale was coded. Note that this gives the null models their best chance of producing successful predictions in the data.

According to the inertia model, the United States will continue its existing policy given a particular type of event. For example, when the United States is confronted with a type 4 event, it searches history for the most recent occurence of a type 4 event and repeats the policy it used then.

The modal model says that the United States will apply its most frequently used policies in response to an event. A naive modal model might only consider the single most frequent response for each type of event. For example, the modal response for a type 4 event is a verbal condemnation of the actor (7–1). Every time a type 4 event occurs, the naive modal model selects 7–1. A more sophisticated modal model, on the other hand, would consider the two or three most common responses to each event. In this case, the model would propose, in response to a type 4 event, *either* a 7–1 or a 1–2 (military aid to the target). If either of these was the actual U.S. response, it would be a success for the sophisticated modal model.

We should expect the modal models to do very well in that they apply future information in selecting a response—they provide more hindsight than prediction. Of course, as I have noted, POLI also applies some beliefs developed on the basis of current events, though the circularity is not nearly as direct as in the modal model. POLI, of course, remained very successful even when this potential circularity was eliminated (75% success). As a final null model, we will use a version of the modal model that does not consider future information. The modal intertia model applies the most frequent responses to events of a given type from history at the time of the event. Instead of doing a frequency count across the decade for responses to each event, we now consider only the frequencies of responses that have occurred before the event. Thus, response 1–2 was the modal inertia response to a type 4 event in January of 1950; but by 1955, the modal inertia response was either 7–1 or 1–2.

TABLE 2

POLI's Predictive Accuracy Compared with Several Null Models

MODELS	CORRECT	INCORRECT	% CORRECT
POLI	143	18	88.8
Sophisticated modal	105	56	65.2
Naive modal	75	86	46.6
Modal inertia	66	87	43.1
Inertia	65	88	42.5

Note: Each of the five models makes predictions about the U.S. response to events in Asia in the 1950s, which are tested against the actual behavior of the United States as compiled by the Royal Institute of International Affairs (N = 161). The inertia models do not generate a prediction the first time an event type is encountered (N = 153).

Finally, POLI's responses to the events were coded using the same scale. Since POLI recommends multiple responses, only the one with the highest CF was coded. If several policies tied for the highest CF, the most extreme response was used.

Table 2 directly compares the four null models and POLI. Note first that POLI improves a little on its earlier performance, because the test is less stringent. Some of the 23 misses in the earlier test are matches at this level of description. The sophisticated modal model also does fairly well (65%), though POLI's proportion of successful matches is significantly better. Using the best null model (the sophisticated modal model) as a baseline for success in predicting the historical trace, the probability of achieving POLI's success if it were not in fact a better correspondence model is very small ($p < .00001$ using the binomial test). POLI clearly outperforms the null models.

Process Validity. It should be clear, of course, that POLI makes many recommendations for each event that are not close to the actual responses of the U.S. government. This is not a defect. The Policy Arguer is a process model of the decision-making system and not just of policy outcomes. It models the diverse arguments in the system, even the ones that are not

acted upon. The previous analysis demonstrated that most of the time, POLI's strongest recommendations are the things that the United States actually did. But a more compelling concern is the process by which those outcomes are reached.

In many respects, the most interesting feature of POLI is the detailed argumentation that supports its recommendations. This section recreates a case study of the U.S. response to the outbreak of the Korean War. Though we focus on this one case here, a similar case study emerges from every POLI run.

Unfortunately, detailed case studies can do no more than establish the process *plausibility* of POLI. As others have convincingly argued, correspondence tests of process generally are a species of face validity, requiring a different mode of analysis than the familiar statistical approach to outcome validity (Job and Johnson 1991; Schrodt 1991; Sylvan, Goel, and Chandrasekaran 1990; Whicker and Sigelman 1991). I will support the face validity of POLI as a process model by detailing POLI's response to the Korean case and comparing it to the reasoning and arguments attributed to the United States in historical case studies.

On June 25, 1950 troops of the North Korean army crossed the border between North and South Korea in a massive surprise attack. Over the next few days, U.S. leaders made three key decisions concerning this invasion: (1) that the United States had to intervene militarily, despite earlier assurances that South Korea (and even Taiwan) was outside of the U.S. defense perimeter in Asia; (2) that they would use the collective security mechanisms set up in the United Nations Charter; and (3) that the intervention must be limited in nature.

When POLI is confronted with the event "North Korea attacks South Korea in 1950," each of the three paradigms interprets the event and then reasons forward from the interpreted event to a set of policy options (Table 3). The trace of this process provides POLI's explanation of each policy.

The militant anticommunist paradigm (MAC) characterizes North and South Korea according to the attributes that the paradigm considers important. For example, MAC believes that both Koreas are strategically peripheral, that North Korea is East-aligned and South Korea West-aligned, and that both Koreas are militarily weak (though MAC is far more confident that South Korea is weak). The values of other attributes are also assigned from the MAC knowledge base. Several basic interpretive rules then operate on this declarative information to classify North and South Korea as unimportant expansionist enemy and unimportant friend, respectively. Finally, MAC interprets the verb *attack* to mean "physically threaten." Of course, MAC's real understanding of *attack* is found in the way its rules actually use the verb *physically threaten*.

Given MAC's interpretation of the actor, target, and verb, POLI can now apply its procedural knowledge to infer courses of action in response to the event. The following display provides the MAC reasoning behind the decision to intervene militarily:

TABLE 3

POLI's Policy Options for the 1950 North Korean Attack

PARADIGM[a]	POLICY OPTIONS	CERTAINTY FACTOR[b]
MAC	Communicate commitment to protect target	.560
	Increase military aid to target	.560
	Increase military support to regional non-communists	.500
	Military blockade on actor	.500
	Send military advisors to target	.500
	Act through the U.N.	.440
	Send U.S. troops to target	.440
	Investigate the event/problem	.440
	Trade embargo on actor	.440
	Reduce aid to actor	.400
	Communicate opposition to regional hegemony	.400
	Aid friends in region who oppose actor	.350
	Diplomatic sanctions against actor	.350
	Increase material support to regional non-communists	.330
	Increase trade with target	.280
	Increase economic aid to target	.280
	Increase industrial aid to target	.270
	Increase global military readiness	.220
	Communicate the containment line in Asia	.220
	Threaten use of atomic bomb	.210
	Directly attack communist actors in Asia	.170
	Subvert communist actors in Asia	.170
	Do nothing	.110
	Withdraw diplomatic recognition of actor	.110
	Withdraw envoys	.110
	Pressure allies to sanction actor	.110
	Consult allies about intended policy	.110
	Lodge protest in U.N.	.056
	Link economic aid to target compliance on other issues	.056
	Attack actor	.000
PAC	Avoid direct U.S. military involvement in Asia	.250
	Consult with Asian leaders before taking action	.250
	Sponsor conferences to gather input from Asian leaders	.250
	Restrict direct use of U.S. ground troops	.250
	Apprise Soviets of limited U.S. objectives	.250
	Do not directly attack Soviet territory	.250
	Raise problem in U.N. Security Council	.250
	Support only popular governments in Asia	.250
	Promote individual freedoms and human rights in Asia	.250
	Help Asian leaders address immediate welfare concerns	.250
	Help aspiring Asian democrats to write constitutions	.250
	Accuse Soviet communism of imperialism in Asia	.250
	If aid is given, do not attach strings to the aid	.250
	Encourage civil liberties in target	.250
	Monitor use of U.S. aid by target	.250
	If target is to be aided, give slowly so target can absorb it	.250
	Increase educational aid to target	.250
	Increase economic aid to target	.250
	Increase technical aid to target	.250
	Promote principles of majority rule, freedom, and equality	.250
	Publicly repudiate colonialism	.250
	Do not approach Soviet territory in military action	.240
	Apprise China of limited U.S. objectives	.230
	Do not directly attack Chinese territory	.230
	Do not approach Chinese territory in military action	.230
	Consider minimal use of air power first	.210
	Publicly support target	.140
	Encourage Asian nationalists to redistribute wealth	.140
	Tolerate independent foreign policies of Asian allies	.140
	Increase humanitarian aid to target	.140
	Freeze military aid to target	.110
ISO	Remain neutral about event	.125
	Offer to mediate dispute between actor and target	.125
	Reduce defense commitments in Asia	.125
	Rely on air and nuclear forces for defense of U.S.	.125
	Withdraw U.S. forces from Asia	.125
	Do not get involved in the internal affairs of Asian nations	.125
	Withdraw from U.N. Security Council	.125
	Reduce U.S. financial commitment to U.N.	.125
	Ask U.N. to leave New York	.125
	Do not join major U.N. peacekeeping efforts	.125

[a]The paradigms are militant anti-communism (MAC), pragmatic anti-communism (PAC), and isolationism (ISO).
[b]These certainty factors (CFs) represent the confidence POLI has that the given policy option would be a good choice. They are based on the empirically-derived CFs for the expert knowledge and the fuzzy inferencing techniques described above.

IF Actor physically threatens target
THEN Actor is important (.6)
AND Target is important (.6)

IF Political system of actor is communist
AND Political system of target is non-communist
AND Actor and target are contiguous
AND Actor physically threatens target
THEN Target is in danger of falling communist (.9)

IF Target is friend
AND Target is important
AND Target is in danger of falling communist
THEN Goal: Prevent target from falling communist (.6)

IF Goal: Prevent target from falling communist
THEN Goal: Reinforce military preparedness of target (.56)

IF Goal: Reinforce military preparedness of target
THEN Policy: Increase military aid to target (.56)
AND Policy: Send military advisors to target (.5)
AND Policy: Send U.S. troops to target (.44)

One of the features of the MAC paradigm is that targets and actors take on new importance when they are active. This comes from the belief that all parts of the world are linked together, so that peripheral nations become the front line when they are threatened (or when they threaten). This was one reason that U.S. leaders explicitly led North Korea and the Soviet Union to believe that South Korea was outside the defensive perimeter of the United States yet responded vigorously to the attack when it happened (Paige 1968, 66–7). Until the attack occurred, Korea *was* outside the U.S. defense perimeter. The belief that peripheral nations take on new importance when militarily active—an essential component of the domino theory—is represented in MAC by the first rule in the display.

The second rule represents another key part of the domino theory: noncommunist nations that border communist nations are in great danger of falling whenever they are the target of communist aggression. Thus, a cascade of collapses can be expected if any nation is allowed to fall. United States leaders used this line of reasoning as their major argument for intervention (George and Smoke 1974, 162–72; Glad and Taber 1991; Nathan and Oliver 1985, 120; Paige 1968). In his *Memoirs*, for example, Harry Truman wrote: "I felt certain that if South Korea was allowed to fall Communist, leaders would be emboldened to override nations closer to our own shores. If the communists were permitted to force their way into the Republic of Korea without opposition from the free world, *no small nation would have the courage to resist threats and aggression by stronger Communist neighbors*" (1956, 333, emphasis mine).

The third rule in the display sets up the goal "Prevent target from falling communist" in response to the facts in working memory: "Target is friend," "Target is important," and "Target is in danger of falling communist." In order to achieve this goal, several subgoals are set up, including the one given in rule 4. Finally, rule 5 operates on this subgoal by proposing three policies: "Increase military aid to target," "Send military advisors to target," and "Send U.S. troops to target." All three of these specific forms of military intervention, recommended by MAC, were actually taken by the United States in response to the North Korean invasion of South Korea in 1950. More importantly, the reasoning found in the display corresponds to the reasoning in the system—or at least to public justifications (Paige 1968; M. Truman 1973, 504–10).

MAC follows several other lines of reasoning to arrive at the policy recommendation "Act through the UN." One is related to the costliness of sending U.S. troops into action. By acting under UN auspices, the United States could keep economic and psychological costs down. This, of course, was part of the concern for limiting the war. Public opinion was strongly opposed to any long-term and bloody intervention (Nathan and Oliver 1985, 121–27; M. Truman 1973, 508–16). Thus, part of the reason for acting under UN auspices was reducing the potential cost to the United States.

A second reason offered by POLI (MAC) for acting through the UN is the principle of collective security. Policymakers of this period, both from the MAC and the PAC paradigms, believed that the proper response to any international aggression which violates international law, is collective. This was one of the primary reasons for the existence of the UN and its Security Council. MAC develops this argument in full—though in MAC it applies only to friends. It would not be triggered if the target of aggression was communist.

The collective security argument was prevalent among the U.S. decision makers reacting to the North Korean invasion (Paige 1968; M. Truman 1973, 498–510). One of the earliest communications from General MacArthur in Japan (Commander of U.S. Pacific Forces) on the morning of 25 June set the tone by calling the North Korean attack an "undisguised act of war *subject to United Nations censure*" (M. Truman 1973, 496, my emphasis). Indeed, Secretary of State Dean Acheson moved to bring the case before the UN Security Council even before notifying President Truman, who was in Missouri on family business (Nathan and Oliver 1985, 120).

Similar sets of arguments can be made about the other MAC recommendations found in Table 3. For example, the strong inference "Communicate commitment to protect target" follows from the belief "Target is friend" and from the domino theory conclusion "Target is in danger of falling communist." Whenever friends are in danger of toppling, according to strong elements of MAC, the commitment to protect them should be expressed publicly.

Although the confidence POLI has in the pragmatic anticommunist paradigm (PAC) is only slightly more than half as great as the confidence it has in MAC,

PAC does make important contributions to the policy debate. The PAC interpretation of the event is very similar to the MAC interpretation. North and South Korea have the same basic characteristics, but PAC attaches different CFs. For example, PAC is less confident that North and South Korea are strategically peripheral. Given this interpretation of the event, POLI now uses the PAC rule base to produce another, and competing, set of policy recommendations (see Table 3).

One line of argument supports the MAC view that action should be taken in the UN. This reasoning parallels the collective security argument just discussed. It is found in the rule,

IF Actor physically threatens target
AND Actor is not US
THEN Policy: raise problem in UN Security Council (.44).[13]

According to this rule, whenever there is international aggression that the United States did not initiate, the issue should be taken to the UN Security Council and dealt with as a collective security problem.

The third major decision made by U.S. leaders in response to the North Korean attack was to limit the U.S. intervention. The MAC paradigm provides a good "replication" of the first two real world decisions—to intervene and to act through the UN. But the only line of reasoning MAC offers relevant to this third decision is more directly applicable to the decision to act through the UN. The following display presents two inference chains from PAC for this third broad decision:

Chain 1

IF Goal: Contain Soviet communism in region
THEN Goal: Promote Asian nationalism (.44)

IF Goal: Promote Asian nationalism
THEN Policy: Avoid direct U.S. military involvement in Asia (.44)
AND Policy: Consult with Asian leaders before taking action (.44)
AND Policy: Sponsor conferences to gather input from Asian leaders (.44)

Chain 2

IF Actor physically threatens target
AND Borders of actor or target is USSR
AND Goal: Avoid World War III
THEN Goal: Limit U.S. military involvement (.49)
AND Goal: Avoid taking action that threatens Soviet security (.4)

IF Goal: Limit U.S. military involvement
THEN Policy: Restrict direct use of U.S. ground troops (.34)
AND Policy: Consider minimal use of air power first (.21)

IF Goal: Avoid taking action that threatens Soviet security
THEN Policy: Apprise Soviets of limited U.S. objectives (.3)
AND Policy: Do not directly attack Soviet territory (.4)
AND Policy: Do not approach Soviet territory in military action (.24)

PAC argues in Chain 1 that one of the best ways to contain Soviet communism in Asia is to strengthen the indigenous support for Asian leaders. This general goal of containment is one of the paradigm goals for PAC that applies in any situation. PAC adopts several subgoals designed to achieve containment, including the goal "Promote Asian nationalism," which in turn causes the last rule to fire, producing three policy recommendations to limit direct U.S. involvement. These recommendations exemplify the belief that Asian nationalism as an anticommunist force will strengthen if allowed independence. This line of reasoning also recognizes that Asian leaders are an important source of information and understanding about events in Asia.

Several congressional leaders used these arguments as part of their attack on the Truman administration's quick decision to intervene. In the early days of the war, the United States did not consult Asian allies about the proper response to the invasion. This failure to consider Asian sensibilities and expertise was severely criticized in the United States (and abroad) along the lines argued in PAC (LaFeber 1972, 100–121).

Chain 2 represents a different line of argument for limited intervention. Whenever international aggression occurs in the geographic vicinity of the USSR (a similar chain fires for China), PAC becomes very concerned about avoiding a direct confrontation. Special care must be taken not to provoke security concerns for either China or the Soviet Union in these cases.

The specific policy recommendations from Chain 2 were clearly followed by the United States in the first few days after the North Korean attack. A chronology of the decision process presented by Snyder and Paige (1962) demonstrates the accuracy of POLI's reasoning. It includes the choices on June 26 "to commit American air and naval units in direct combat support" of the South Koreans and "not to use ground forces in direct combat at this time"; on June 28 "to extend American air and naval action north of the 38th parallel"; and on June 29 "to restrict use of ground troops to protection of supply lines and evacuation of citizens," "to restrict American air and naval action to purely military targets north of the 38th parallel," and "to order air and naval units to stay clear of the Soviet and Chinese borders" (pp. 237–38). United States leaders also explicitly informed Moscow on June 27 that American objectives in Korea were limited (LaFeber 1972, 99). In addition, the arguments used in the actual decision making system to support these policies were precisely the ones used

by PAC (outlined in the text and in the display) to infer them (Paige 1968).

Because POLI has little confidence in the isolationist paradigm, its policy recommendations have significantly lower CFs. The ISO dominance of .125 is assigned to all of its inferences to reflect this skepticism. I will trace two lines of reasoning from ISO, one resulting in the recommendation to "remain neutral" and the other in the recommendation to avoid involvement in "internal affairs of Asian nations."

Unlike MAC, the isolationist paradigm does not invest added importance in militarily active nations. The Koreas were strategically peripheral before the attack, and they remain so after the attack. So the first chain consists of the single rule,

IF Strategic location of actor/target is peripheral.
THEN Policy: remain neutral about event (.7).

The second inference chain responds to the general ISO goal, "Never violate sovereignty." To achieve this goal, ISO recommends the policy, "Do not get involved in the internal affairs of Asian nations."

In fact, only a fraction of American elites objected to the U.S. intervention, and only part of this dissent was motivated by isolationist arguments like these. Senator Taft, one outspoken isolationist critic in 1950, accused the Truman Administration of overstepping its authority by intervening without consulting Congress. In some of his criticisms, Taft used both of the arguments just sketched for ISO (Foster 1983, 103–4). At other times, however, Taft seemed to support the intervention, merely disagreeing with Truman's failure to consult Congress (LaFeber 1972, 99).

Though I certainly cannot claim to have proven POLI's process validity, I do believe the reasonableness, or face validity, of the model to be well supported by this detailed comparison of POLI's reasoning with that of the U.S. policy-making system. In addition, two other case studies generated by POLI (attempts to settle Japan's disputes with South Korea in 1953 and the 1958 Taiwan Straits crisis) have been successfully compared to historical descriptions (Taber 1991).

Sensitivity Analysis. Like many computational models, POLI is very complex, containing three sets of production rules and three large frame systems for declarative information in each time period. An important question in models of this sort is, How sensitive performance is to changes in the knowledge base? (Benfer, Brent, and Furbee 1991). Do the model's reasoning and outcomes change with minor modifications of CFs or when we eliminate some of the rules? A reasonable model should eventually degenerate with the elimination of model components (Whicker and Sigelman 1991); but it should also be robust in the face of minor changes of features of the model about which we are relatively unsure. In other words, we want a model that is sensitive to modifications of central components but less sensitive to changes of peripheral components.

I will test sensitivity in POLI on three levels. First, the model should clearly be sensitive to substantial changes in paradigm dominance. If the relative dominance of the MAC and ISO paradigms flipped in 1950, I would expect a somewhat different response to the Korean invasion than the one we saw. In fact, I do find that systematic variations of the relative dominance of the three paradigms affect model performance. (It would be rather disingenuous of me to applaud this result, however, since it clearly follows from the model structure as described.)

Second, I identified the elements of belief that appear *never* to be used in a chain of reasoning. I ran the 161 events in the test data set through POLI, keeping a counter for the number of times each element of belief was used. Typically, large rule-based systems like POLI contain a significant amount of redundant information. So I was surprised to find that POLI was really quite lean. Only 108 of the 1,467 rules were never active in this test (though a somewhat larger number were rarely active). Note that in addition to estimating the amount of chaff in POLI, I can also use this sensitivity test to establish rigorously the core and periphery of the paradigm. Not surprisingly, there was a strong correspondence between a rule's activity counter and its CF. All 108 inactive rules had CFs too small ever to lead to a highly recommended policy.

Third, the model should be sensitive to major changes in the relative CFs of core and peripheral beliefs in a paradigm; but it should not be sensitive to minor changes in these CFs, especially when the relatively crude empirical basis of these CFs is considered. I checked this sensitivity by systematically varying the CFs in the MAC paradigm while monitoring the policy recommendation set. Because this procedure is computationally very intensive, I have not repeated it for the other two paradigms. The broad results for MAC should generalize, since the three paradigms were produced in the same way.

Each rule in MAC was tested by varying its CF while all other elements were held constant at their empirically derived confidence levels. Eight events, one of each type from Table 1, were run through the paradigm for each set of CF values. The purpose of this sensitivity test is to estimate the amount of change in CFs needed to alter the set of recommended policies. Three broad results emerged: (1) averaging across all rules, a fairly substantial change is needed in a single rule's CF to alter the policies in the recommendation set ($\pm.26$);[14] (2) the recommendation set is much more sensitive to core beliefs than to peripheral beliefs; and (3) the behavior of the model changes *completely* when the rule CFs are inverted (i.e., subtracted from 1.0). These results are a very nice fit with the general desiderata for sensitivity in a model like POLI. As we would wish, POLI is sensitive to major changes in the CFs without being highly sensitive to minor changes. In addition, the distinction between core and peripheral beliefs has a meaningful impact on output.

POLI on the New Frontier of Computational Modeling

The detail with which POLI is able to replicate traditional historical case studies of U.S. foreign policy decision making is a powerful argument for its validity as a descriptive model of the decision-making system. While maintaining analytic focus in the sense that all behavior within the model is fully understood, POLI achieves a very high level of descriptive realism. In particular, POLI represents foreign policy making as a complete process rooted in prior beliefs and controversy.

Perhaps the most important contribution of the Policy Arguer project is that it demonstrates the utility of the computational approach for foreign policy analysis. The computational modeling techniques described in this work (along with many others that were not appropriate for POLI) offer tremendous opportunities for political science analysts, who must frequently deal with large complex systems based on either natural language communication or symbolic processes generally. Computational modeling techniques allow the information-processing black box to be opened and modeled in great detail and in an intuitive way. Natural language systems resonate with our common notions of how the black box really works. In this way, formal analyses can approach the descriptive complexity of traditional analyses.

In addition, computational modeling provides a platform for controlled experimentation. To the extent that a model like POLI is a reasonable descriptive representation, it can be used to simulate the behavior of the real system. This allows a degree of manipulation and control impossible with real political systems. In particular, counterfactual simulation using computational models promises to become a useful mode of inquiry in foreign policy analysis (Taber 1991; Thorsen and Sylvan 1982). Sometimes the things that did not happen or the decisions that were not made become very important in understanding a historical case.

Finally, computational modeling techniques are now readily available to a large number of users at fairly low cost. One does not have to be an experienced programmer or have a mountain of equipment to use these techniques. (POLI I, for example, was developed on an IBM-compatible personal computer and runs easily within 640k, using less than 1.5 megabytes of disk storage space.)

The generality of the techniques used in POLI allows them to be widely used by nonprogrammers.[15] Perhaps the success of POLI and other recent computational models will inspire other political scientists to avail themselves of this new method.

Notes

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1. The operational code approach has commonly been used to study particular leaders and how their belief systems influence their decision making. Ramsey MacDonald, Getulio Vargas, MacKenzie King, Dean Acheson, John Foster Dulles, Dean Rusk, Arthur Vandenburg, Lyndon B. Johnson, Mao Tse-tung, Liu Chao-chi, Willy Brandt, Kurt Schumacher, Lester Pearson, Pierre Trudeau, J. William Fulbright, and Henry Kissinger have all been subjects of such analyses (Holsti 1977).

2. Francis Crick recently wrote that "elegance and a deep simplicity . . . are useful guides in physics, but in biology such intellectual tools can be very misleading (1988, 6). The same might be said for many problems in the social sciences.

3. To avoid "the AI hype" and controversies over the meaning of *intelligence*, most practitioners prefer the label *computational modeling* to the better known *artificial intelligence*. For a general overview of computational modeling in international relations, including a discussion of this debate see Schrodt 1991. The work of Anderson, Minsky, Newell, Simon, and others in cognitive science will not be reviewed here (though their influence will be readily apparent to all familiar with their work). Several excellent reviews of the cognitive science literature exist, including Collins and Smith 1988, Sharples et al. 1989, and Shapiro 1990.

4. Successful expert systems exist for many domains, including geological exploration and medical diagnosis. See Benfer, Brent, and Furbee 1991 for a discussion of expert systems in the social sciences.

5. POLI was programmed in C on a Dell System 310 personal computer and compiled using Microsoft C 5.1.

6. In other words, POLI takes a situation as input and reasons forward to conclusions. A backward-chaining engine takes conclusions as input and reasons back to the conditions that may have caused the conclusions. I discuss fuzzy inferencing below.

7. Nathan Leites (1951) took a similar approach when he outlined the "operational code" of Bolshevik leaders. This shared belief system was a crucial element in explaining Soviet behavior. See also Majeski 1987, Boynton 1987 and 1991, and Hayward Alker's (1984, 1988) work on historical argumentation in which he formally analyzes the logics used in classical and contemporary political debates.

8. These paradigms are essentially equivalent to a three-part typology of "public attitudes" toward U.S. foreign policy identified by Bardes and Oldendick (1978), Holsti (1979), and Modigliani (1972): hawkish internationalism, dovish internationalism, and isolationism.

9. Actually, despite my strong endorsement of fuzzy inferencing (based on *process validity* concerns) it makes little *practical* difference which approach is taken. To satisfy my curiousity, I programmed POLI to use probabilistic and fuzzy inferencing in parallel and I compared the results (Taber 1991). The differences were minor. In general, fuzzy inferencing produced *slightly* more complex choices for POLI because the policy recommendations were less distinguishable—there were more ties. I report the fuzzy results here because I am convinced that it is a more appropriate representation of the real inferencing process.

10. To identify POLI's highly recommended policies (those with the top five CFs) I used the deterministic selection mechanism described above. POLI identified the five policies (more if there were ties for fifth place) with the highest CFs in the recommendation set.

11. On the other hand, since belief systems are the *cause* of policy output in this analysis, the use of belief systems from an earlier period may not be appropriate in predicting output. In essence, I am now testing the degree of change in belief systems (i.e., how well belief systems from an earlier period predict policies in a later period). It's not clear that we should expect much agreement.

12. These categories are similar to those developed in the

WEIS (Fitzsimmons et al. 1969) and COPDAB (Azar 1972) events data sets.

13. Note that this CF is PAC's confidence, not POLI's.

14. This figure represents the average amount of change in a single rule's CF that was needed to change the set of highly recommended policies *for at least one* of the eight event types.

15. Many of the inferencing techniques are general across problems. Inference engines and user interfaces (called shells) are readily available for the user who does not need special inferencing features. These shells are empty of content. They guide the user through the development of knowledge bases for the problem of interest and through the use of the knowledge bases once developed (Carrico, Girard, and Jones 1989; Martin and Oxman 1988). POLI's inference engine, for example, is a completely general forward-chaining engine. It could be used with any set of IF→THEN rules for many different research problems. The new user interface and inference engine for POLI will eventually be available as a stand-alone shell, which will be superior to commercially available shells for many analytic purposes. In particular, its inference engine employs fuzzy reasoning in a more sophisticated way than commercially available shells, which were designed for decision support, rather than research.

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PUBLIC OPINION AND THE COMMON DEFENSE: WHO GOVERNS MILITARY SPENDING IN THE UNITED STATES?

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We measure the extent to which military spending policy reflects public opinion, while controlling for other reasonable influences on policy. We use survey data as an indicator of aggregate public opinion on military spending and find evidence that changes in public opinion consistently exert an effect on changes in military spending. The influence of public opinion is less important than either Soviet military spending or the gap between U.S. and Soviet military spending and more important than the deficit and the balance of Soviet conflict/cooperation with the United States. We also examine the hypothesis that public opinion does not influence the government but that the government systematically manipulates public opinion. We find no evidence to support this hypothesis.

"Liberal" democracy, or polyarchy, is typically defined procedurally, in terms of the institutions that guarantee freedom of organization and expression, a wide franchise, and competitive and fair elections. These conditions are thought to make government policies depend on the vote, as well as on other expressions of the preferences of the public (Dahl 1971, 1989).

Some scholars have found evidence that democratic institutions in the United States may be decreasingly compatible with democratic norms. Mayhew (1974), for example, points out that congressional races are increasingly dominated by incumbents. Burnham (1982), among others, maps the decline in voter turnout; and Ginsberg (1990) argues that the influence of electoral mechanisms has declined.

Other scholars, however, have found that policy does reflect public opinion, evidence of the representativeness of American government. Ostrom and Marra (1986) find that policymakers seem to consider public support or opposition to increases in military spending (where *public opinion* is defined as a plurality favoring either an increase or decrease in military spending). Page and Shapiro (1983, 1992) find that change in public opinion tends to precede change in policy. They conclude that in a wide variety of issue areas "opinion changes are important causes of policy change" (Page and Shapiro 1983, 189). Other scholars including one of the present authors, Russett, have found similar results (e.g., Jacobson 1985; Risse-Kappen 1991; Russett 1990, chap. 4; Russett and Graham 1989; for a contrary view of West Germany, see Brooks 1990). On balance, this evidence supports the normative expectation, derived from democratic theory, that governments should respond to public opinion. Yet the findings of scholars who trace changes in the efficacy of representative institutions justify further research.

We explore the influence of public opinion on military spending, an issue that focuses attention on the question of whose will should predominate. Defense spending reflects a policy matter—national security—of supreme importance to all citizens, who should therefore decide how best to achieve it. Yet it also represents a realm of policy traditionally regarded as requiring some degree of expert judgment and access to privileged information. For many, it is a realm best left to those institutionally responsible for the common defense (Almond 1950; Lippmann 1955). If there were an external threat to national security that was not perceived by the public, those who govern could well be expected to act contrary to the common will. Yet policymakers might also act contrary to the common will in the absence of such a danger.

The issue of whose will should predominate is one of at least four questions that are important to democratic theory. The first question is whether the majority should rule.[1] Even in an area like defense spending, which is undeniably an issue of salience to the public, the mass public can have little influence under the strict standards of majoritarian democracy. After all, there have been absolute majorities (rather than mere pluralities) supporting changes in defense spending in only two of the past 25 years (1980 and 1981). Majoritarian democracy asserts that in the absence of a majority, the status quo should be maintained. But this would imply that over most of the past 25 years public opinion should have no effect on policy. Neither control of policy by policymakers nor control by groups with some particular interest in the size of the defense budget is a solution acceptable by the norms of democratic governance (McConnell 1966; Schattschneider 1960). We argue that due to the incremental nature of policymaking in military spending, *changes* in public opinion should influence policy.

The second question concerns the needs created by representative democracy—how to meet the need for stability and predictability of policy despite momentary shifts in popular sentiment, how to protect minority rights from majority tyranny, and how to give special weight to portions of the population who may be better informed or more intensely interested

in a particular policy.[2] These needs may modify the appropriateness of reflecting majority opinion in particular instances. Few observers, however, would deny that to deserve the label *democratic*, a state's institutions should, over time, produce policy outputs substantially consistent with some indication of public opinion. If public opinion influences decision makers in competition with considerations of special importance to the decision makers, then we must ask what will be the importance of public opinion relative to these other factors? To answer this question we estimate the magnitude of the influence of public opinion on defense spending and compare this with the magnitude of autonomous government influence.

Our third question concerns the degree to which public opinion *consistently* affects policy over time. Here, we are interested in systematic effects. Democratic theory's requirements are not satisfied by merely occasional compliance with the common will.

Finally, we ask whether consistency between public opinion and policy arises because public opinion influences government or because the government influences public opinion. Sometimes leaders adopt policies that are not initially preferred by the public, and approval develops subsequently. Scholars generally agree, however, that policy adoption should not consistently precede the emergence of popular approval in a democracy. Questions have been raised by a number of scholars (e.g., Edelman 1964; Lindblom 1977; Miliband 1969; Weissberg 1976). More recently, Ginsberg has asserted that by "using polling, media and public relations techniques, modern states have learned a good deal about the manipulation and management of public opinion" (1986, 224).[3] To answer this question, we empirically test how far public opinion has consistently *responded to* changes in U.S. military spending.

AN ALTERNATIVE TO THE MAJORITY RULE

In order to assess the relative influence of public opinion, and in order to model the decision-making process, we must define how change in a policy area (military spending) occurs. For budgetary issues like military spending, change must occur in an incremental manner. We argue that for such decisions the definition of individual interests in democratic theory can remain unchanged but that the majority rule must be replaced. In general, we suggest that with each budget cycle, the government should be expected to support incremental changes in the defense budget that reflect *changes* in the level of public support or opposition to increases in military spending.

Policy decisions regarding military spending and other budgetary items are made in increments (Wildavsky 1964). Making only incremental changes in the budget eliminated certain inefficiencies in the cost of prosecuting the Cold War. Annual budgets fund multiyear projects in which substantial investments are at stake; by changing military spending at the margin, many of the costs of stopping and restarting projects, or of abandoning investments only to reinvest, were avoided.

What implications do incremental decisions have for democratic theory? The incremental politics of the military-budget-making process pose no special problem for the definition of individual interests. We follow Dahl (1989), who uses a twofold argument to justify a particular definition of individual interests that gives a prominent place to the values of inclusiveness and equality. First, "the good or interests of each person must be given equal consideration." Dahl argues that "during a process of collective decision-making, the interests of every person who is subject to the decision must (within the limits of feasibility) be accurately interpreted and made known" (pp. 86–87). Second, Dahl defends a "presumption of personal autonomy" implying that "each adult person whose interests are involved in the outcome ought to have the right to specify what those interests are" (p. 99).

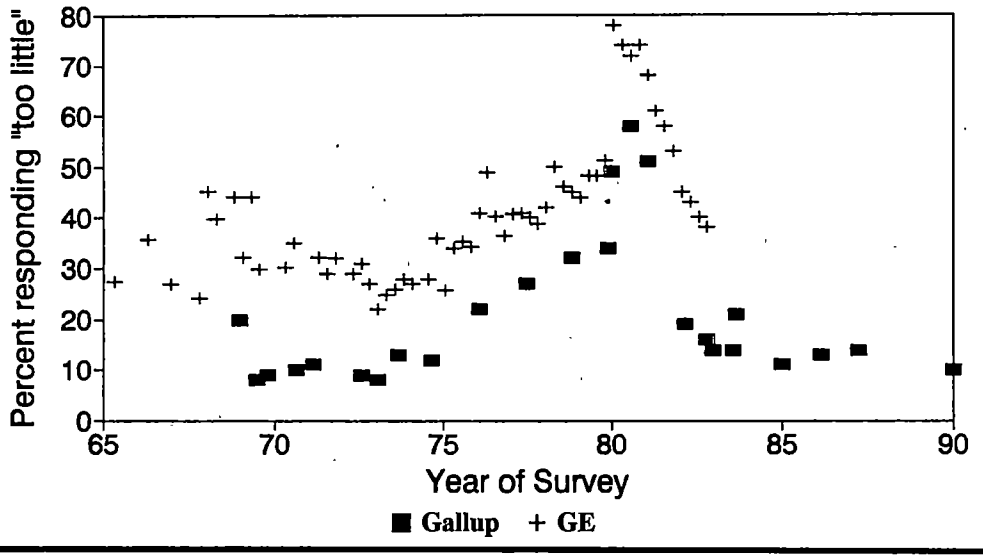
Random sample surveys satisfy both of Dahl's criteria and so can be thought to reflect the public interest accurately. In such a survey, each individual has an equal probability of voicing an opinion, and each individual's opinion is usually weighted equally.[4]

If incremental politics pose no problem for the definition of individual interests, they do pose some problems for the choice of a decision rule. When analyzing the effect of public opinion on policy in a representative democracy, democratic theorists have long used the principle of majority rule in order to justify the United States' democratic institutions of governance. Certain kinds of policies lend themselves to the majority rule—requiring a vote for enactment and a vote for repeal. The changes of incremental politics, in contrast, are small relative to the absolute size of the budget and do not constrain future action. It is reasonable to expect that in many years so many individuals whose interests are at stake might abstain from following the policy debate that no absolute majority would favor change in a particular direction.[5]

If the regular emergence of majorities is unlikely, then we must abandon the requirement of majority rule and search for a weaker rule that is consistent with the norms of democratic governance. In the United States, we often neither have nor need majority support in policy areas where decisions are characterized by incremental change. When there is no majority, we still expect the government to pay attention to changes in public opinion. When support for further increases in military spending drops, the government should respect this shifting preference in its incremental decisions by not increasing defense spending (barring temporary considerations such as a newly identified threat). Conversely, we might expect government to spend more on the military when support for increases in military spending rises. Consistent failure to do so would be to ignore the pref-

FIGURE 1

Gallup and General Electric Opinions on Change in Military Spending Compared



erences of the public most of the time. In policy areas where decisions recur periodically and change occurs incrementally, our rule satisfies the requirements of democracy better than the rule of the majority. Policymakers should be interested not only in whether a majority exists but also in the direction that public opinion is currently moving.

In short, testing governmental responsiveness in a policy area where change is incremental would involve examining change in public sentiment and change in government policy. We shall measure the importance of public opinion relative to other factors that might influence policy makers in the area of military spending, looking for a systematic influence of public opinion on policy. We will then see whether changes in public preferences for increased or decreased military spending precede governmental decisions to increase or decrease military spending. We now turn to the specification and estimation of our model.

MODELING MILITARY SPENDING

Changes in military spending are expected to be influenced by changes in public support for, and opposition to, changes in military spending. Before specifying our model, however, we must consider some issues of measurement.

We are concerned with actual expenditures rather than budgetary projections. Jacobson (1985) measures overall U.S. military spending with *total obligational authority* (TOA). We prefer total obligational authority to *budget authority* (BA) because spending is not completely determined by the final budget approved by Congress and the president. For example, some budget authority lapses before obligations are incurred, making TOA smaller than BA. An additional advantage of TOA over other indicators for military spending is that the military constructs a deflator specifically for TOA, and this deflator more accurately reflects changes in costs than does a deflator intended for use with the entire budget.[6]

Measuring public opinion also requires some discussion. One appropriate survey question used by several survey houses asks whether military spending should increase or decrease. With the problem conceptualized in terms of change, those with no current interest could either fail to express an opinion or simply support the status quo.

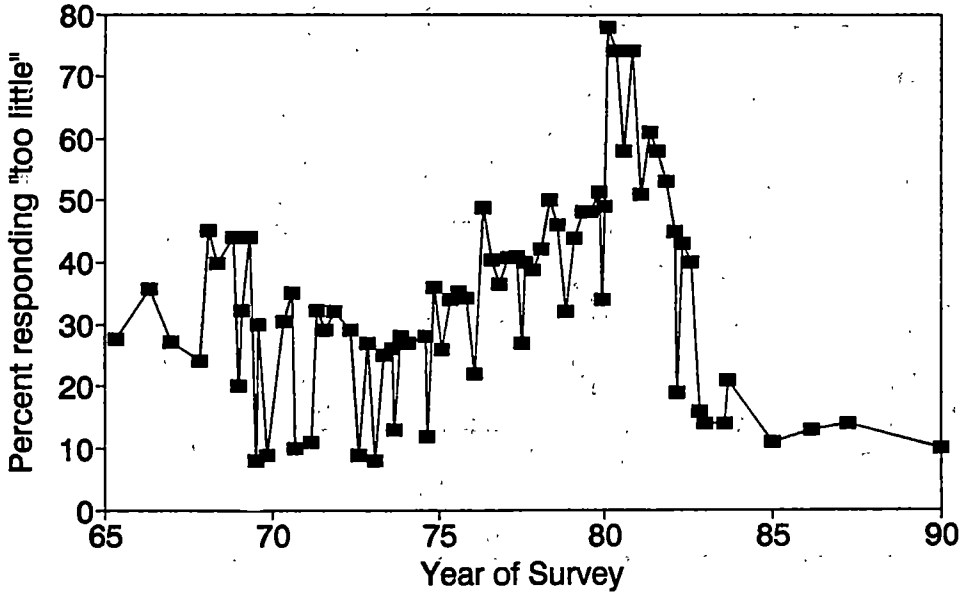
Public opinion is not measured at regular intervals, nor do any of the survey houses which measure support for changes in military spending produce enough estimates for us to be able to use a single survey house's estimates. We use a dataset compiled by one of the present authors and Thomas Graham (Graham and Hartley 1990). The dataset goes back to 1965, further than other available measures of public opinion; and it is carried forward to 1990. It allows us to avoid the methodological problems caused by a limited number of degrees of freedom and to check for the robustness of our results. Also, because we have data from a variety of sources (six different survey houses), we are able to ascertain the sensitivity of our results to the effect of house and question bias.

Using our comprehensive data set, we pooled the different public opinion series over a relatively long time series. Figure 1 illustrates that a single polling organization's measurements tend to move relatively smoothly over time. In contrast, Figure 2 shows that pooling the observations from different survey houses leads to great variance.

To ensure that survey house bias and variance do not affect our results, we construct three indicators of public opinion. The first is created by averaging

FIGURE 2

Gallup and General Electric Opinions on Change in Military Spending Combined



pooled public opinion measurements for all six survey organizations over annual periods that correspond with the budgetary cycle. Gallup, Roper and National Opinion Research Center tend to cluster together with less variance, and so were used to generate the second indicator. A third, less-dovish, indicator was obtained using data from General Electric; Columbia Broadcasting System; and Yankelovich, Skelly, and White.[7] Figures 3 and 4 illustrate how each group of three survey houses produces similar trends. Regressions were run using the indices derived from all three of these pooled indicators. Some results are not reported here, because the use of different groupings did not affect our findings. The coefficients from each of the three regressions were statistically significant and their slopes led to the same substantive conclusions. The series we use to generate the reported estimates is from all six survey houses averaged annually and can be found in Table 1.[8]

So far we have only discussed our dependent variable, U.S. military spending, and one independent variable, public opinion. Several other important factors are needed to specify the model. Policy might also be expected to reflect the concerns of officials and specialists faced with the conflicting

FIGURE 3

Hawkish Bias: General Electric; CBS; and Yankelovich, Skelly, and White Opinions on Change in Military Spending

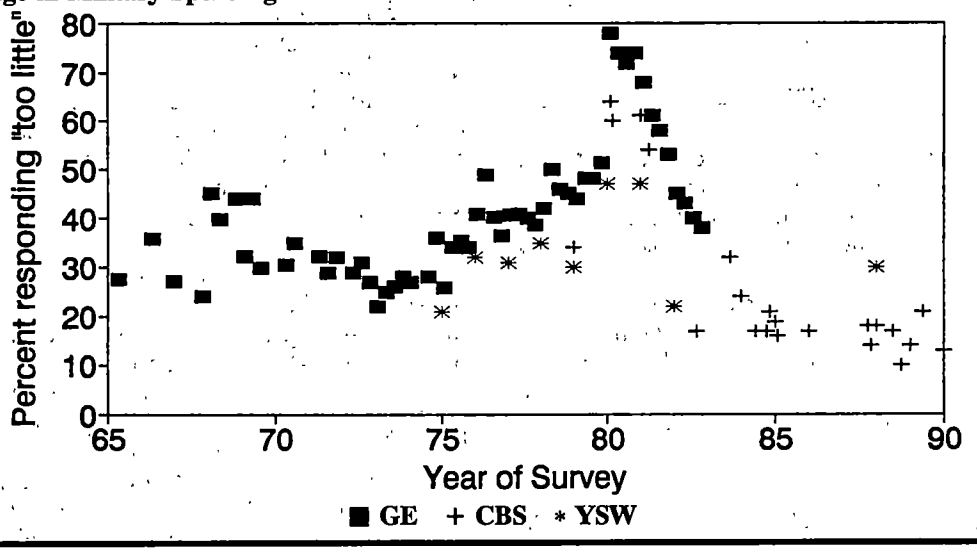
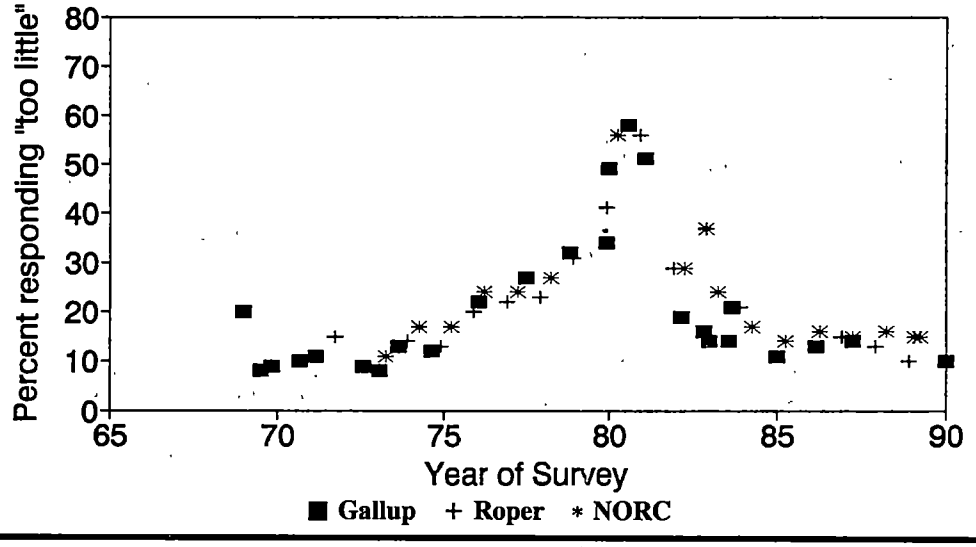


FIGURE 4

Dovish Bias: Gallup; Roper; Yankelovich, Skelly, and White Opinions on Change in Military Spending



imperatives of national security and domestic policy. A model explaining change in total military spending could draw on a variety of earlier models. (See Anderton 1989; Etcheson 1989; McGinnis 1991; Russett 1983 for reviews.) A reasonable model, that is, one in which omitted substantively significant variables are uncorrelated with our included independent variables, would include three other factors: changes in Soviet military spending, changes in the difference between U.S. and Soviet military spending, and changes in the U.S. federal budget deficit. Other variables might be added, such as bureaucratic inertia or the importance of stockpiles of armaments. However, given our goal of estimating the relative influence of the mass public on policy, these variables, plus a term for Soviet conflictual or cooperative behavior toward the U.S., should suffice.[9]

TABLE 1

Averaged Values of Survey Responses Concerning Military Spending, 1965–1990 (%)

YEAR	"TOO LITTLE"	"TOO MUCH"
1965	28	16
1966	36	13
1967	27	22
1968	36	14
1969	35	24
1970	19	36
1971	22	37
1972	26	28
1973	20	32
1974	21	30
1975	23	30
1976	32	21
1977	33	18
1978	37	16
1979	40	16
1980	58	11
1981	60	10
1982	35	25
1983	22	36
1984	21	30
1985	17	31
1986	15	37
1987	16	39
1988	17	33
1989	14	34
1990	12	43

Sources: See Appendix A.

Our first policy-related variable, Soviet military spending, reflects the assumption that if military spending could have been affected by public opinion, it could also have been spurred by an arms race. Arms race models have emphasized international causes—adversary's military spending or military strength, international tensions, and the like (Richardson 1960; Ward 1984). Most arms race models assume that military spending is an important measure of a nation's future military capabilities and therefore also intentions and commitment. Marra and Ostrom (n.d.) and Ward and Davis (1990) argue that the measure of aggregate Soviet military spending reflecting the information most likely used by U.S. policymakers *at the time their budgetary decisions were made* is generated from the "building block method" used by the Central Intelligence Agency, and published by the U.S. Arms Control and Disarmament Agency.[10] This method involves estimating the change in tangible assets of the Soviet military (e.g., tanks, aircraft) and then estimating what it would cost to duplicate this capability in the United States. Marra and Ostrom argue that the most relevant features of Soviet military spending for U.S. policymakers are the sign and size of annual changes. They also argue for the importance of the gap between U.S.

and Soviet military expenditures. We use the constant dollar estimate for U.S. and Soviet military spending because policymakers try to account for inflation. Inflation-adjusted budgetary outlays also reflect the real economic resources that would be required to produce, maintain, and use a given level of military resources and that are ultimately paid for by the public.[11]

If changes in U.S. military spending are the product of policymakers' national security considerations, they are also determined in part by the administration and Congress who must consider domestic political influences, the most important of which are fiscal constraints. Whereas the armed forces might be expected to support expenditures that increase military capability in response to the Soviet Union, the administration and Congress need to balance increased military expenditures with domestic priorities such as an increasing budget deficit (Huntington 1961, 125; Ostrom and Job 1986).[12]

We estimated two equations. Both of these equations are differenced, so the constant captures any linear trend in U.S. military spending. The first one includes the policy variables and an indicator of the extent to which public opinion feels the government is spending too little on defense:

change in total U.S. military spending = constant

+ change in Soviet military spending$_{t-2,\,t-3}$

+ change in (Soviet military spending

− U.S. military spending)$_{t-3}$

− change in deficit$_{t-1}$

+ change in percentage responding *too little*$_{t-1}$.

The second equation is the same as the first except that it reads *too much*, instead of *too little*. We expect a negative coefficient for this variable.

A note is in order regarding lag lengths. Marra and Ostrom (n.d.) and others have argued that a lag length of three years is required for measures that include Soviet military spending: one year because budgets are presented and debated in the year preceding the one they cover and two additional years to allow for the time required to turn evidence of Soviet military strength into Arms Control and Disarmament Agency (ACDA) estimates of Soviet military spending. Since information on Soviet military spending is not available in the first year after the Soviets make their budget, we do not include the earlier lag. A one-year lag for Soviet military spending would have no theoretically meaningful interpretation. However, we do include a lag length of just two years to test for the possibility that preliminary and confidential estimates are used in the budgetary process.

Note also that we did not include a lagged dependent variable, because we are dealing with differences in U.S. military spending. While last year's level might be expected to be a good predictor of this year's level, we would expect this year's changes to result from variables important to policymakers, such as the latest estimate of the Soviet threat and public opinion, not from last year's change.

TABLE 2

Explaining Changes in U.S. Military Spending, 1965–1990

INDEPENDENT VARIABLES	COEFF.	S.E.	*P*
Constant	−1.29	5.37	—
Change in			
Soviet military spending$_{t-2}$	.357	.257	.08
Soviet military spending$_{t-3}$	.503	.173	.01
Difference between Soviet and U.S. military spending$_{t-3}$	−.151	.173	.18
Deficit$_{t-1}$	−.064	.028	.02
"Too Little"$_{t-1}$	.261	.149	.05

Note: $\widehat{R}^2$ = .77; standard error for the equation = 5.44; degrees of freedom = 14. All monetary units are billions of constant (1982) dollars. P-values are for one-tailed t-tests.

FINDINGS

Table 2 shows our results.[13] The variable of interest here is the public opinion variable. We find evidence that increases in the percentage of the public that believes that the government is spending too little on the military result in increases in military spending ($b = .261, p < .05$). The statistical significance level for this independent variable is impressive, considering that the indicator is the average of values taken from different survey houses.[14] Substantively similar results hold if we use as our independent variable the percentage of the public responding that the government spends too much on military spending.

The greatest influence on change in U.S. military spending is change in Soviet military spending. Change in Soviet military spending as reported by the ACDA, lagged three years, is substantively and statistically significant ($b = .503, p < .04$). We find support for Ostrom and Marra's (1986) assertion that decision makers primarily use publicly available information regarding Soviet military spending. Change in Soviet military spending, lagged only two years, has a marginally statistically significant influence on change in U.S. military spending ($b = .357, p < .08$). Because these variables covary, it is likely that Soviet military spending is even more statistically significant than our results indicate.

We do not find statistically significant results for the gap between U.S. and Soviet spending ($p < .18$). It would seem that in the political debate the gap does not add much, but, rather, U.S. spending responds to Soviet spending. The sign of the coefficient for the deficit is in the expected direction, with increased

deficits exerting pressure against further increases in U.S. military spending ($b = -.064$, $p < .02$).

Two other variables that were not included in our model nevertheless merit special attention. Models of arms races and of Soviet–American relations suggest that military expenditures are affected by changes in the level of international political tensions, that is, by the behavior, as well as the expenditures, of the adversary. To account for the influence of international tension, we included the indicator of Soviet behavior toward the United States constructed by Joshua Goldstein from the World Event/Interaction Survey dataset aggregated to the annual level (Goldstein and Freeman 1990).[15] However, we found no significant effect.

We also might expect military spending to be greater during the Reagan years, all else being equal. Analysis (not reported in the table) using dummy variables for Reagan's first term, (1981–84), the years 1981–88, and the years 1981–90 did not produce stable and statistically significant coefficients. It appears that increases in military spending during those years are explained better by changes in the other variables, including public opinion.

Our results also suggest that the effect of public opinion on policy is consistent. In fact, regressions run with either earlier years or later years omitted from the sample resulted in the same conclusions.

In short, much of the influence on military spending stems from "objective" characteristics of the arms race, such as the change in Soviet military spending. Some of these influences on military spending other than public opinion can be considered normatively justifiable. One might argue with the wisdom of engaging in an arms race, but it would be difficult to refuse experts some autonomy to assess the external military threat and react to it. Other nondemocratic influences would have been more controversial; but there is no strong evidence that a significant part of the increase in military spending during the Reagan years was due to the particular perspectives of his administration, independent of other factors.

Even controlling for the effects of other factors that concern policymakers, we find that public opinion exerts an effect on military spending in a way that is substantively plausible as a cause. With other influences held constant, we find that if the percentage of public opinion favoring increases in military spending rises, then the level of military spending will increase. Conversely, if the percentage of public opinion opposing increases in military spending rises, then actual spending tends to come down. It is important to note that these effects were systematic over the time period that we studied, not occasional effects too erratic to result in a statistically significant regression coefficient.

More specifically, ceteris paribus, for every 1% change in support for (or opposition to) increased spending there is about a $.33 billion increase (or decrease) in defense spending. This is a small but not insignificant effect, especially considering that swings in public opinion were as large as 25% during the late 1970s and early 1980s (see Figure 1). Neither, however, does the public have an overwhelming impact on the nearly $300 billion military budget.

ASSESSING THE INFLUENCE OF GOVERNMENT ON PUBLIC OPINION

We shall test the hypothesis that consistency arises because the government influences public support for increases (or decreases) in military spending. When it changes the military budget, does the government create a change in public opinion supporting that action? To address this question partially, we use Granger's concept of causality to assess the directionality of influence. Granger causality—developed by Granger (1969) and used in vector autoregression (Freeman, Williams, and Lin 1989; Sims 1980)—is a method that relaxes assumptions about causal direction and lag length and so allows one to assess the directionality of influence. The technique involves regressing each variable on a reduced form equation that includes its own lagged values and the other variables' current and lagged values. We hypothesize that government does not influence public opinion. In the language of Granger causality, we hypothesize that changes in policy do not "Granger-cause" changes in public opinion, that is, that lagged values of public opinion would do just as well explaining current values of public opinion as would a model that also included lagged values of military spending.[16]

One potential criticism of our use of vector autoregression is discussed in Appendix B. It can be avoided by making the reasonable assumption that there is a first-order autoregressive process in U.S. military spending. The finding of autocorrelation in our regressions supports this assumption.

Our model includes three lags each of public opinion, U.S. military spending, and Soviet military spending. We tested the hypothesis that change in U.S. military spending Granger-causes subsequent change in public support for military spending in two ways—that $\pi_{12} = 0$ first for public support for increased, and then for decreased, military spending. The results were far too weak to be acceptable at any reasonable significance level (chi-squared [3] = 1.87, $p = .60$; chi-squared [3] = 2.66, $p = .44$).[17] The absence of significant results in either equation suggests that public opinion may in fact be exogenous to policy on military spending.[18]

CONCLUSIONS

We have assessed how far public opinion influences government policy in the realm of military spending. Despite other researchers' findings that institutions that maintain public control over government may be losing their efficacy, we found strong evidence consistent with the hypothesis that public opinion did in

fact influence government policy, although the exigencies of the arms race and the budget deficit were equally or more influential. The influence of public opinion was systematic over a period of years. One might regard this mixture of public and elite influence as acceptable, at least in an era when the Soviet Union was actively building up its military and a countervailing military buildup might be necessary. We also found some evidence supporting the contention that public opinion was not merely the result of overt government influence.

A great deal of research remains to be done in this area, however. For one thing, it is uncertain whether public opinion affects policy consistently in other areas of incremental politics, and it is uncertain to what degree public opinion will continue to influence military spending in the absence of a superpower arms race. Second, it is uncertain what influence the media has on the process. It is possible that network television (as documented in the Vanderbilt Archives) has an effect on public opinion but no direct influence on policy. Network media might thus be used as an instrument to specify a simultaneous equation model to test further the hypothesis that government influences the public (see Page, Shapiro, and Dempsey 1987). Third, arms race modelers might consider the effect of public opinion in models in which U.S. and Soviet budgets affect each other simultaneously and in which military strength is also a factor; that might provide a more accurate measure of the relative importance of public opinion and arms race coefficients. Fourth, the findings of Ostrom and Marra (1986) suggest the possibility that different branches of government are more responsive than others in this policy area. Analyzing each of these three questions would require several more years of data if results are to be subjected to sensitivity analysis. Fifth, it is uncertain what the structure of public opinion that underlies the changes in preferences on military spending is. Analyzing this would require panel data. Finally, all of these results are merely suggestive. To strengthen an assessment of causality, historical research is required, to show that policymakers actually sought out and used information regarding public opinion when making their decisions.

APPENDIX A: DATA SOURCES FOR PUBLIC OPINION INDEX

All of the public opinion data used in this analysis can be found in Graham and Hartley 1990. Question wording for the different survey houses is listed below.

Gallup, American Institute of Public Opinion, Gallup/*Newsweek*, Gallup/*Times Mirror*

Version 1. "There is much discussion as to the amount of money the government in Washington should spend for national defense and military purposes. How do you feel about this: Do you think we are spending too little, too much, or about the right amount?"

Version 2. "Do you think that we should expand our spending on national defense, keep it about the same, or cut back?"

The Roper Organization

"Turning now to the business of the country—we are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. Are we spending too much, too little, or about the right amount on . . . the military, armaments and defense?"

National Opinion Research Center

"We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money, too little, or about the right amount: . . . military, armaments, defense."

General Electric/Trendex

Version 1. "I would like to get your opinion on several areas of important expenditures, first on the part of the government. As I read each one, please tell me if you would like to see the government spend more, spend less, or spend about the same amount as they have been . . . on defense."

Version 2. "I would like to get your opinion on several areas of important government activities. As I read each one, please tell me if you would like to see the government do more, do less, or do about the same amount as they have been: . . . defense."

CBS, CBS/*New York Times*, *New York Times*

Version 1. "Should federal spending on military and defense programs be increased, decreased, or kept about the same?"

Version 2. "Do you think government/federal spending on military and defense programs should be increased, decreased, or kept about the same?"

Yankelovich, Skelly, and White/*Time*; Yankelovich, Clancey, Shulman/*Time*

"Do you feel that government spending should be increased, decreased, or kept where it is now in the following areas? . . . the military."

APPENDIX B: CRITICISM OF VECTOR AUTOREGRESSION

One criticism of vector autoregression is important to our example (Cooley and LeRoy 1985). Note that our model is

$$\begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} = \begin{bmatrix} 0 & \alpha_{12} & \alpha_{13} \\ \alpha_{21} & 0 & \alpha_{23} \\ \alpha_{31} & \alpha_{32} & 0 \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} A_{11}(L) & A_{12}(L) & A_{13}(L) \\ A_{21}(L) & A_{22}(L) & A_{23}(L) \\ A_{31}(L) & A_{32}(L) & A_{33}(L) \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} u_1 \\ u_2 \\ u_3 \end{bmatrix},$$

where X_1 = public opinion, X_2 = U.S. military spending, X_3 = Soviet military spending, and L is the lag operator, so that $A_{11}(L^1)X_1 = A_{11}X_{1\ t-1}$ and $A_{11}(L^2)X_1 = A_{11}X_{1\ t-2}$. The reduced form of this model is

$$\begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} = \begin{bmatrix} \pi_{11}(L) & \pi_{12}(L) & \pi_{13}(L) \\ \pi_{21}(L) & \pi_{22}(L) & \pi_{23}(L) \\ \pi_{31}(L) & \pi_{32}(L) & \pi_{33}(L) \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} v_1 \\ v_2 \\ v_3 \end{bmatrix}.$$

We can accept the null hypothesis of no Granger causality if the parameter π_{12} does not differ significantly from 0. Military spending (or the influence of government) is said not to Granger-cause public opinion if $\pi_{12} = 0$. However, Cooley and LeRoy (1985) point out that Granger causality is distinct from strict exogeneity. Cooley and LeRoy would argue, in this case, that public opinion can be considered to be causally prior to military spending if and only if public opinion is strictly exogenous and military spending is endogenous. In this case, public opinion is strictly exogenous with respect to U.S. military spending if $\alpha_{12} = A_{12}(L) = 0$. Note that the reduced form equation is

$$\begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} = \frac{1}{|\alpha|} \begin{bmatrix} 1-\alpha_{23}\alpha_{32} & \alpha_{12}+\alpha_{13}\alpha_{32} & \alpha_{12}\alpha_{23}+\alpha_{13} \\ \alpha_{21}+\alpha_{23}\alpha_{31} & 1-\alpha_{13}\alpha_{31} & \alpha_{23}+\alpha_{13}\alpha_{21} \\ \alpha_{21}\alpha_{32}+\alpha_{31} & \alpha_{32}+\alpha_{12}\alpha_{31} & 1-\alpha_{12}\alpha_{21} \end{bmatrix} \begin{bmatrix} A_{11}(L) & A_{12}(L) & A_{13}(L) \\ A_{21}(L) & A_{22}(L) & A_{23}(L) \\ A_{31}(L) & A_{31}(L) & A_{33}(L) \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} v_1 \\ v_2 \\ v_3 \end{bmatrix},$$

where $|\alpha| = 1 - \alpha_{23}\alpha_{32} - \alpha_{12}\alpha_{21} - \alpha_{12}\alpha_{23}\alpha_{31} - \alpha_{13}\alpha_{21}\alpha_{32} - \alpha_{13}\alpha_{31}$, so that

$$\pi_{12} = \frac{(1-\alpha_{23}\alpha_{32})A_{12}(L) + (\alpha_{12}+\alpha_{13}\alpha_{32})A_{22}(L) + (\alpha_{12}\alpha_{23}+\alpha_{13})A_{32}(L)}{1-\alpha_{23}\alpha_{32}-\alpha_{12}\alpha_{21}-\alpha_{12}\alpha_{23}\alpha_{31}-\alpha_{31}\alpha_{21}\alpha_{32}-\alpha_{13}\alpha_{31}}.$$

There are several circumstances in which public opinion would not be strictly exogenous and yet π_{12} would equal 0, leading to a false finding of exogeneity. One of these circumstances is relatively unlikely, namely, that in which $1 = \alpha_{23}\alpha_{32}$, $\alpha_{12} = -\alpha_{13}\alpha_{32}$, and $\alpha_{13} = -\alpha_{12}\alpha_{23}$. However, π_{12} could equal zero and yet α_{12} not equal 0 if $A_{22}(L) = A_{32}(L) = 0$ and either $A_{12}(L) = 0$ or $1 = \alpha_{23}\alpha_{32}$. We can avoid this pitfall by assuming that $A_{22}(L)$ does not equal 0, that is, by assuming that there is a first-order autoregressive process in the military spending budget. This is a theoretically justifiable assumption and is empirically supported by our finding resulting from the correction of first-order autocorrelation reported in the results.

Notes

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1. Few observers in modern states would make the requirement for a simple majority absolute. For one thing, it is well known that if policy issues are distributed on multiple dimensions, various shifting majorities may form even under procedures widely regarded as democratic (Arrow 1963; Caplin and Nalebuff 1988, 1991).

2. See the *Federalist Papers* 10.

3. Some evidence indicates that crises and other factors are used to raise the popularity of leaders when the economy is weak or elections are near (Brody 1984; Kernell 1978; Mueller 1973; Ostrom and Job 1986; Russett 1990, chap. 2; Russett and Barzilai n.d.).

4. Actual sampling procedures may, of course, deviate from the assumptions without generating a biased sample.

5. An additional reason why a majority might not emerge in the realm of military spending is that issues of national defense are complex. They entail balancing the risk of international conflict with domestic priorities. It is never clear "how much defense is enough" or whether additional military spending serves as a deterrent or as an incentive to arms race or war. There is never an unambiguous answer to the question of whether to increase or decrease military spending. Many individuals select the answer *don't know*.

6. Data on U.S. military spending come from U.S. Department of Defense 1990.

7. For a more detailed discussion of house bias in aggregate survey data, see Cowden and Hartley 1991. Question wording is included in Appendix A, but it is important to note that question wording does not seem to be the only determinant of bias in aggregate public opinion measurements.

8. Note that we cannot include each survey house individually because no single house measured public opinion on military spending at regular intervals over the period. Surveys dating further back are available, but it is impossible to assess their comparability because there is no overlap in the early 1960s. We could have extended the analysis into 1991, on the basis of public opinion in 1990. But we preferred to restrict the analysis to the Cold War era, during which we would expect the coefficients to be reasonably stable. In both November 1989 and January 1990, when the public was asked, "In view of recent events in the Soviet Union and Eastern Europe, do you think the cold war is over?, only 37% said *yes* and 54% said *no*. But by October 1990, following German unification, 49% said *yes* and 43% *no* (CBS/*New York Times* press releases).

9. Marra and Ostrom (n.d.; Ostrom and Marra 1986) have discussed the specification of a model explaining what policymakers consider in determining levels of U.S. military spending. Since we are interested in assessing the overall responsiveness of American government to public opinion, we do not separately model the different institutions that participate in the budgetary process. Two variables that they considered and we excluded are change in war costs and change in revenues. The Vietnam War was primarily deficit-financed; thus, war spending should be primarily correlated with the deficit. Excluding war costs might be expected to attenuate the deficit coefficient, because its sign will be

negative, the coefficient for the war cost variable would be positive, and the two independent variables should be positively correlated. Change in revenue is probably not correlated with any of the independent variables, so that its exclusion should not bias any of the estimates.

10. This estimate is reported annually in the U.S. Arms Control and Disarmament Agency's *World Military Expenditures and Arms Transfers*. The Defense Intelligence Agency estimate was also very probably used by policymakers, but it is not in the public domain. It is less likely that alternative estimates, produced by Stockholm International Peace Research Institute and International Institute of Strategic Studies, were used by U.S. policymakers.

11. For additional arguments regarding the consideration of military spending in constant dollars, see Kinsella 1990. Kinsella also argues persuasively that there is no evidence that military spending has a negative effect on the economy at the macroeconomic level. Thus, other measures of fatigue (e.g., Richardson 1960) are not applicable in this context.

12. Data for our measure of the deficit was taken from U.S. Office of Management and Budget (1991) for fiscal year 1989.

13. First-order autocorrelation was corrected for using the Cochrane–Orcutt method; rho was .408; higher-order autocorrelation was not found.

14. It is important to be cautious regarding the interpretation of the magnitude of the coefficients for the public opinion variables. Separating out hawkish from dovish survey houses (as in Figure 2) changes the magnitude of this variable's coefficient, though not its sign or statistical significance. However, since this variable is almost certainly measured with random error, all coefficients obtained are conservative estimates.

15. Goldstein and Freeman's data end in June 1990; we have used the first six months of 1990 as an estimate for the entire year. They caution that the coder changed in 1986 and 1987.

16. It is worth noting at this point that to be exact, our theoretical question is not, To what does extent actual military spending influence public opinion? Rather, we are using U.S. military spending as a proxy and asking, Do the officials who influence the complex process of policymaking in military spending also influence public opinion?

17. Since there is no statistically significant finding, we do not discuss the magnitudes of the response here. Full results of our analysis, including impulse response functions and innovation accounting, are available on request.

18. Eichenberg and Stoll (1991) suggest that, if anything, a change in military spending subsequently produces an opposite change in public preferences, as the public reacts against perceived policy extremes. They do not, however, use vector autoregression or a simultaneous equation model.

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PATTERNS AND SOURCES OF TICKET SPLITTING IN SUBPRESIDENTIAL VOTING

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The primary source of divided government in the United States is voters who split their ballots between the parties. Yet there has been little comprehensive examination of either patterns or sources of ticket splitting in recent years. Instead, divergent lines of research have emerged, emphasizing such things as voter partisanship, incumbency, and a "new" (young, well-educated, even partisan) kind of ticket splitter; and their focus has been too often restricted to the atypical president–Congress pair. We seek to unify these research traditions in a comprehensive model of split-ticket voting and to test this model across the partisan ballot in a typical election setting–here, the contests for five Ohio state-wide offices in 1990. The model incorporates partisan strength, candidate visibility, and the individual characteristics that distinguish the "new ticket splitters". The results support our partisan strength and candidate visibility explanations but provide little support for the emergence of a new type of ticket splitter.

Divided control of government, that uniquely American form of coalition government in which two different parties simultaneously control key governmental institutions, may be the most important political phenomenon of our time (Cox and Kernell 1991; Fiorina 1990; Jacobson 1991; Mayhew 1991; Sundquist 1988). In 20 of the last 24—and 32 of the last 48—years an American president has faced a Congress in which at least one of the houses was controlled by the opposition party. In the earlier years of American national politics, by contrast, divided control emerged from time to time, usually as a result of midterm election reversals for the presidential party; but it never dominated as it has in recent years.

Divided control is hardly exclusive to the banks of the Potomac. As V. O. Key (1956) noted 35 years ago, it has long been common in the state governments; and it has become increasingly widespread in recent years. The 1990 elections produced 32 states[1] with a governor of one party and a legislature in which the opposition party controlled at least one house—tying the historical high in 1988 but considerably higher than the average since 1967 or for the 1931–52 period (Fiorina 1990; Key 1956, 55).

The constitutions of almost all states provide a further possibility for divided government by separately electing different executive branch officials. In 1991, 35 states had top officeholders from both parties. This figure is well above the average over the last 20 years, and recent levels of executive splits greatly exceed those reported by Key for selected states during the 1900–1952 period (Key 1956, 197–216). When party control of the plural executive is combined with control of the legislature, only 12 states (most of them in the South) emerged from the 1990 elections as wholly dominated by a single party. Since 1967, in fact, there has never been a year when a majority of the states had single-party control of all executive offices and the state legislature. Divided control, in short, has become the *norm* for state governments just as it has for the government in Washington. Its causes, therefore, are a worthy topic of investigation.

Scholars have offered numerous structural reasons for divided government (Fiorina 1990; Jacobson 1991; Key 1956). Separate elections for different officials and the staggering of elections for various offices always have provided opportunities for divided party control in America that are not available in most democratic nations. The translation of popular votes into seats (or into electoral votes in the presidential contests), including the effects of gerrymandering, also may produce divergent results for different offices from the same vote totals. The office-block ballot form adopted in some of the states is conducive to divided government as well, with the arrangement of candidates by office (rather than party) and the correlative absence of a single-action party-vote choice making straight-party voting more difficult.

The principal roots of divided government are to be found in voter choice, however, not political structure. The direct election of multiple officials—a legacy of the powerful democratic tide in the Jacksonian era—*permitted* a voter to support different parties on the same ballot once the consolidated Australian ballot became widespread after the 1890s.[2] The office-block form, a legacy of the Progressive Era, virtually extends this permissiveness to license. But voters must be motivated to split a ticket in order for these possibilities to become realities.

More and more voters in recent decades have been seizing the opportunities to split their votes between the parties in the same elections. Based on reported votes for president and the U.S. House of Representatives in the American National Election Study surveys, split-ticket voting rose from 13% in 1952 to a historic high of 30% in 1972 and has settled into the

25%–28% range since then (Stanley and Niemi 1990, 132; Wattenberg 1986, 17–23). That ticket splitting has exhibited parallel gains in other office combinations can be inferred from the increasing frequency of split results. At the individual level, then, contemporary American voters are less motivated than before—probably than ever before—to vote only for the candidates from a single party.

Who are these ticket splitters? Past studies of ticket splitting have relied heavily upon vote divergence in a pair of contests—typically, the presidential and congressional races—to address this question. In an electoral system in which voters can vote for a multiplicity of offices, however, such a two-contest focus surely underestimates the extent of ticket splitting. Moreover, by restricting attention largely to the presidential and congressional contests, researchers have selected a pair that is quite unrepresentative of the situation individual voters typically face. The presidency is the most visible office in the land, and virtually all voters possess considerable information about both parties' candidates for it. Congressional contests, by contrast, typically involve a relatively visible incumbent running against an almost invisible challenger, if any. These visibility differences may accentuate ticket splitting. Furthermore, voters have very different expectations about candidates for the two offices—the one executive, the other legislative. This may translate into quite dissimilar criteria for choice (Fiorina 1990) and higher levels of ticket splitting for this pair than for offices that are similar in function. These considerations combine to make the presidential–congressional pair unique.

This study examines ticket splitting from a new and different vantage point. It focuses upon voter behavior across a five-office partisan ballot, rather than for a particular pair of offices. The case that we examine is the 1990 statewide election in Ohio. The five partisan races appearing on the ballot in that year provided a nice mix of the two characteristics—candidate visibility and the presence of an incumbent—that have proven important in earlier studies of ticket splitting. They also produced victories by candidates from each party and by both incumbents and challengers.[3]

Substantively, this focus moves us away from the current overdependence of research on ticket splitting that involves votes for president (an atypical office in terms of the information voters possess about its candidates) and toward the more commonplace low-information contests. It also examines this behavior in voting for offices at the state level. States are important policy actors in the American federal system, but they largely have been ignored in studies of voting behavior. To establish the generality of ticket splitting and its determinants requires a focus on the states.

There are good methodological reasons for this focus as well. First, we can examine electoral choices across contests for offices in the same electorate. Studies that mix the levels of contests and electorates, such as the familiar presidential–congressional comparisons, confound individual- and contest-specific factors because members of their samples face many different congressional contests. Second, by containing our comparisons within executive contests, we avoid the confounding effects of measuring ticket splitting across offices with vastly different functions and popular expectations. Consequently, results from a single state can greatly advance our understanding of ticket-splitting behavior and can also establish a baseline for replicative efforts in other states and other circumstances.

Voter behavior in the 1990 Ohio elections was tracked with a statewide panel study of Ohio voting-age adults conducted by telephone by professional interviewers in the Polimetrics Laboratory at Ohio State University. This analysis focuses on the 567 *voters* (out of 1,033 respondents) who completed both pre- and postelection interviews. Virtually all of the variables we analyze come from wave 3 of the study, but a few are drawn from wave 1.[4]

THE SOURCES OF TICKET SPLITTING

Split-ticket voting is hardly a new phenomenon in American electoral politics. Its trace markings in split election results have long intrigued scholars (Cummings 1966, chap. 2); and since the first individual-level data on voting appeared, considerable effort has been directed to explaining why voters would choose candidates from different parties. Nonetheless, for a variety of reasons, no comprehensive theory of ticket splitting has emerged that incorporates the different explanations provided for the phenomenon.

Previous explanations of ticket splitting have followed two divergent paths. Some scholars have emphasized the role of individual characteristics (particularly, partisan loyalties) in ticket splitting. Others, especially the most recent studies, have concentrated on the nature of the candidates, particularly the visibility edge enjoyed by incumbent officeholders. Our analysis draws upon each of these research traditions.

The earliest research on ticket splitting based on surveys emphasized, not surprisingly, the distinctive characteristics of the ticket splitters. In their seminal study of ticket splitting, using the 1952 and 1956 Michigan election studies,[5] Campbell and Miller (1957) attribute split-party votes for president/senator, president/representative, and state and local offices primarily to the absence of a strong party identification to motivate a straight-party vote, particularly in states that did not enable straight-ticket voting by a single action. Further analysis of multiple contests using the Michigan surveys through 1976 (by which time ticket splitting had grown to considerable proportions) corroborates the importance of strong party identification as an inhibitor of ticket splitting (Maddox and Nimmo 1981).

DeVries and Tarrance (1972) have dissented from the prevailing view that varying levels of partisanship are the dominant individual-level force behind split-

ticket voting. Their study of votes across several different offices based on national polls and state polls in Michigan and Texas concludes that the partisan-versus-independent distinction was overemphasized. Only about half of the ticket splitters they uncovered were self-identified independents, which left room for considerable ticket splitting among partisans. Two factors, however, weaken the force of their dissent. First, by ignoring the differences in intensity of partisanship among the large majority of the electorate who have party loyalties, they have understated the influence of partisanship on ticket splitting. Second, partisanship still emerges from their study as the most important correlate of ticket splitting.

The foregoing studies identified other voter characteristics that should be considered in any comprehensive explanation of ticket-splitting behavior. The lack of consistency in the correlates of these characteristics across studies suggests, however, that their exact role is questionable. Campbell and Miller (1957) found ticket splitting to be more widespread among men, the less educated, urban dwellers, and southerners. DeVries and Tarrance (1972) found it greater among those who are better educated, young, more affluent, of higher occupational status, and more dependent upon the media for their political information and touted these as the characteristics of the "new" ticket splitters. Maddox and Nimmo (1981) also showed ticket splitters to be more media-oriented and found that higher degrees of interest in the campaign induced more ticket splitting.

The three studies that examine the individual voter characteristics of ticket splitters share a focus on multiple offices in their operationalizations of ticket splitting, but their differences in other respects may account for the contrasts in their findings (Campbell and Miller 1957; DeVries and Tarrance 1972; Maddox and Nimmo 1981). The early Campbell and Miller (1957) study and the national survey results used as a part of the DeVries and Tarrance study measure ticket splitting by answers to a general question asking about the extent of ticket splitting, rather than by comparing reported votes for different offices. It is likely that these responses are skewed toward a more divided vote than actually occurred because of the social desirability of ticket splitting in American life (Feigert 1979). These studies also differ in the years they cover. Systematic explanation may have been confounded by a change in the patterns of ticket splitting as it became more prevalent after the mid-1960s.

An alternative stream of research has concentrated on the inducements to ticket splitting presented by popular and visible incumbents from the opposition party. This research has focused on presidential–congressional splits and has taken advantage of the different pairings of candidates across the various congressional districts contained in national surveys (e.g., well over 100 districts are covered in the Michigan surveys). The advantages of incumbency in a time of reduced partisan loyalties have been commonly cited as leading to the split results in presidential and congressional voting (Cover 1977). Gary Jacobson (1989, 1991) has demonstrated, however, that incumbency alone is not the key to a divided vote; rather, the failure of experienced challengers to emerge to face incumbents leaves voters with little real choice but to support the current officeholder. In short, he contends, candidate visibility, rather than incumbency per se, accounts for ticket splitting.

These alternative partisan and candidate theories of ticket splitting are not logically contradictory. The frequency of straight-ticket voting may increase generally with the strength of partisanship even while the presence of strong candidates from the opposing party may undermine party fidelity. The growth of ticket splitting in recent years may have resulted from the decline in the strength of partisan loyalties within the electorate (Wattenberg 1986), as well as from changes in the visibility of challengers. Any comprehensive theory of ticket splitting, consequently, should encompass the characteristics of voters as well as of contestants for office—especially the partisanship of the voter and the relative visibility of the candidates from whom they must choose. A comprehensive theory of this phenomenon requires the incorporation of other factors too; media habits, interest, attentiveness, education, and age may affect ticket splitting independently of partisanship and the nature of the candidates. Only by considering all of these factors together with partisanship and candidates can the divergent results of previous studies be reconciled.

TICKET SPLITTING ACROSS THE BALLOT

Data from the 1990 Ohio statewide election provide a highly appropriate test for this comprehensive model of ticket splitting. On the typical American long ballot, voters have multiple opportunities to split their votes between the parties. We focus our attention upon the five statewide partisan races in the 1990 Ohio election.

Our first task is to estimate the extent of ticket splitting in the election. The conventional way to measure it is to determine the percentage of voters who split their tickets in each pair of races. The range of this measure in the 1990 Ohio elections is from a low of 23% to a high of 38% (Table 1). At the high end of this range, ticket splitting was higher than for president–House pairs in the Michigan surveys in recent years (Fiorina 1990), so that this election already evinces higher rates of splitting than the usual contests of reference.

The level of ticket splitting increases substantially when one examines voting patterns across all five contests. A total of 54% of the voters in our sample cast at least one vote for each party—a number that is virtually twice the often-reported magnitudes when

TABLE 1

Ticket Splitters as Percentage of Voters in Each Pair of Contests

	GOVERNOR	SECRETARY OF STATE	ATTORNEY GENERAL	AUDITOR	TREASURER
Governor	—	23 (487)	23 (422)	34 (440)	38 (392)
Secretary of State	—	—	25 (409)	32 (430)	30 (378)
Attorney General	—	—	—	36 (390)	33 (347)
Auditor	—	—	—	—	25 (363)

Note: Entries are percentages voting for candidates of different parties among those who voted in both contests of the pair. Ns are in parentheses.

only pairs of races are considered. That this level is attained in a strong party state (Fenton 1966, chap. 5; Mayhew 1986; Baum and Patterson 1992) in which both parties provide party slate cards that can be carried into the voting booth is perhaps surprising, even though the Ohio ballot does not permit a straight-party vote by a single action. The 1990 Ohio results show how widespread ticket splitting has become in American elections, and they document the high incidence of such behavior outside of presidential elections.

The analysis that follows concentrates on the patterns and sources of the split party vote across all five contests. The variable in focus is a simple one—whether or not the vote was divided between the parties. This is a dichotomous variable, yielding 53.5% ticket splitters and 46.5% straight ticket voters within the entire sample of voters.[6]

Our objective is to explain why some voters cast a straight party vote whereas others were motivated to divide their vote among candidates from different parties. One approach in answering this question is to consider that voters viewed each contest independently and, after weighing all important considerations, decided to vote for a candidate that turned out to be of the same or the opposite party from the other candidates preferred. In this explanation, each contest is treated as an independent event, requiring a more or less unique accounting. Furthermore, it does not presume a normal behavior of partisan voting from which deviations are to be explained.[7]

While the urge to examine voting behavior contest by contest is strong, particularly given the normal orientation of voting behavior studies and the understandable desire to try to maximize the amount of variance one can explain, our interest is in an alternative "whole ballot" approach that can account for the patterning of votes across the ballot. Only with this focus can a comprehensive theory of ticket splitting be developed.

We shall pursue two lines of analysis to develop this theory. First, we focus intensively on the two most promising variables in explaining the voting patterns, namely, partisanship and candidate visibility. The former is a key voter characteristic, while the latter contains the most salient feature of each contest. Thus, we bring together in the same analysis the two perspectives on ticket splitting that have characterized past research. Second, after we have carefully explored the effects of partisanship and visibility, we turn to a multivariate analysis of the split vote incorporating these and a number of other theoretically promising variables. By estimating a more fully specified model of ticket splitting than earlier studies, we hope to be able to determine the effects of party and candidates, as well as to sort out the relevance of other individual characteristics, especially those thought to characterize the "new" ticket splitters.

THE ROLE OF PARTISANSHIP IN VOTER TICKET SPLITTING

An examination of patterned voting in the contemporary American electorate must begin with the partisan loyalties of individual voters. The most visible—and often the only—link among candidates for office is their political party. In Ohio, this link is clearly identified on the ballot, so that even voters coming into the voting booth with absolutely no prior information about candidates receive this valuable voting cue. For voters who possess party identifications, the party of the candidates should be an especially potent cue; the stronger the voter's party identification, the more we should expect it to influence the voting choice.

The results of Table 2 support the hypothesis that party identification is related to a partisan pattern of voting in a powerful fashion. Strong Democrats and strong Republicans least frequently split their votes in the 1990 Ohio contests. A majority of them supported only the candidates of their party. Those who did divide their votes between the parties did so only minimally. Nonetheless, a substantial minority of strong partisans defected at least once, which suggests that strong partisanship alone is not sufficient to produce a straight party vote.

The threshold of party regularity is crossed once we move to weaker party-identifiers. Majorities of

TABLE 2

Party Identification and Split Ticket Voting, Ohio 1990 (%)

PARTY IDENTIFICATION	ALL D	SPLIT TICKET D > R	SPLIT TICKET D = R	SPLIT TICKET R > D	SPLIT TICKET TOTAL	ALL REP.	N
Strong Democrats	72	24	3	3	30	0	(81)
Weak Democrats	34	41	8	17	66	0	(92)
Ind. Democrats	31	26	14	21	61	7	(42)
Independents	9	49	15	25	89	2	(53)
Ind. Republicans	2	17	7	46	70	28	(54)
Weak Republicans	2	12	1	50	63	34	(90)
Strong Republicans	0	4	4	32	40	60	(92)

Note: N = 504. Pearson's r = .75/Kendall's Tau B = .63. Entries are the percentages for each category of partisanship who vote for all Democrats, all Republicans, or split their tickets. Ticket splitters are divided into those who voted for more Democrats than Republicans, equal numbers of each, and more Republicans than Democrats. Entries were made for voters who cast ballots in two to five of the five contests. Single-contest voters were eliminated because it was impossible for them to split their tickets.

less-than-strong partisans in every category split their votes between the parties, and almost all pure independents were split-ticket voters. Weak partisans and independent-leaners, in fact, behave more like pure independents than strong partisans. These two kinds of partisans, moreover, exhibit such similar patterns of voting behavior that in the subsequent multivariate analysis they will be combined.[8]

One other feature of Table 2 bears mention. In this particular election, strong Republicans were more likely to split their tickets than were strong Democrats. As we shall see, this is entirely attributable to the disproportionate Democratic advantage in incumbency, which that party has enjoyed for the last several decades throughout the country. What we observe in the 1990 Ohio elections, therefore, is an illustration of a general pattern of subpresidential voting: Republicans are more likely to be ticket splitters.

The reasons why partisanship plays such an important role in inhibiting ticket splitting are familiar. Viewed from the perspective of *The American Voter*, partisanship is an indicator of identification with the party as a group (Campbell, Converse, Miller, and Stokes 1960). Support for candidates from that party is an expression of group loyalty and should become more complete as one's party identification strengthens. The same result emerges when partisanship is viewed, from the rational choice perspective, as a valuable voting cue in an environment of imperfect information (Downs 1957; Fiorina 1981). Without information to the contrary, partisans assume that the candidates of their party share their own outlooks and preferences. In the relatively low information context of subpresidential races, then, partisanship should be a particularly valuable guide to voting. While these two perspectives on the role of partisanship in voting behavior rely upon different cognitive processes for explanation, they predict the same result for ticket splitting. Consequently, it is not necessary to choose between these explanations; nor is it possible with the data at hand.

PARTY, CANDIDATE VISIBILITY, AND TICKET SPLITTING

Candidate characteristics, of course, can overwhelm partisanship. The first sign of this is the integral role of incumbency in ticket splitting in the 1990 Ohio races. Almost three-quarters (72%) of the split-ticket voters in our sample divided their votes between the two parties by deviating from a preponderantly Republican voting pattern to support Democratic officeholders seeking reelection. Nearly two-thirds (64%) of these incumbent-oriented ticket splitters voted Republican for governor, then voted for at least one of the Democratic incumbents for other executive offices. This incumbent-seeking behavior, however, does not tell us why incumbency is so potent a force. For a preliminary answer to this question, we move momentarily away from a focus on the whole ticket to examine individual contests and how candidates in them may be advantaged by the visibility they enjoy in the voters' minds.

Our treatment of the candidate factor focuses on candidate differentials in visibility as perceived by voters. Visibility differentials can have many sources, which are worthy of exploration; but in the end, all that matters is *how* (not why) the voters see the candidates. The measure of perceived candidate visibility we employ is based on a series of questions from the postelection survey asking the respondent to evaluate the candidates on a variety of policy issues, personal traits, and group attachments. (See the Appendix for the text of these and other questions we use.) Each evaluation was scored as an indication of visibility. A candidate's overall visibility to the respondent was the sum of these particular evaluations, and the candidate with the larger total in each race was credited with the visibility advantage for that respondent. Visibility advantage (or disadvantage) is the simple difference between the number of respondents who knew a candidate better and the number who found the opponent more visible.[9]

TABLE 3

The Visibility of Candidates in the Ohio 1990 Election (%)

	VISIBILITY			
OFFICE	REPUBLICAN MORE	EQUAL	DEMOCRAT MORE	N
Governor	21	67	12	428
Secretary of State	19	74	7	421
Attorney General	16	71	13	396
Auditor	4	64	32	412
Treasurer	3	70	27	377

Note: Includes only respondents who voted in each contest. The figures for incumbents are underlined.

The relative visibility advantages in each contest, presented in Table 3, describe a common pattern. The incumbents seeking reelection to the auditor's and treasurer's offices enjoyed substantial visibility advantages over their challengers. That the incumbent secretary of state did not enjoy a similar edge is explained by the fact that the Republican challenger, Robert Taft, was a scion of Ohio's most famous political family. Taft *began* the campaign with high visibility and ended up with a substantial net edge over his incumbent opponent. Thus, our Ohio data contain one case in which a challenger was able to overcome the natural incumbent advantage; and this case will play a crucial role in specifying the nature of the usual incumbency advantage.

Our hypothesis is that these visibility advantages gained extra votes for the candidates who enjoyed them. Testing this hypothesis is no simple matter, however, because the vote gains must be assessed against some kind of baseline. One way to demonstrate the effects of visibility is to examine the relationship between visibility and vote, *controlling for party identification* as the baseline. These relationships are presented in Table 4. If visibility has no independent effect over and above partisanship, then these correlations should be uniformly insignificant. Where visibility has an impact beyond partisanship, by contrast, the correlations should attain significance and should, if visibility has the advantage we expect, be positive in direction.

The results in Table 4 show that candidates gained votes from a visibility advantage that transcended the effects of party identification. Almost all of the correlations are positive, and 14 of the 25 attain statistical significance. The only negative relationships are registered for independents, but they fall far short of significance. The visibility advantage was most pronounced in the three races with the greatest visibility discrepancy between the two candidates. Some of the largest correlations between visibility and vote appear among Democrats in the secretary of state race and among Republicans in the auditor and treasurer contests, which demonstrates the power of visibility in overwhelming partisan attachments. That in one of these contests the challenger, rather than the incumbent, was benefited by visibility, however, suggests (with Jacobson 1989) that incumbency does not guarantee a visibility advantage.

Exactly how visibility influences the vote is so far left unspecified. Candidates' characteristics always have been important in American elections; and with the weakening of partisanship in recent years, their importance seems to have grown. In many subpresidential elections—in contrast to the more familiar presidential contests—the amount of information citizens possess about candidates is so low that virtually any information the voters have may be critical in their vote decision. Since this information is typically favorable, sheer visibility often produces an electoral advantage. Consequently, the difference in what voters know about candidates is an important variable in accounting for vote decisions, even without further refinement as to the exact content and direction of that knowledge (Jacobson 1987).

TABLE 4

Effects of Candidate Visibility Differences on Vote by Party Identification

OFFICE	STRONG DEM.	WEAK/IND. DEM.	PURE IND.	WEAK/IND. REP.	STRONG REP.
Governor	.11	.25*	.13*	.09	.04
	(72)	(111)	(38)	(106)	(83)
Secretary of State	.41*	.18*	.43*	.18*	.13
	(71)	(110)	(39)	(105)	(82)
Attorney General	.23*	.30*	−.07	.08	.08
	(67)	(102)	(34)	(102)	(78)
Auditor	.07	.19*	−.06	.38*	.24*
	(70)	(111)	(36)	(101)	(79)
Treasurer	.08	.18*	−.08	.33*	.43*
	(68)	(100)	(36)	(91)	(70)

Note: Entries are Pearson correlations between candidate visibility differentials and vote within each partisan category for those who voted in four or five contests. To keep the number of cases in each cell relatively high, weak and leaning partisans, who behave similarly, are combined. Ns are in parentheses.
*$p \leq .05$ (two-tailed test).

This, surely, is the source of the well-known incumbency effect. Officeholders have been in the spotlight of usually favorable public attention in previous years and are able to marshal crucial vote-getting resources (e.g., money, media coverage, an already-established organization) during an election campaign. Few challengers can match—much less overcome—the visibility incumbents naturally enjoy. When challengers can erase the incumbent visibility advantage, however, they are likely to win the election (Jacobson 1989). Looking across the ballot, then, when there is variation in candidate visibility by party (as is typically the case), one would expect a divided party vote.

Any comprehensive analysis of split-ticket voting must take into account the nature of the candidates for office and how their public recognition can be an electoral asset or debit. On a case-by-case basis, we have shown that the sheer visibility of a candidate can overcome the normal partisan tendencies of voters. What remains to be done is to integrate this factor systematically into a comprehensive explanation of ticket splitting. To accomplish this task, we turn to multivariate analysis and to a more complicated measure of visibility.

A MULTIVARIATE ANALYSIS OF THE SOURCES OF TICKET SPLITTING

The analysis so far has identified partisanship and candidate visibility as key determinants of ticket splitting. As the representatives of the alternative theoretical foci on voter and candidate characteristics, they are the two leading candidates in an account of the division of the vote between parties across a long ballot. However, they are not the only promising explanations of ticket-splitting behavior. Previous studies have identified other reasons why voters may divide their votes between the parties, and we need to expand our analysis to include them. In order to assess these alternative explanations in conjunction with the partisanship and visibility accountings we have relied upon so far, a full multivariate analysis is required. The dependent variable in this analysis is the dichotomous measure of whether or not the respondent split his or her ticket.

The first variable added to partisanship and visibility in the multivariate analysis is age (measured in years). Some previous research has suggested that younger voters are more inclined than older voters to split their tickets. This behavior may lend itself to a life-cycle explanation. But the contention that the "new" ticket splitters are to be disproportionately found among the young suggests that there is something particular about this *generation* of young voters (DeVries and Tarrance 1972). One reason why the young may be more likely to cast divided party votes, of course, is that their partisan loyalties are weaker, for either life-cycle or generational reasons. By assessing the effects of age in a multivariate model that contains partisan intensity as a variable, we can examine this interpretation of the relationship of age to ticket splitting.

A second explanation for ticket splitting that merits representation in our multivariate model stems from the notion that cognitively sophisticated voters are less dependent on decision-making shortcuts. Their vote is more likely to be based on candidate-by-candidate evaluations, instead of relying on such heuristics as party or even the coattails of the top-of-the-ticket candidate (Chubb 1988; Shively 1979). Cognitive sophistication is captured in a crude but meaningful basis by education, measured in years of schooling. Education has been variously related to ticket splitting in previous research: better-educated voters were more likely to vote straight party in the Campbell and Miller (1957) study but more likely to *split* their votes in the DeVries and Tarrance (1972) analysis. By including education in our multivariate model, we can help to reconcile these divergent results.

Political sophistication is not simply a function of general cognitive ability (Converse 1964, 1980). Some people at each level of education pay inordinate attention to politics and, as a result, are able to bring considerable information to bear on their political decisions. This political specialization may be reflected in self-reports of political interest at the end of the campaign. Maddox and Nimmo (1981) found that ticket splitting was higher among those with the greatest political interest. There are good reasons to pursue this line of analysis here. To test the hypothesis that political interest induces a split ballot, we include the variable INTEREST, which varies from *not much interested* (scored 1) to *very much interested* (scored 3) in the campaign.

Third, ticket splitting may be a function of how people receive their information about politics. Television has become a key source of political information for almost all Americans. With its reliance on visual images, this medium is seen as fostering a personalization of politics, an emphasis on candidates over issues or party (Popkin 1991; Rubin 1981). Consequently, voters who derive their political information primarily from television may be expected to be more candidate-oriented, which leaves them more inclined to divide their votes between the parties across the ballot (Maddox and Nimmo 1981). By this logic, it is the increasing dominance of television as an information source in recent decades that accounts for the rise in ticket splitting.

This television-centered explanation for ticket splitting, too, warrants inclusion in our multivariate model. Because television use is universal, we represent it by determining whether respondents report a greater reliance on television as a source of their political information than either newspapers or personal discussants. The variable is TVDOM, scored 1 if television is favored and 0 if it is not. Those who rely upon television most, this hypothesis claims, should be more likely to split their tickets.

Fourth, some scholars have seen split-ticket voting

as strategic behavior, in which voters who lack confidence in, or support for, either party are rationally motivated to divide their votes so that they can prevent one party from gaining full control of government (Alesina and Rosenthal 1989; Fiorina 1990). A test of this explanation for ticket splitting can be fashioned by employing attitudes toward divided government. We employ an agree–disagree question about its desirability, SPLITCON—scored from *strongly disagree* (1), to *strongly agree* (4), with the ideal of one-party control—that can be thought of as a general orientation because it was asked at the beginning of the 1990 campaign. If votes are motivated by a conscious desire to divide executive positions between the two parties, then this variable should be significantly related to ticket splitting.

Finally, the party identification and visibility variables are each represented in our model. Party is measured in terms of strength (PTYSTR) by folding the seven-point party identification scale at its independent midpoint and combining the virtually indistinguishable categories of weak partisan and independent-leaner. (The result is 1 = independent, 2 = leaning partisan/weak partisan, 3 = strong partisan.) Based on the results of previous research, partisan strength is expected to be significantly related to ticket splitting (as Table 2 demonstrates clearly in the bivariate case).

Table 2 also exhibits a tendency for Republicans to split their tickets more than Democrats in the 1990 Ohio elections. We hypothesize that this tendency can be accounted for by the generally higher visibility of Democratic candidates in the election and perhaps some of the other factors we have identified. To test this hypothesis fully requires the inclusion of a Republican partisan direction variable (REP, scored 1 if weak or strong Republican, 0 if independent, independent-leaner, or Democrat) in our model, so that we can determine whether its relationship to ticket splitting vanishes once the effects of other variables are taken into account, as we expect.

The measurement of perceived candidate visibility across the subpresidential ballot is more complex. Earlier, the effects of visibility in individual contests were inferred from the relationship between a visibility difference between candidates and the vote with controls for party identification. The conceptualization of visibility as it operates across the ballot necessarily takes this logic one step further. It depends upon the notion that the pressures to split a ticket are increased when candidates from different parties enjoy the relative visibility advantages across the ticket. The variable that expresses these expectations, called VISSPLIT, is scored 1 when candidates from different parties are more visible to the voter and 0 when all of the more visible candidates come from the same party.

Analysis of Voters in at Least Two Races

Table 5 presents the results of a multivariate analysis of the split ticket vote dichotomy on the variables just identified (see the Appendix for the questions underlying these measures) for those who have voted in more than one race.[10] These results confirm the dominance of partisan strength (PTYSTR) in accounting for split-ticket voting. No other variable is as strongly associated with ticket splitting behavior. It is hardly surprising that departures from a single-party vote decrease as the voter becomes more strongly committed to the party and that this commitment is the primary predictor of party steadfastness in voting. This relationship also makes the contemporary increase in ticket splitting more comprehensible. As partisan intensity has declined in recent years, it surely has brought with it a parallel decline in party-ticket voting.

TABLE 5

Probit Analysis of Ticket Splitting

INDEPENDENT VARIABLE	*b*	S.E.	CHANGE IN PROB. OF SPLITTING
PTYSTR	−.79***	.112	.09
REPUBLICAN	−.02	.125	—
VISSPLIT	.62***	.160	.10
SPLITCON	.05	.128	—
TVDOM	−.22	.133	—
INTEREST	−.13	.099	—
EDUCATION	.07**	.028	.01
AGE	.01**	.004	.01
Constant	1.22	.670	—

Note: N = 463. McKelvey Pseudo R^2 = .29; Dhrymes Pseudo R^2 = .15; likelihood ratio = 83.18 (approx. chi-square with 9 d.f.); percentage of cases predicted accurately = 70%. The table excludes those who voted in only one contest. The dependent variable in this analysis is ticket splitting, scored 1 if vote was for candidates from two different parties and 0 if for candidates from only one party. Probabilities of splitting are the changes in the probability of split-ticket voting associated with a one-unit increase in that variable with every other variable held at its mean. Probabilities are not shown for variables whose impact on split-ticket voting fails to achieve statistical significance. In calculating the percentage of cases predicted accurately, the prediction is straight ticket if estimated *p* is less than .50 and split-ticket if estimated *p* is at least .50.

**$p \leq .01$ (one-tailed test).

***$p \leq .001$ (one-tailed test).

The greater tendency for Republicans to split their tickets that appeared earlier in the simple relationship between partisanship and ticket splitting vanishes in a multivariate context. Republicans are more likely to vote for candidates from both parties; but the results suggest that the reason lies in factors other than their partisanship—chiefly, we surmise, in the advantages Democratic incumbents enjoy in visibility.

Candidate visibility joins partisan strength in the multivariate analysis as a potent predictor of ticket splitting. Voters who saw at least one candidate from each party as more visible across the five races were significantly more likely to split their tickets than those for whom the visibility advantage always remained with the same party. Incumbency no doubt explains a substantial part of this effect, since incumbents are best able to overcome the natural advantage held by candidates of a voter's own party or to

monopolize the attention of independents. But—as the presence of a visible Republican challenger in Robert Taft makes it possible to show—it is differential visibility rather than incumbency per se that is conducive to a divided party ballot.

Only two other variables exhibited significant relationships to ticket splitting, and the strength of these relationships fell far short of those for visibility and partisan strength. One of these variables is education, in which a one-year increase in attainment was associated with a .01 increase in the probability of ticket splitting. Unlike the results for the 1950s (e.g., Campbell and Miller 1957) but consonant with more recent findings (e.g., DeVries and Tarrance 1972), more educated respondents were more likely to split their tickets in the 1990 Ohio elections. Because the education of the electorate has been growing in recent decades, the overall importance of this relationship probably transcends its coefficient. The case for a "new" ticket splitter finds some support in the reversal of the old voting patterns by those with higher levels of education.

Split-ticket voting is also more common among older voters. This is an unanticipated result from the standpoint of both earlier research and theoretical expectations. Younger voters are normally expected to be less firmly anchored in their voting behavior. They tend to be less partisan than their elders, although there has been a surge of the youngest age group into partisanship in more recent years. They also are better educated. Both of these factors should predispose them to split their tickets more; yet even the bivariate correlation between age and ticket splitting is positive, not negative. Moreover, the magnitude of this positive relationship increases once other variables, including partisanship and education, are taken into account. The image of the "new" ticket splitters as disproportionately young does not bear up under closer scrutiny in our data.[11]

The other hypotheses tendered to explain ticket splitting fail to survive our analysis. The image of ticket splitters as television-dependent is undermined, at least in Ohio for 1990, by the fact that respondents for whom television dominates newspapers and personal discussions as a source of political information *less* frequently divide their votes between the parties (although this relationship falls short of significance).

Nor does the hypothesis that ticket splitters are rationally dividing governmental powers between the parties (Fiorina 1990) receive support in our data. The attitude that government works better under conditions of divided control bears no relationship to ticket splitting, even after a host of other factors predisposing the voter toward dividing the vote are taken into account. This result accords with findings from another contemporary study (Alvarez and Schousen 1991), which makes it all the more likely that the divided-government-motivated-voter hypothesis lacks explanatory power.

Another hypothesis that emerged from earlier research is that voters with higher levels of interest in politics were more likely to split their ballots. This hypothesis, too, receives no support in our analysis. As interest increases, frequency of ticket splitting declines—rather than increasing, as previous research had led us to expect it would. But the relationship is so modest that it falls far short of achieving significance.

Demonstrating the Robustness of the Results

Ticket splitting has proven to be a more complicated phenomenon than it appeared to be at first blush. Additional analysis is needed to make certain that these complications have not produced anomalous results. One complication is created by widespread *roll-off* (the tendency not to vote for some contests—see Burnham 1965) in the 1990 Ohio elections. The failure to cast a vote in all races on the ballot lowers the probability of ticket splitting. By restricting analysis to those voters who cast ballots in two or more contests, Table 5 has eliminated the complication imposed by those for whom the probability of a split ballot was zero. But 115 voters (almost 20% of all the voters in our sample) cast ballots in only two or three contests, thereby severely restricting their opportunities to divide their votes between parties. We shall have more confidence if our results hold up after these voters, too, are removed.

Another complication is created by the possibility that idiosyncratic forces may play upon the vote. Deviations to vote for the other party by the erstwhile party voter can occur for a variety of reasons, such as a personal friendship, a local "favorite son or daughter," a key issue, or a personal scandal. While the resulting votes are appropriately counted as divided between the parties, a peculiar distribution of these idiosyncrasies in an election might give a distorted picture of the sources of ticket splitting. It is probably safe to assume that these reasons are randomly distributed across contests and years, so that they do not confound our results. To guard against the possibility of this kind of bias, however, we have used the more stringent comparison of those who voted for at least two candidates from each party, as opposed to straight-ticket voters.

Table 6 presents the results obtained from these two different lines of analysis. What is most striking about these results is that the coefficients for the partisan strength and visibility variables change hardly at all from those in Table 5. These two variables appear to be powerful sources of ticket splitting regardless of the circumstances. The same cannot be said, however, for age and education. While virtually unaffected by increases in the opportunities for ticket splitting, these coefficients decline and become insignificant among the most extensive ticket splitters, namely, those who have voted for at least two candidates from each of the parties. Only partisan strength and differential candidate visibility are related to this extreme form of ticket splitting.

All in all, this analysis increases confidence that our results are not an artifact of a measure that is overly

TABLE 6

Probit Analysis of Ticket Splitting for Voters in Four or Five Races

INDEPENDENT VARIABLE	ONE SPLIT b	ONE SPLIT S.E.	TWO SPLITS b	TWO SPLITS S.E.
PTYSTR	−.86***	.127	−1.01***	.165
REPUBLICAN	.03	.158	−.03	.214
VISSPLIT	.62**	.173	.85***	.207
SPLITCON	−.01	.125	.06	.177
TVDOM	−.08	.157	.03	.188
INTEREST	−.13	.108	−.09	.150
EDUCATION	.08**	.032	.05	.038
AGE	.01*	.005	.00	.006
Constant	1.47*	.724	1.76*	.912
McKelvey Pseudo R^2	.31		.38	
Dhrymes Pseudo R^2	.16		.22	
Likelihood ratio (approx. chi-square with 8 d.f.)	83.18		71.44	
%Cases predicted accurately (%)	70.0		73.0	
N	385		249	

Note: Only voters in four or five races are included in this table. In column 1 the dependent ticket-splitting variable is scored 1 if the respondent voted for candidates from two different parties and 0 if for candidates from the same party. In column 2 it is scored 1 if the respondent voted for two candidates from each party and 0 if for candidates from the same party, with those who split their tickets only once eliminated. In calculating the percentage of cases predicted accurately, the prediction is straight-ticket if estimated p is less than .50 and split-ticket if estimated p is at least .50.

*$p \leq .05$ (one-tailed test).
**$p \leq .01$ (one-tailed test).
***$p \leq .001$ (one-tailed test).

sensitive to a sample that includes too many partial voters or to idiosyncratic behaviors. While other (albeit less theoretically justifiable) validity tests could be run, we are confident that they would yield similar support for the robustness of the results reported in Table 5.[12]

CONCLUSION

Divided control of government is as important a feature of the American states as it is of the national government. The results of the 1990 Ohio election could not have produced better evidence of this. When the dust had settled, the Republicans continued their control of the Ohio senate; the Democrats, their hold on the state House of Representatives. Furthermore, the Republican party broke the previous Democratic monopoly on executive positions by gaining the offices of governor and secretary of state, leaving the other three statewide executive positions in the hands of their Democratic rivals. Thus, the contests for executive office in Ohio in 1990 have provided us with an excellent vantage point from which to explore the electoral bases for divided government.

The traditional American long ballot, in a political structure and political culture that subvert party regularity, offers voters ample opportunity to support divided party control of government. A majority of the Ohio electorate in 1990 took advantage of this opportunity by casting ballots for candidates from each of the two major parties for the executive offices. The election's split outcomes are primarily (although not wholly, because of roll-off) attributable to this behavior.

Based on these results, it seems reasonable to suppose that split-ticket voting in American elections is far more common across the full ballot than previous studies focusing on pairs of contests or congressional–presidential divisions have implied. To adequately study ticket-splitting behavior, we believe that it is imperative to consider the full menu of contests placed before the voter in any single election, including both the rare high visibility contests and the commonplace races of low visibility. The heavy focus in previous research on splits in pairs of contests, especially the presidential–congressional set, risks both underestimation of the frequency of the behavior and misspecification of its sources.

Despite the fact that ticket splitting is often the result of independent decisions for the different contests on the ballot, clear sources of this behavior across the subpresidential ballot have been identified. The primary motivating factor for ticket splitting—contrary to the discovery of ticket splitters as a "new force" two decades ago (DeVries and Tarrance 1972)—is a long established and powerful force in American voting behavior, strength of party identification. Documented as the key determinant of the split-party vote as long ago as the 1950s (Campbell and Miller 1957), partisanship turns out to be the prime determinant of split-ticket voting in Ohio in 1990. This result would surely be replicated in elections at various levels of government across the nation.

A focus on multiple offices across the ballot enables us also to determine how different candidate pairings may affect ticket splitting. This analysis shows that *the same voters* react differently to candidates based solely on their relative visibility, without regard to the content of these candidate perceptions. Candidates who enjoy a visibility advantage are very successful in attracting votes beyond their own partisan camp and in solidifying the support of their fellow partisans. The major source of visibility is, of course, incumbency. Indeed, in the 1990 Ohio elections, the preponderance of split tickets were the result of the powerful pull of incumbency and the great advantages it conferred on two of the incumbent candidates. Nonetheless, because incumbency does not always ensure such an advantage (as our results have shown), the explanation should rest more generally upon candidate visibility.

Especially because these Ohio results parallel those from studies of congressional–presidential splits in important respects, we have little hesitation in generalizing beyond the confines of our specific data.

Incumbency is an important contributor to ticket splitting, but this is because of the visibility advantages it often confers and not some intrinsic quality of incumbency itself. The visibility edge induces opposite-party loyalists to defect from their party's candidates, pulls independents into the favored candidate's orbit, and reinforces the natural tendencies of fellow partisans. In Ohio in 1990, this phenomenon generally favored the election of Democrats; and it is likely that the same would hold true for other states and levels of government where the Democrats enjoy an incumbency advantage. If so, this "pull of incumbency" should be added to macro theories of electoral change.

Evidence in our results for the "new" ticket splitter described by DeVries and Tarrance (1972) is mixed. In accordance with the results of more recent studies, those who split their tickets in our sample were generally better educated than those who voted straight tickets, even after the effects of partisan intensity and visibility were taken into account. By contrast, the classic 1950s study of ticket splitting found it occurring disproportionately among the *least* educated voters (Campbell and Miller 1957). The effects of education, however, appear to apply only to single-contest deviations. Recurrent splitters (at least across the 1990 Ohio ballot) were not distinctive in their educational levels.

This "new" ticket splitter apparently does not include the youngest members of the electorate. The young in our study were more likely than older voters to vote *straight* tickets, rather than to divide their votes between candidates from different parties. We can hypothesize alternative reasons for this anomalous result. First, the relationship between ticket splitting and age is consonant with a situation in which realigning forces have produced a new generation of loyal partisans among the young without converting often-unfaithful older partisans from their established loyalties. Second, older voters may be more familiar with candidates who have been prominent on the political scene (but not just the Taft name, as we have shown); consequently, these voters are less dependent on the party cue. Whatever the explanation, the greater propensity of the young to divide their votes that appeared in previous studies was not replicated in the Ohio 1990 election.

Finally, the story of ticket splitting in Ohio during the 1990 elections can be told entirely without resort to some variables that have proven important in earlier studies. In particular, levels of interest in politics proved to have no significant relationship to voting for candidates from both parties. Neither did dominance of television as an information source or a motivation to produce divided government emerge as important. These results could be anomalies of our case, but we suspect that they are more likely to be typical results of the multivariate, multiple-vote approach that sheds more light on ticket splitting as patterned behavior.

A combination of motivation (partisanship) and opportunity (the visibility differentials of the candidates), with a pinch of education and age, then, accounts for a substantial portion of ticket splitting across the 1990 Ohio subpresidential ballot. To be sure, the nature of the ballot itself, by making it easier or harder to vote a straight ticket, can accentuate these effects. The Ohio office-block ballot, for example, makes it easier than in many states to split a vote between the parties. A cross-state study would have to take this structural feature into account. But in the end, any theory attempting to explain ticket splitting behavior would be wise to focus its attention on partisanship, candidate visibility, education, and age, paying special attention to the first two. To understand systematic patterns of ticket splitting, in short, requires a merging of the two separate research traditions that have characterized its study to this point.

APPENDIX: MEASUREMENT OF KEY VARIABLES

Votes. The measures of votes in the various contests are based on the questions, "Did you vote in the contest for ——— between ——— and ———?" and if *yes*, "Did you vote for ———, the Democrat, or ———, the Republican?"

Party Identification. The measures of party identification are based on the question, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an independent, or what?" and, if Republican or Democrat, "Would you call yourself a strong (Republican, Democrat) or not very strong (Republican, Democrat)?" or if independent or *no preference*, "Do you think of yourself as closer to the Republican party or to the Democratic party?"

Visibility. The measures of visibility are based on questions asking respondents to place each candidate on a five-point continuum for four issues that were important in the 1990 campaign (abortion, campaign fund-raising, taxes, and government regulation of business); on a five-point liberal–conservative continuum; on six traits (competent, honest, strong as a leader, caring about people, decent, hardworking); and on closeness to six groups (Catholics, business, Christian fundamentalists, unions, blacks, and feminists). The issue questions (too lengthy to list here) were similar in form to those used in the American National Election Study (ANES) with specific positions anchoring each pole. The trait questions were identical to the ANES questions. The group closeness questions were worded: "Now I want you to tell me if you see any of the candidates as *really close* to some groups in American society. The first candidate is ———. Do you see him as *really close* to ———?"

SPLITCON. This variable is based on responses to the statement "State government works best when one party controls all the statewide offices."

TVDOM. This variable is based on the combination of responses to three questions: "In an average week, how often do you read a daily newspaper?", "In an average week, how often do you watch the local television news?" and "Have you talked about the campaigns, the candidates, or the issues in the 1990 elections with anyone—such as a friend, neighbor, coworker, your spouse or other relative?" Where the respondent watched television regularly but did not regularly read newspapers or did not talk with anyone, television was coded as dominant.

Interest. This variable is based on the following question asked after the election: "Some people don't pay much attention to political campaigns. How about you? Would you say that you were very much interested, somewhat interested, or not much interested in following the campaigns this year?"

Notes

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1. This number includes Nebraska's nonpartisan unicameral legislature, which has long been controlled by Republicans. Nebraska currently has a Democratic governor.

2. Before the 1890s, each party prepared its own ballot in most of the states. Ticket splitting, consequently, required the taking of two party ballots in full public view and the combining of them in some fashion to register votes for each party. Given this requirement, it is understandable that ticket splitting was so rare prior to the turn of the century.

3. In the two open-seat contests, a Republican was elected governor, and a Democrat won as attorney general. Two of the three incumbents won reelection, with incumbent Democrats winning the races for treasurer and auditor and a Republican challenger being elected as secretary of state.

4. A total of 1,277 Ohio adults were interviewed in wave 1 of the panel, which was conducted in September and early-to-mid October. The response rate for this wave was 57%. (A wave 2 interview was conducted for 492 wave 1 respondents within two weeks of the election, but these data are not utilized in this analysis.) Of these wave 1 respondents, 1,033 completed wave 3 postelection interviews for a reinterview rate of 81%. Only 567 of the wave 3 respondents reported having voted in the election, however; and they are the major focus of analysis. This two-wave panel sample matches up well with the known characteristics of the Ohio adult population in gender, age, race, and county of residence (based on the 1990 census). Like most surveys, however, it overrepresents people with higher levels of education. This sample bias should be taken into account in generalizing from the results.

5. The studies were conducted by the Survey Research Center at the University of Michigan. Beginning in 1970, the Michigan surveys were conducted by the Center for Political Studies, which had devolved from the Survey Research Center. Since the mid-1970s, these studies also have been referred to as the American National Election Studies.

6. A total of 6% of the voters cast a ballot in only one race (usually for governor), while 6% and 12% voted in two and three contests, respectively. One-race voters could not split their tickets and the opportunities to split were low for two- and three-race voters as well, so it should not be surprising to find that the percentage of ticket splitters is considerably lower among these voters (36%) than among four- and five-race voters (59%). Roll-off could be construed as a form of ticket splitting, but we reserve this treatment for subsequent study.

7. In effect, our treatment assumes that a latent continuous variable (reflecting propensity to split the ticket) underlies the dichotomous split/straight categories. We did consider, however, a number of alternative functional forms for the dependent variable. Because our theoretical interest lay in allowing alternative foundations for ticket splitting to compete as explanations for a singular, across-the-ballot behavior, in the end we chose to focus more or less exclusively on a dichotomous dependent variable.

8. When the party identification measure is folded at its independent midpoint so that it ranges from strong partisans of either party to independents, the relationship (and the similarity between weak partisans and independent-leaners) becomes even clearer. The percentage of ticket splitters declines in step fashion (82%, 63%, 60%, 33%) from independent to leaners to weak partisans and finally to strong partisans; and the Pearson's $r = -.30$. All subsequent analyses were conducted by measuring partisan strength in alternative ways—with and without leaners and weak partisans combined. The results were virtually identical and yielded no differences of substantive importance. Because combining leaners and weak partisans produces a slightly greater impact for partisan strength and using dummy variables for each category (save one) produces the same result but is more complicated to explain, we report the estimates for the three-category variable.

9. Visibility is measured from the postelection surveys, because the preelection results (wave 1) yielded so many no-information ties between candidates that they had virtually no discriminatory power.

10. We exclude the 36 voters who cast ballots for only one contest so as not to confound our analysis with those for whom ticket splitting is a logical impossibility. Because the dependent variable is a dichotomy, these are probit estimates rather than ordinary least squares regression estimates. Probit has the further advantage of allowing us to translate the effects of each variable into probability terms. To accomplish this, we set all other predictors at their mean values and then compute the change in the predicted probability of a split ticket that would occur as a result of a one-unit increase in the explanatory variable of interest.

11. We have estimated a variety of different equations to try to eliminate this unexpected age effect, but it has proven to be robust throughout. In particular, we reestimated the equations in Tables 5 and 6 without the race including Bob Taft, to see if the attraction of the Taft name to older voters might have induced them to split their tickets in this one instance; but the results were unchanged.

12. It is important to keep in mind that the variations on the basic model reported here are only two of a number of alternative specifications, many of which may extend beyond simple restrictions on the sample or measurements of the dependent variable. Indeed, we hope that our work will stimulate future efforts at exploring alternative qualitative and quantitative representations of ticket splitting across the ballot.

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ADAPTIVE PARTIES IN SPATIAL ELECTIONS

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We develop a model of two-party spatial elections that departs from the standard model in three respects: parties' information about voters' preferences is limited to polls; parties can be either office-seeking or ideological; and parties are not perfect optimizers, that is, they are modelled as boundedly rational adaptive actors. We employ computer search algorithms to model the adaptive behavior of parties and show that three distinct search algorithms lead to similar results. Our findings suggest that convergence in spatial voting models is robust to variations in the intelligence of parties. We also find that an adaptive party in a complex issue space may not be able to defeat a well-positioned incumbent.

Since Anthony Downs's *Economic Theory of Democracy* (1957), a spatial theory of elections has occupied a prominent theoretical status within political science. Modelers use the intuitive notion of ideological distance to develop explanations for observable electoral trends. The most famous of these trends is the Downsian idea that in a two-party system, given certain assumptions, parties converge toward a median position on the continuum of possible voter positions. Although simple spatial models have produced this result, extensions to the models have questioned the robustness of this prediction. Following the voting paradox and the results of Plott (1967) and McKelvey (1976), some scholars have speculated that chaotic results are possible and, in some cases, likely. In two or more dimensions, the top cycle set consists of the entire space of issue positions.[1] Bates summarizes the more general result: "The principle lesson is that, in general, one cannot expect an equilibrium to exist; and, because any outcome can be defeated, political decisions represent arbitrary outcomes" (1990, 45). Whereas some scholars concede the predicted instability in multidimensional voting models (Riker 1982), others see the Downsian model's stability and convergence as more empirically accurate and (perhaps) more normatively desirable.

Some theorists, inclined to believe that electoral chaos is extremely unlikely, have incorporated various complexities to explain stable, often centrist, outcomes. Coughlin (1990a) divides these models into four general categories: (1) models that allow for mixed strategies by parties, (2) models that track dynamic trajectories of party locations, (3) models that search for uncovered or undominated sets, and (4) models that include candidate uncertainty over voters' behavior (probabilistic voting models). He writes, "It is hard to resist the alternative inference that the primary contribution of recent work on the majority rule relation is as a grand 'reductio ad absurdum' that tells us to go back to the basic model that has been used to see how it should be modified in order for theory and empirical observations to match up" (p. 164).

Two-party electoral outcomes appear more stable than the chaotic results predict, so the task of seeking alternative assumptions to get more realistic outcomes has a sound scientific basis. Nevertheless, both original spatial models and many contemporary revisions rely on unrealistic assumptions to produce equilibria. They often assume fully informed and optimizing voters and parties (Davis, Hinich, and Ordeshook 1970). Spatial modeling seems wedded to the defining assumptions of rational choice, namely, that people and organizations are self-interested, have complete information, and can locate optimal strategies regardless of complexity. Chaos results, for example, rely on unrealistic assumptions about the abilities of parties to locate winning platforms. Probabilistic voting models assume that parties position themselves optimally given complete knowledge of the probabilities of voters' actions (Coughlin 1990b). We find implausible even the restricted assumptions that parties have sufficient information and analytical abilities to locate optimally. Perhaps different assumptions about the abilities of parties or voters lead to stable outcomes.

We incorporate boundedly rational parties in a model of two-party competition. Instead of modeling parties as full-information global maximizers, we model them as incompletely informed and adaptive. They move incrementally toward better regions of the space through the use of search algorithms. Our model involves a dynamic interaction between parties who make decisions in an evolving environment. Much like Kramer (1977), we are interested in the trajectory of party platforms over a sequence of elections. Using our methodology, we are able to explore a variety of questions: Do imposed informational and computational constraints lead to arbitrary outcomes, with winning party platforms scattered throughout the space? Do boundedly rational parties converge toward centrist platforms? Do parties always defeat incumbents, as many models suggest? Does altering the preferences of parties from vote

maximizing to winning with ideals affect the behavior of parties? Our use of adaptive agents allows us to address these questions without wholly abandoning formal modeling.

We shall discuss the advantages of using artificial adaptive agents in the social sciences and present our basic model and a description of the two types of party preferences that we consider. We then present the three search procedures used by our parties and describe our results. We conclude with a discussion of the role of information in elections and possible directions for future research.

THE USE OF ARTIFICIAL ADAPTIVE AGENTS

Holland and Miller advocate a class of models using artificial adaptive agents (AAA), a technique in which "the unfolding behavior of the models can be observed step-by-step" (1991, 366). Based on computer languages, these models are flexible enough to capture a variety of situations yet they maintain precision and logical consistency.[2] Computational models allow the exploration of systems of well-defined agents in a perfectly controlled environment that can be easily and rapidly replicated under many conditions. Moreover, any conceivable analysis is feasible since the state of the system is fully recoverable. This allows the researcher to generate, develop, and test theoretical hypotheses quickly. Although optimization is the key benchmark in much social science theory, learning, adaptation, and bounded rationality are recognized as important processes. Through the use of AAA models, questions about the relationship between optimization and adaptation can be explicitly explored. We adopt the AAA technique because of these advantages and because of our belief that political parties are better characterized as partially informed, finite-memory information processors who rely on rules of thumb than as completely informed rational agents.

Underlying our methodology is the notion that there exist important classes of behavior that can be captured in models too complex for traditional mathematical analysis. Absence of equilibria in a model (or equilibria that human agents could locate only by chance) does not necessarily imply a lack of predictability. AAA allow us to search previously inaccessible models for patterns of generic behavior. Our choice of technique has costs. Restriction to any one type of algorithm can yield ad hoc results, and predictions are often less precise than those of rational actor models. To control for these possibilities, we compare three distinct types of AAA, and arrive at strikingly similar results for all three. The use of multiple algorithms to discover equivalence classes of adaptive behavior distinguishes our model from others and strengthens our results.

There are many precedents for the use of AAA and similar techniques in the social and behavioral sciences. For example, economists have applied AAA to evolve efficient bilateral trading rules (Marimon, McGrattan, and Sargent 1990) and to explore learning in varying economic environments (Arifovic 1989; Miller 1986). In political science, Cohen (1984) has used computer models to explore competing theories of organizational decision making, and Whicker and Strickland (1990) have used computer simulations to test the importance of opinion distribution and decision rules on the constitutional amendment process. Axelrod (1986) has simulated the evolution of norms and has also used a genetic algorithm to develop new strategies that were superior to Tit for Tat in his 1984 computer tournaments (Axelrod 1987). One of the present authors, Miller (1987), has analyzed the coevolution of strategies and the emergence of cooperation in noisy repeated prisoner's dilemma games using adaptive algorithms.

Thus, we agree with Coleman (1989) that simulation models with humans, computers, or both can be useful in the construction of social theory. We shall use computer models to trace the emergent behavior of boundedly rational parties in the context of spatial voting models. Since our parties are limited in both the information they possess and in how they process the information, there is no guarantee that in finite time they will find optimal locations in the policy space. We can, however, compare the general movements of the parties over time to the predictions of mathematical models of party competition. The model we put forth does not stretch the boundaries of our technique. Our present purposes—to address a central problem in positive political theory and show the strength of our approach—are best served by a simple model.

THE BASIC MODEL

Our model incorporates many of the assumptions of spatial voting models, including voters who are perfectly informed about candidate platforms. We follow more recent models and relax the assumption of identical voter preference intensities. We call a voter's preference intensity a *strength*, interpreted as the importance that the voter attaches to an issue. Strengths vary, so voters have noncircular indifference curves. Our model departs from standard spatial models in three respects. First, we consider both purely office-seeking parties and ideological parties. Second, our parties are not perfectly informed, and do not know individual voter utility functions. Rather, they obtain information through "test" elections (like opinion polls) that measure how well their current platform would do relative to their opponent's platform. During a campaign, parties test their platforms on voters, receive feedback in the form of vote totals, and alter their platforms to improve vote totals. Finally (as discussed), our parties do not optimize given their limited information. Instead, using the vote totals from test elections, they adap-

tively alter their platforms, trying to defeat their opponent.

Formally, there are two parties competing for V votes in an n-dimensional issue space. Each voter's preferences are represented by two vectors of n integers, which give the voter's ideal positions and strengths on the n issues. We assume that there are k possible positions on each issue $\{0, 1, \ldots, k-1\}$ and s possible strengths $\{0, 1, \ldots, s-1\}$. The utility to a voter from a party's platform, $y \in \{0, 1, \ldots, k-1\}^n$, is given by the negative of the squared weighted Euclidean distance, where the weight on the ith issue is the strength associated with that issue by the voter. If s_{ji} denotes the jth voter's strength on the ith issue and x_{ji} is the jth voter's ideal point, then a voter's utility is given by

$$u_j(y) = -\sum_{i=1}^{n} s_{ji} \cdot (x_{ji} - y_i)^2$$

In this model we assume that both strengths and ideal points are independently and uniformly distributed. The election results we present consider 251 voters, 15 issues, 7 positions per issue, and 3 possible strengths. Therefore, on average, each voter has five issues of major importance ($s_{ji} = 2$), five of minor importance ($s_{ji} = 1$), and five of no importance ($s_{ij} = 0$). The assumption that voter ideal points are uniformly distributed does not necessarily imply regularity. A relatively small number of voters are generated in a large space, so a spray of points is a more appropriate way to think of the distribution.[3] Each voter casts a ballot for the party giving the higher personal utility. To evaluate the trajectory of democratic outcomes, we introduce *centrality*, a measure of the goodness of each outcome. Without such a measure, we cannot compare our model analytically to any other model. We calculate centrality by computing both the sum of the utilities of the individual voters if the winning party were located at the median on all issues and the sum of utilities resulting from the winning party in the election, then divide the former by the latter.

$$c(y) = \left[\sum_{j=1}^{V} u_j(\text{median})\right] \Big/ \left[\sum_{j=1}^{V} u_j(y)\right]$$

It follows that $c(\text{median}) = 1$. This normalization has the interpretation that the higher the centrality, the closer the winning candidate is to the weighted center of voter preferences and therefore the more responsive the democratic outcome. We attach no normative significance to the median itself as an outcome. We merely exploit the fact that it is generally of high aggregate utility. There may exist platforms with centralities greater than 1. Ideally, we would find the platform of minimal average distance and use its utility as the numerator; but the costs in computer time outweigh any advantages. Regardless of the numerator, we have a measure of aggregate utility, or the average weighted distance to a voter.

Parties are initially represented by a randomly selected "ideal" platform. Since parties are aggregates of individuals, we assume they have uniform strengths across all issues. We consider two types of parties: *ambitious* and *ideological*. Ambitious parties care only about winning elections, and their ideal platform serves only as a starting point for the initial campaign. Ideological parties also want to win the elections, but they want to win with a platform that is spatially close to their ideal platform. Formally, if $v(\mathbf{y}:\mathbf{x})$ is the number of votes the challenger party receives if it takes platform $\mathbf{y}$ and the incumbent is at $\mathbf{x}$, and $\mathbf{Y}$ is the challenger party's ideal point, then the objective functions for the ambitious and ideological parties can be written as:

$$\text{Obj}_{\text{am}}(y) = v(\mathbf{y}:\mathbf{x})$$

$$\text{Obj}_{\text{id}}(y) = \begin{cases} v(\mathbf{y}:\mathbf{x}) & \text{if } v(\mathbf{y}, \mathbf{x}) \le V/2 \\ V/2 + n \cdot k^2 - \sum_{i=1}^{n} (y_i - Y_i)^2 & \text{if } v(\mathbf{y}:\mathbf{x}) > V/2. \end{cases}$$

Recall that V is the total number of voters, n the number of issues, and k the number of positions per issue. This implies that

$$n \cdot k^2 \ge \sum_{i=1}^{n} (y_i - Y_i)^2$$

for any y and $\mathbf{Y}$. Thus, ambitious parties attempt to maximize votes in the hopes that a larger margin of victory makes them more difficult to defeat in subsequent elections, while ideological parties have lexicographic preferences. Their primary goal is to win the election: $v(\mathbf{y}:\mathbf{x}) > V/2$. Once this is accomplished, they attempt to get as close to their ideal platform as possible.[4]

During each election, the incumbent party's platform is fixed, and the challenger party attempts to find a platform in the issue space that defeats the incumbent. In the first election the incumbent party (arbitrarily chosen) remains at its ideal platform. Thereafter, the incumbent remains at the platform where it won the previous election. The challenger party attempts to defeat the incumbent by choosing a new platform. The challenger party, during a finite campaign, tests new platforms on voters who are assumed to have perfect information about both platforms. These tests are accurate polls of political popularity.

Both ambitious and ideological parties are constrained in how they search the issue space for good platforms. First, the campaigns are of finite length, so parties are limited in the number of polls they can take. For example, a party may only be able to take 40 polls before the election. Second, during any platform adaptation, our parties are limited by the number of issues they can change and the degree of change on any such issue.[5] Even for ambitious par-

ties, the ideal platform functions as an ideological tether in early elections.

Positioning constraints and finite campaign length imply that our parties fail to fulfill their goals optimally. Ideological challengers should typically win elections because ideological incumbents attempt to stay near their ideal platforms, which generally lie in regions of average centrality. However, because of their limited ability and information, ideological challengers accept platforms further from their ideal platforms than necessary. With ambitious parties, after a few elections the incumbents' platforms are located in regions of high centrality, making them difficult to defeat. Therefore, ambitious challengers often lose.

HOW PARTIES FIND PLATFORMS

Once we relax the assumptions that parties have complete information and the ability to locate optimal platforms, we can model our parties in a variety of ways. There are many ways to not be perfectly rational. We chose three types of parties, each having a different platform search procedure: *random adaptive parties* (RAPs), *climbing adaptive parties* (CAPs), and *genetic adaptive parties* (GAPs). The search procedures were constructed to be crude approximations of actual procedures. More important, they provide reasonable bounds on the ability of parties to locate platforms. The procedures themselves are mechanisms for the party to choose the platform it presents to the voters against the incumbent. All three procedures will be discussed primarily within the context of ambitious parties. The extension to ideological parties is straightforward.

The RAPs are the least adaptive of our parties. Letting L represent the length of the campaign, RAPs randomly generate L platforms in the neighborhood of their previous platform and choose the platform that receives the most votes against the incumbent. The RAPs approximate a smoke-filled-room selection process. The party gathers immutable potential candidates and selects the highest vote-getter to represent the party in the election. We do not dispute the contention that this underestimates the ability of parties or candidates to adjust to public opinion. The RAPs are intended as lower bounds on the ability of parties to position themselves.

In contrast to the RAPs, both the CAPs and GAPs selectively refine their platforms to improve vote totals. The CAPs begin with their current platform and experiment, slightly changing positions on a few issues. If the new platform fares better against the incumbent than did the previous one, the party switches to the new platform. These platform tests are called hill-climbing iterations. Here, the campaign length L equals the number of hill-climbing iterations (including those resulting in no improvement) that a party performs before the actual election. The CAPs enter the election with their final—and therefore best-to-date—platform. The CAPs represent parties that select a candidate and then adapt the candidate's platform to the electorate's views by testing alterations with focus groups and speeches. After finitely many refinements, the improved challenger faces the incumbent.

The GAPs, the third type of parties we consider, employ a genetic algorithm to guide their search (see Goldberg 1989; Holland 1975). Instead of adapting a single position, genetic algorithms adapt a population of platforms, attempting to discover nonlinear (epistatic) interactions among variables. Genetic algorithms were designed to work well in "complex" environments—spaces with nonlinearities, discontinuities, noise, and high dimensionality. For our purposes, GAPs provide an indication vis-à-vis the other algorithms of the inherent difficulty of the spatial election problem.

The GAPs represent parties whose potential candidates shift positions both by borrowing from competitors and by testing their own alterations. The genetic algorithm generates new platforms using three procedures. It begins with the random creation of, say, 12 platforms. The first operator, reproduction, randomly selects (with replacement) 12 pairs of candidates from the list and reproduces only the preferred member of the pair. The resulting candidates are then randomly arranged in pairs to which the cross over operator is applied. During crossover, the candidates randomly decide (with probability 50%) whether or not to trade positions on a few issues. If they decide to switch, they exchange groups of positions. Finally, the mutation operator allows each candidate to alter positions randomly on an issue or two.

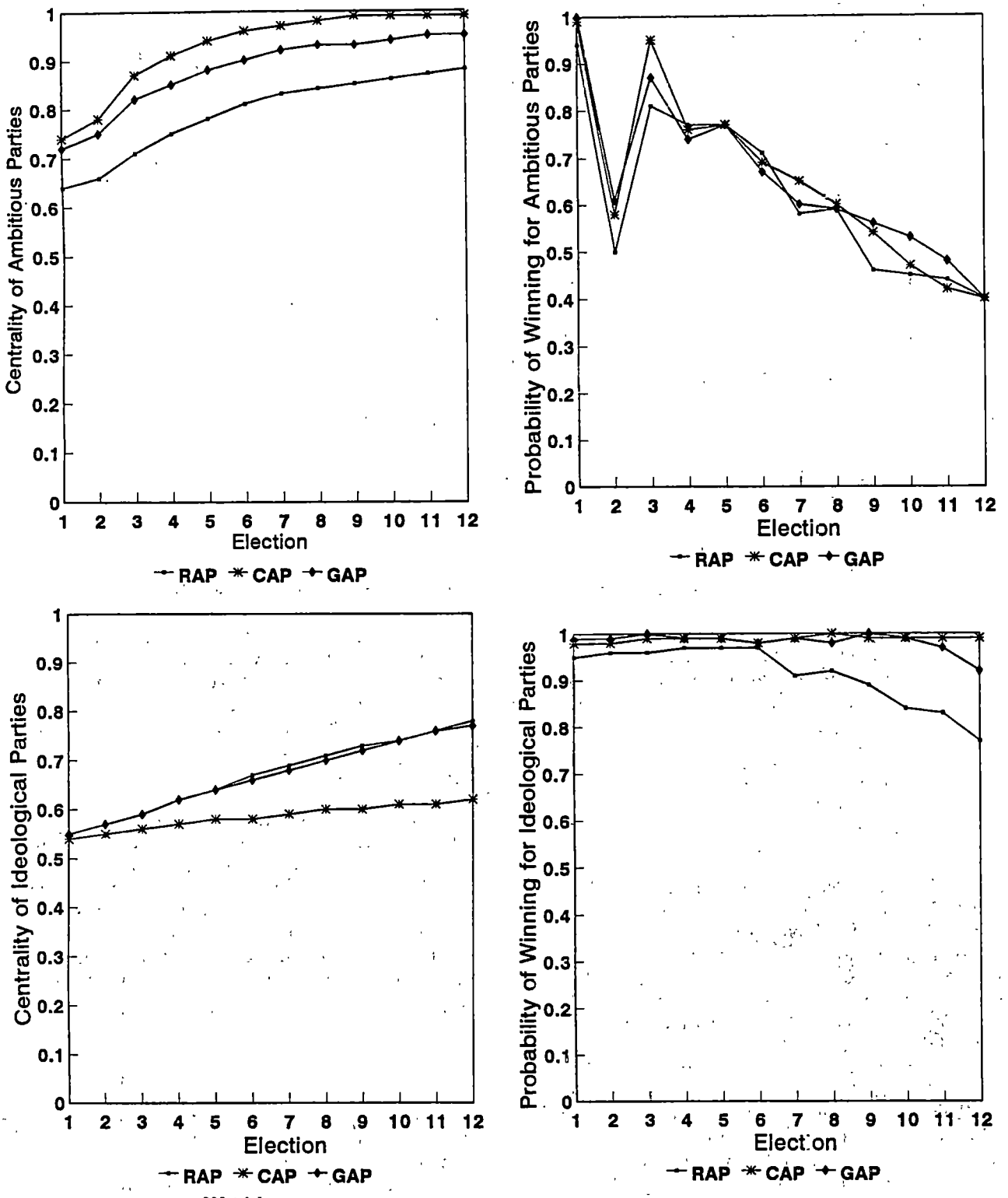
Following biological convention, each application of the reproduction, crossover, and mutation operators is called a *generation*. We count each generation of the genetic algorithm as two units of campaign length, since both crossover and mutation involve candidate platform alterations. At the completion of a campaign of length L, which consists of $L/2$ generations of the genetic algorithm, the party chooses the best-to-date platform.[6]

RESULTS

Several measures are of interest in comparing the different parties and search procedures. Given that our primary concern is the extent to which the distribution of winning party platforms is biased toward regions of high centrality, we record the centrality of winning platforms. We also want to measure the ability of ambitious and ideological parties to defeat the incumbent and to know how far ideological parties must stray from their ideal platforms to do so. Finally, we are concerned with the effect of varying the length of campaigns. The length of campaigns corresponds roughly to the amount of information parties have about voters before an election. Campaigns of length 40 ought to enable parties to compete more effectively against incumbents than campaigns of length 5.

FIGURE 1

Centrality and Success of Adaptive Parties

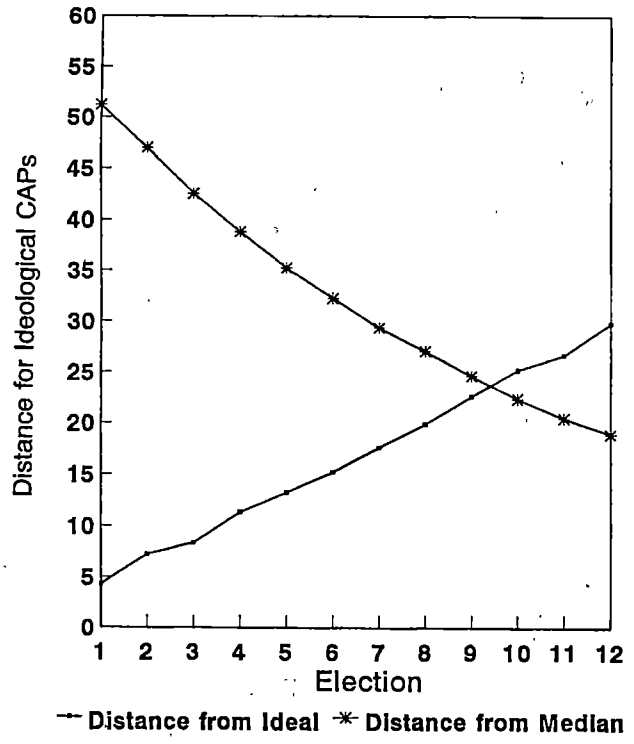


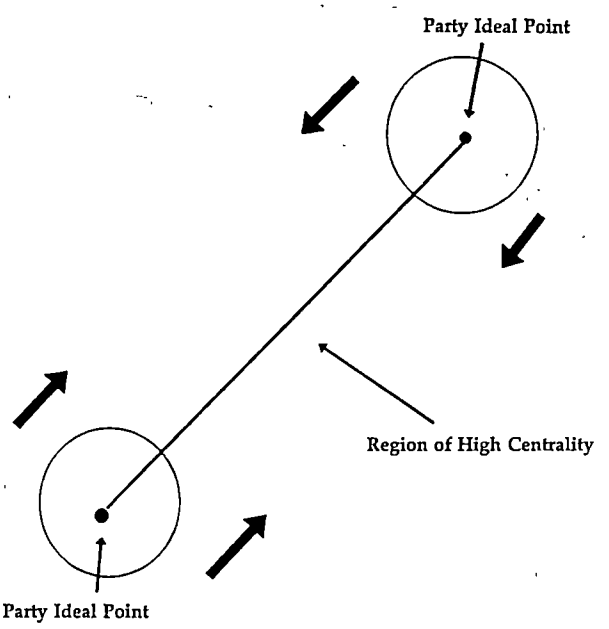
Data represent means over 200 trials

Before presenting our findings, we should note that the robustness of computer simulation results often is sensitive to parameter values. Our findings appear qualitatively invariant to reasonable changes in parameters. We chose parameter values that seem realistic in the study of democratic elections and for

FIGURE 2

Ideological Parties: The Dumbbell Waltz





which a wide range of surrounding values produced similar results. For example, we chose 12 elections because most interesting phenomena were manifest by that time. The following parameters were used:

Voter types (V)	251
Number of issues (n)	15
Positions per issue (k)	7
Strengths (s_{ji})	3
Elections	12
Campaign length (L)	40

All of these fall safely within ranges for which we observed no significant changes in the conclusions.

Centrality values have greater significance when viewed with respect to the distribution of centrality. The Appendix shows numerically estimated distribution and density functions for the centrality of platforms. We compare election outcomes to the cumulative distribution function. Note that a winning party with a platform having a centrality of .55 lies in the upper 17% of the distribution. We ran two hundred trials of a 12-election sequence for each party and type of algorithm. We recorded the centrality of the winning party, the probability that the incumbent would be defeated, and (in the ideological case) the distance to the party's ideal platform.

Ambitious Parties

For all three types of algorithms, ambitious parties moved in directions of higher centrality (see Figure 1). By the sixth election the CAPs and GAPs had average centralities above .9, and the RAPs above .8, which placed all three types of parties in the top .01% of all platforms! It appears that convergence to high centrality and the increase in centrality over time are invariant to the type of search algorithm. As might be expected, the CAPs and GAPs had higher centrality than the RAPs. With respect to the distribution of centrality, however, the differences were not large.

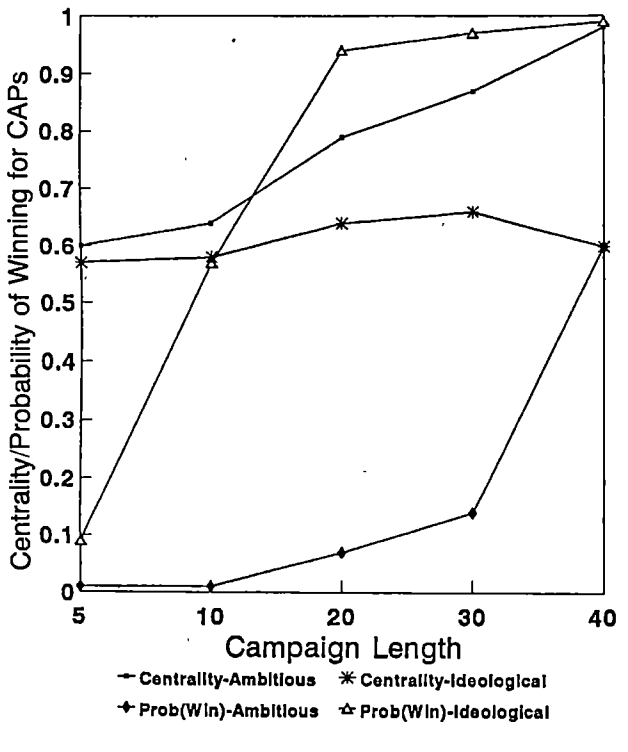
The probability of a challenger's winning decreased from almost 1 in the first election to near .4 by the twelfth election for all three algorithms (see Figure 1).[7] In our model, ambitious challengers have increasing difficulty defeating the incumbent. Incumbency advantage can be attributed to challengers' lack of information, the limitations of adaptive search, and the positions of incumbents' previously adapted platforms.

Ideological Parties

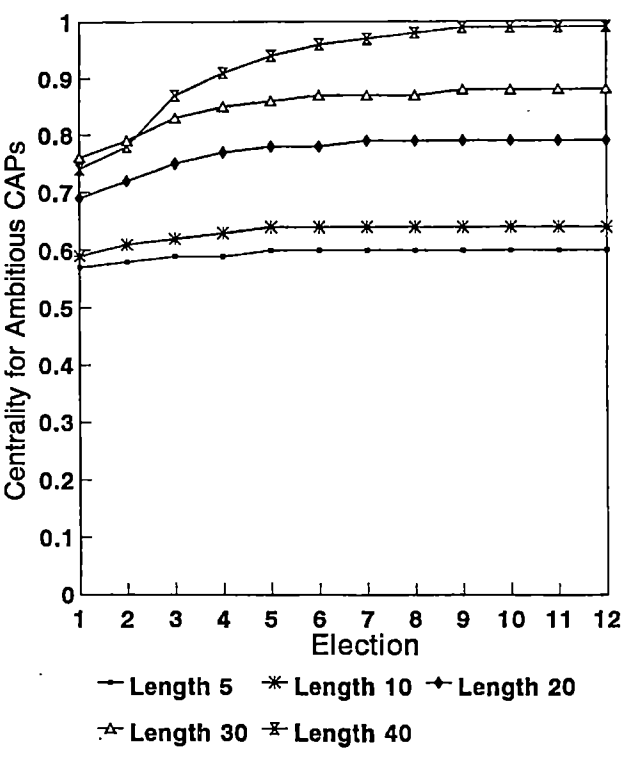
We would expect the centrality of outcomes in elections between ideological parties to be lower than if the parties were solely ambitious. As a consequence, the probability of winning should be much higher in ideological contests. Our results confirmed these expectations. Centralities were lower overall for ideological parties compared with ambitious parties, and there was less variation over elections (Figure 1). Still, all three algorithms were in the top 3% of platforms at the end of 12 elections. Moreover, the probability of

FIGURE 3

Centrality and Success by Campaign Length



200 trials
Values for the 8th election



200 trials

winning was high throughout the sequence of elections, tapering off in the later elections only for the RAPs (Figure 1). Note that the CAPs were not only more likely to win than the RAPs and the GAPs, but they also won with lower centrality. Data reveal the CAPs stayed closer to their ideal points. The CAPs ability to fine tune platforms issue-by-issue generates these results.

Another noteworthy result came from the simulation of ideological parties. We observed that for all three algorithms, the distance to party ideals increased by small amounts, while the distance to the median decreased over time (Figure 2). We refer to this positioning behavior as the *dumbbell waltz*. The challenging party dances in a neighborhood of its ideal platform until it finds a winning platform. This neighborhood slowly converges to areas of high centrality. A chart of winning platforms would consist of two disjoint neighborhoods (one near each of the ideal platforms) and would resemble a dumbbell. The ends of the dumbbell slowly converge as the number of elections increases (Figure 2).

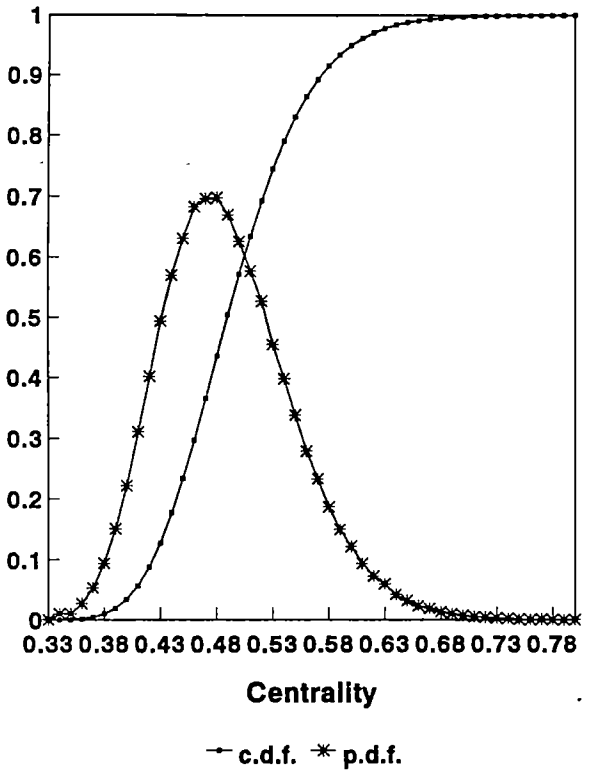
Campaign Length

Increasing the length of the campaign increases the ability of parties to learn about, and adapt to, voters' collective preferences. Figure 3 shows centralities and probabilities of winning for ambitious and ideological CAPs as the length of the campaign increases from 5 to 40. As expected, both centrality and probability of winning tend to increase with campaign length. To give a complete 12-election example, the figure also reveals how increasing campaign length for ambitious CAPs qualitatively increases centrality. For ideological parties, however, centrality varies only slightly with campaign length (see Figure 3) because more informed and intelligent ideological parties are able to locate winning platforms nearer their ideal platforms.

Our results tend to support the idea that boundedly rational parties will converge to central regions of the issue space in a Downsian fashion.[8] Since centrality measures the closeness of parties to voters' ideal points, winning parties with very high centralities can be said to give voters high utility. In this sense, our results suggest that even with fairly simple adaptive parties (e.g., the RAPs), a two-party system should lead to normatively appealing outcomes. Not surprisingly, ambitious parties reach higher centrality than ideological parties. Finally, all three search procedures for both types of parties produced similar outcomes, indicating that there may exist large equivalence classes of adaptive behavior by parties that may allow researchers to undertake a unified analysis. Substantively, our use of three algorithms bol-

FIGURE A-1

Centrality Distribution/Density



Note: 10,000 Monte Carlo trials. c.d.f. = cumulative distribution function; p.d.f. = probability density function

sters the conclusion that departures from the strict informational and rationality assumptions of earlier formal models lead us to convergence results.[9]

DISCUSSION

Rational choice theories of electoral competition have led to two incompatible conclusions. On the one hand, convergence results offer an intuitively appealing explanation for the responsiveness and stability of two-party systems. On the other, chaotic results raise doubts about the legitimacy of democratic outcomes. Both sets of results have relied on strict assumptions of complete information and global optimization. We have relaxed these assumptions about parties and have observed the parties in our model (even ideological ones) converging to central regions of the issue space. Although some mathematical models indicate that democracy might yield arbitrary results with respect to voters' preferences, our model suggests that this typically will not happen with adaptive parties. Moreover, boundedly rational parties may not be able to defeat incumbents. Mathematical proofs of the existence of winning strategies should not be equated with the ability of human actors or organizations to locate them.[10]

Our results, supported by their invariance to parameter choices and search procedures, also help confirm basic intuitions about the role of information and ideologies in elections. As the length of campaigns increases or as parties have more information about voters, parties tend to converge toward centrist outcomes. This observation seems to be consistent with general comparisons of both national and local elections. Extreme candidates rarely emerge as national candidates; and when they do, they lose by a wide margin. Yet at the local level, extremists can thrive. The lack of information among voters has received a great deal of theoretical attention; the lack of information among parties and candidates deserves to be explored, as well.

Our techniques are designed to analyze the behavior of a complex adaptive system, a system in which endogenous aggregate behavior emerges from the knowledge- and rule-based behavior of individual agents. Our model can be extended in many directions, incorporating a variety of components known to exist in the real world. It is possible to include correlated preferences, noisy polls, coadaptive parties, interest groups, issue polling, voters with incomplete information, and multiple parties in future models.[11] With all of these extensions, there is the opportunity to calibrate a simulation to data.

APPENDIX

Figure A-1 shows numerically estimated distribution and density functions for the centrality of platforms.

Notes

Complete data and computer programs are available upon request. We wish to thank Jeffrey Banks, Michael Kirschenheiter, Roger Myerson, Benjamin Page, and Stan Reiter for helpful comments on earlier drafts. The research support of the Center for Mathematical Studies in Economics and Management Science at Northwestern University and the Santa Fe Institute and a grant from Sun Microsystems are gratefully acknowledged.

1. The top cycle set is the smallest set of points whose members defeat all points not in the set. For example, if A defeats B, B defeats C, C defeats A, and each defeats all other points in the space, then {A, B, C} is the top cycle set.

2. By *logical consistency* we mean in the narrow sense of adherence to a rule and not in the broader sense of satisfying mathematical axioms.

3. Central limit theorems and the like are not appropriate given the relatively small number of voters and the size of the space. With 15 issues, 7 positions, and 3 strengths, there are 21^{15} ($\approx 10^{20}$) possible voter types.

4. This construction allows an ideological party to choose a platform that is further from its ideal platform than the incumbent's platform is. However, this only occurs when the distance between party ideal points is improbably small.

5. As an example, in a five-issue, seven-position space, where the current party platform is (6, 2, 5, 4, 1), the party will be much more likely to test the platform (5, 2, 5, 4, 2) than the platform (1, 6, 1, 3, 6), given their relative distances to the current platform.

6. We use a crossover rate of .5; so on average six platform adaptations occur per generation. Therefore, in L/2 generations, the GAPs will test more than L platforms.

7. The dip in the second election results from our random assignment of an incumbent prior to the first election. Half the time, the challenger defeats the incumbent prior to adapting a new platform. After adaptation, the election is a landslide. Subsequently, the exincumbent (now the challenger) has a difficult time winning the second election against such a strong opponent.

8. Kramer (1977), relying on stricter assumptions, has shown that parties will converge toward the minmax set (the set of platforms that lose by a minimal amount) but that the minmax set is not a stable attractor. In other words, parties can leave the minmax set to win once they converge to it. Our results suggest that if movements out of the minmax set occur, they do not, on average, lower centrality.

9. The equivalent performances of the GAPs and the other less sophisticated algorithms also suggest that the underlying search problem is relatively easy.

10. In chess, a finite perfect information game, an optimal strategy exists, yet no one has found it to date.

11. Convergence tends to be robust to changes in the distribution of voters' preferences, although convergence rates may change. For a complete discussion, see Page, Kollman, and Miller (1992).

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REPRESENTATIVE DEMOCRACY AND STATE BUILDING IN THE PROGRESSIVE ERA

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I draw upon state-building and legislative literatures to investigate how constituency-based representative institutions in the Progressive Era nationalized innovative public policies, thereby expanding the authority of the federal government as a component of the modern American state developing at that time. Using state-level referenda votes as measures of grassroots views, multivariate analysis discloses the impact of district opinion, as well as party and district economy, as major determinants of House roll call voting on landmark regulatory legislation authorizing federal intervention in market relationships, state suffrage qualifications, and life-style behaviors involving intoxicating beverages.

The Progressive Era has long been depicted as a responsive "search for order" activated by social, economic, and political problems attending rapid and brutal industrialization at the turn of the century (Burnham 1982, 47; Wiebe 1967). Recently, however, "new institutionalist" approaches point to the expansion of state capacity as the most salient outcome of that search, designating the Progressive Era as a critical time that set parameters for the development of the modern American state (Amenta et al. 1987; Buenker 1977, 57; Ethington n.d.; Orloff 1988; Skocpol 1992; Skowronek 1982).[1]

State capacity grew in two significant ways in the Progressive Era. First, after the turn of the century, more and more observers acknowledged that social and economic ills attributed to an unrestricted capitalistic market system were anything but self-correcting. Consequently, union-backed legislative policies, passed at both state and national levels, thereby expanding the scope of government to include intervention in laissez-faire's hitherto sacrosanct free-contract relationship between business and labor.[2] The power of government regulation also expanded to such new areas as the food and drug industries, as well as development of innovative income tax and fiscal policies (Epstein 1986).[3]

Second, the scope of government authority was not only reconstituted in the Progressive Era at the state and national levels but was transferred from the former to the latter. Previously decentralized state capacities located in dispersed "island communities" became unified and centralized (Wiebe 1967, xiii), producing what Samuel Beer terms a transitional period of dual federalism (1973, 69). How this reconstitution and transference came about, of course, poses one of the more intriguing questions to scholars investigating this period.

Electoral Models of Policy Innovation: Mainsprings and Wellsprings

The realignment thesis provides a powerful explanation accounting for how decentralized electoral opinion can be the "mainspring" of policy innovation at the national level, once it becomes expressed in a cataclysmic, critical election establishing a long-lasting partisan regime shift (Brady 1973, 1984, 1988; Burnham 1970; Key 1955, 1959).[4] Yet this model is not applicable to the 1900–1920 Progressive Era decades, falling (as they do) in the nonrealignment period between the critical election of 1896 and the New Deal.[5]

Others point to the "electoral connection" as the key feature of the American political system (Mayhew 1974) by showing how political elites attend to what we might term the "wellspring" of constituency opinion on an ongoing basis. In this view, citizens continuously have "a form of indirect access to public policy making," exerting pressure both at the point of elections and throughout terms of office (Arnold 1990; Bartels 1991; Ginsberg 1976; Mayhew 1974, 1987; Powell 1982, 658; Walker 1966, 286).[6]

Though scholars typically research the effects of discontinuous realigning elections upon adoption of innovative national policies (Shafer 1991), the state-building effects of continuous electoral processes remain relatively unexplored. Yet in at least one context, we might expect the ongoing electoral connection between local constituents and national elites to have state-building outcomes at the federal level, namely, a stage of policy development in which grassroots issue positions anticipate those to be established at the national level. In such circumstances, national political elites following progressive grassroots policy cues not only represent their constituents but thereby effectively transfer policy innovations from the state level (where they are already established) to the national level. In such a scenario, it is the representative structure of Congress linking it to grassroots constituents as a source of policy innovation that accounts for its institutional role as state-building agent passing legislation which expands federal authority.[7]

The Progressive Era embodies such a context as a time when innovative public policies were widely diffused across states prior to the adoption of similar policies at the national level (Gray 1973; Skocpol

1992). Before 1900, for example, 28 states passed some form of hours laws limiting the working day; and 15 states, including the most industrial, had passed laws specifically regulating hours of work for railroad and streetcar employees. Not surprisingly, in the wake of state-level diffusion of such policies, a comparable program of union-backed labor legislation began to be adopted at the national level after the turn of the century.

Yet while many view our federal system as one in which reform "percolates" up from the local to the state to the national level, few analyze what constitutes the percolation mechanisms.[8] Rather, scholars most often depict innovative public policies established at the national level in nonrealignment periods as a result of national forces alone or partisan sectional cleavages generated solely by economic interests.[9] This project expands such analysis by also assessing how local policy positions influenced the passage of innovative national legislation in the Progressive Era.

POLICY CONGRUENCE AND CONSTITUENCY OPINION

Policy congruence linkages between grassroots constituents and their representatives constitute, for many scholars, the core of democratic theory, empirically defined by the degree of issue concordance between the two. Though the meaning of policy congruence between representatives and their constituents raises complex questions, empirical assessments of how policy opinions of grassroots voters match those of their representatives are relevant to all analyses of representative roles and norms (Bartels 1991, 468, 457; Fiorina 1974; Kingdon 1981).[10] As Robert Weissberg put it succinctly, "Representation means a high correlation between constituency opinion and roll-call voting on a pairwise basis" (1978, 536).

Measuring Constituency Opinion

While roll call votes are readily available as concrete indices of legislators' issue positions in policy congruence research, measurement of constituency issue opinion has proved more vexing (Clausen 1973, 119; Hedlund and Friesema 1974, 399; Jewell 1985; Miller and Stokes 1963, 47). Warren Miller and Donald Stokes (1963) use direct, attitudinal survey measures of constituents' issue preferences in their classic study of policy congruence in the United States in the late 1950s. Philip Converse and Roy Pierce (1986) replicate this design in their study of France in the late 1960s and early 1970s. While both studies provide direct measures of constituency opinion, they also illustrate problems endemic to the use of attitudes as indices of policy positions.[11]

Yet, for historical research, the most seriously limiting feature of attitudinal surveys is the lack of them. The first systematic public opinion polling did not commence until the 1930s, rendering earlier periods, such as the Progressive Era, barren of attitudinal data. Clearly, researchers interested in the early twentieth century must turn elsewhere for direct evidence of grassroots opinion.

Referenda Votes

A technique advocated by contemporary researchers having potent utility for research on historical periods is the use of state-level referenda votes as direct measures of constituency issue positions. In some ways, referenda votes are the more preferred index of constituency opinion. They are discrete, behavioral legislative choices made by grassroots voters on specific policies and thus inherently more comparable to the roll call votes of legislators than the responses to survey questions can be.[12] Furthermore, their use in contemporary and historical studies of state legislative policy outcomes is impressive (Arneson 1927; Campbell 1977; Crane 1960; Dyksira and Hahn 1968; Erikson, Luttbeg, and Holloway 1975; Hedlund and Friesema 1974; Kuklinski 1977, 1978).

More recently, referenda votes have been used to study policy connections operating between state and national levels. L. Marvin Overby (1991), for example, used the returns of state referenda on nuclear freeze policies to show that constituency opinion affected House roll call voting on similar legislation in the 1980s. This resourceful application of referenda votes, nevertheless, failed to aggregate returns by congressional districts, using, instead, only state-level totals.

I use referenda votes, aggregated by congressional districts, as a direct measure of constituency opinion to investigate how policy congruence operated in the Progressive Era between grassroots voters and their representatives in the House. In addition to demonstrating the utility of referenda votes, I push a study of policy congruence back in time to key historical decades, illuminating how policy congruence contributed to the centralization of state authority occurring in the early twentieth century.[13]

DATA AND METHODOLOGY

To develop a measure of district-level opinion on union–labor, woman suffrage, and prohibition policies, I collected as many county-level state votes on referenda and initiatives as I could find by means of an extensive two-year search of state yearbooks found in the libraries of Harvard, Michigan, and Minnesota universities, the Library of Congress, uncoded data files at the University of Michigan Inter-University Consortium for Political and Social Research (ICPSR), and correspondence with many state archive facilities. I expanded the data set for labor to include more states by using state legislature votes as proxies for district referenda returns.[14] All data collected were at the county level, which I recoded to the

district-level on the basis of the *Official Congressional Directory*, which lists congressional districts by House member and county for every Congress.

The resulting data set, which includes most of the country on at least one issue, comprises a measure of grassroots opinion for 244 districts on union–labor issues, 241 districts on woman suffrage, and 101 districts on Prohibition.[15] I pooled local union–labor votes covering a range of issues, such as workmen's compensation, hours laws, the right to picket, minimum number on railroad crews, and minimum wage and conducted several tests to be sure these policies all fell on the same union–labor domain.[16] I also pooled district-level votes on woman suffrage and prohibition and developed a longitudinal measure of woman suffrage to examine how shifts at the district level over time correspond with shifts at the national level resulting in the 1919 passage of the amendment.[17]

District opinion is measured two ways: as a continuous percentage (percent voting in favor of an issue) and as a dichotomous measure defined as "oppose" (0–49% support) and "favor" (50–100% support). The latter is used in logistic regression analysis for scale comparability with other dichotomous variables.[18]

The Sixty-Third (1913–15) and Sixty-Fourth (1915–17) congresses passed landmark labor bills. I chose to analyze legislation that (1) was decided by a roll call vote, (2) had at least a minimal level of opposition (5%), and (3) was clearly labor in content, verified by reference to debates in the *Congressional Record* and historical scholarship.[19] In cases where several votes were taken on the same bill, I chose the votes most closely allied with labor interests, as determined by historical and *Congressional Record* sources. The roll call vote I analyze for the Clayton Act, for example, is the first version of this bill voted on by the House, since this was the version most actively backed by labor organizers, rather than the final compromise bill voted on after it had been sent to the Senate and returned to the House (Link 1956, 427–31). Additional labor legislation chosen for analysis on this basis are the vocational education bill and a labor dispute fund bill voted on in the Sixty-Third Congress.

In the Sixty-Fourth Congress I analyze the landmark vote on the Adamson Eight-Hour Day Act and the vote taken just prior to its passage on a procedural tactic related to its passage (1916); the first, 1916 vote on the Immigration Act requiring a literacy test for entry into the United States, rather than the vote taken in 1917 overriding Wilson's veto;[20] and unemployment legislation. I develop four labor scales out of this total collection of labor votes by cumulatively adding the number of supporting votes cast by House members on each of these scales to determine members' scale scores, as described below.

All House roll call votes on union–labor, woman suffrage, and Prohibition policies are coded at the House level as 0 for *nay* and *paired nay* votes and 1 for *yea* and *paired yea* votes; missing votes are omitted from the analysis. (The use of masculine pronouns when discussing representatives in the Sixty-Third and Sixty-Fourth congresses reflects the fact that no women had been elected to the House at this time.)

I use indices originally developed by Richard Bensel (1984) to code district economy as 1 for core districts ($300 or more value added per capita in manufacturing), 2 for diverse districts ($200–299) and 3 for periphery districts (less than $200).[21] I code party of House member as 1 for Democrats and 0 for Republicans. Policy congruence between House members and their constituents is analyzed using correlational analysis, cross tabulation, and logistic and linear regression analysis.

ANALYSIS AND FINDINGS

Let us begin by examining the impact of district opinion, House partisanship, and district economy on roll call voting on three key legislative policies debated in the Sixty-Third Congress: the Clayton Act, which passed with a strongly supporting vote of 83.4%; the Woman Suffrage Amendment, which failed to garner even a majority vote; and the Prohibition Amendment, which a bare majority in the House supported (52.3%).

Union–Labor: The Clayton Act

Passage of the Clayton Act in 1914 was a jewel of an accomplishment for the new presidential administration of Woodrow Wilson. Hailed by Samuel Gompers as "labor's magna carta" (quoted in Link 1956, 432), this legislation not only put in place moderately strong antitrust regulations but also supported the right of union workers to strike, thereby countering antilabor Supreme Court interpretations of the Sherman Anti-Trust Act (Sanders 1986). Democratic forces in the House backed the Clayton Act to the hilt, going so far as to invoke a binding caucus vote. The result was that over 95% of the Democrats voted for the Clayton Act compared to only 44% of the Republicans, leading some to the obvious conclusion that national-level partisan dynamics backed by executive leadership were the primary causes responsible for its passage.

According to our electoral model of policy innovation, however, constituency opinions on union–labor legislation should also figure prominently as influences determining the outcome of roll call votes on the Clayton Act. With our direct measure of grassroots opinion, we can investigate this possibility. Indeed, as indicated by the logistic regression analysis presented in Table 1 for Model 1, district opinion was clearly a powerful force contributing to the success of the Clayton Act. District opinion *alone* correctly predicts nearly 80% of the roll call votes on the Clayton Act. Even more telling, the addition of partisanship in Model 2 by no means negates the influence of district opinion; it remains virtually as strong a predictor as party. (Logit coefficients are 2.071 and 2.231, respectively, the positive sign for party indicating greater Democratic support.)

TABLE 1

Logit Coefficients for House Support for Clayton Act, Sixty-Third Congress, 1913–15

	HOUSE ROLL CALL VOTE ON CLAYTON ACT[a]		
INDEPENDENT VARIABLES	MODEL 1	MODEL 2	MODEL 3
District opinion[b]	1.896 (.599)**	2.071[c] (1.065)**	2.080 (.998)**
Party of House member[d]	—	2.231 (.333)***	2.268 (.347)***
District economy[e]	—	—	1.230 (.453)**
Intercept	−2.336 (1.155)**	−2.455 (2.048)	−4.208 (.638)**
Correctly predicted (%)	79.41	87.96	90.05

Note: Figures are unstandardized logit coefficients; standard errors are in parentheses.
[a]House roll call votes coded 0 for nay, paired-nay, 1 for yea, paired-yea.
[b]For greater comparability with dichotomous variables, district issue vote coded 1 for 0–49%, 2 for 50–100%.
[c]Significance level calculated by computer program reported at .05 level; calculation based on truncated printed output falls slightly under requisite 1.96 z-score.
[d]House member's party coded 1 for Democrat, 0 for Republican.
[e]District economy coded 1 for core ($300 or more value added per capita in manufacturing), 2 for diverse ($200–$299), 3 for periphery (<$200); codes made available by Elizabeth Sanders.
**$p = .05$.
***$p = .001$.

By adding constituency opinion, therefore, we gain a more complete understanding of how the Clayton Act passed in the House with such an impressive majority. Not only was the Democratic party in control, having won 69% of the seats relative to 32% for the Republicans, but also grassroots opinion was strongly behind union–labor legislation. Support for union policies had diffused widely across states by 1914, reflected in an overall district mean support of 80.66 (McDonagh 1992). It was the duality, therefore, of both broad-based favorable grassroots sentiment and such national forces as the Democratic caucus and executive branch advocacy that produced winning margins for the Clayton Act.

Uncovering the strong relationship between constituency opinion and roll call voting on the partisan-backed Clayton Act raises another intriguing question: How might district opinion be involved in establishing party discipline operating on the passage of this controversial bill? Specifically, could broad-based constituency support for union–labor policies be one reason why Democrats made the Clayton Act a binding caucus vote in the first place?

I cannot investigate directly how constituency opinion may have influenced the Democratic caucus without access to its notes recording the roll call tally on this decision. It is relevant, however, that 87.8% of the Democrats in my sample were elected by districts registering strong support for union policies and that the overall district mean support for union–labor policies was 10 points higher for Democrats than Republicans (83.96 and 73.80, respectively, a statistically significant difference at $p = .05$; see McDonagh 1992).

For most Democrats, therefore, supporting the Clayton Act posed no conflict between constituency and party; rather, the dual congruency with both was most likely mutually reinforcing. It is not unreasonable, therefore, to assume that constituency influence—although empirically most easily measured in terms of its congruence with roll call votes (Kingdon 1981; Overby 1991)—undoubtedly had additional influences on informal decision-making processes responsible for the very visible partisan cleavages characterizing this bill (Holt 1967; Jones 1970; Link 1956).

Elizabeth Sanders (1986) points to the influence of level of industrialization on House roll call voting on the Clayton Act, which contained an antitrust component traditionally important to farmers, as well as the right to strike component appealing to labor interests. She analyzes how a farmer–labor alliance coalesced around its support, reflected in the disproportionate tendency of House members from rural, nonmanufacturing districts to support the measure compared to those who were elected from core districts where manufacturing interests usually electorally dominated those of labor (1986, pp. 144–45).

Looking at the relative impact of district, party, and district economy, therefore, broadens our understanding of influences upon passage of the Clayton Act, as reported in Table 1 for Model 3. District opinion clearly remains a distinct and powerful influence, even with the inclusion of party and district economy. (Logit coefficients are 2.080, 2.268, and 1.230, respectively.) Although we know that some northern Democrats and Republicans representing labor constituencies voted for the Clayton Act (Sanders 1986), the positive sign for district economy indicates that relatively greater support stems, in general, from periphery districts (as we would expect).

Analyzing votes *incongruent* with partisan and/or district economic interests also reveals the impact of district opinion. Republican House members representing core districts generally should be expected to vote against the Clayton Act. Yet, a small minority (19%) cast yea votes, and *all* of these incongruent votes are from Republicans representing districts supportive of progressive labor policies. For some, though not all, Republicans, prolabor constituents overrode both party and economic interests combined.

Similarly, despite their nearly unanimous support of the Clayton Act, a few Democrats voted against it; and these incongruent votes were cast by Democrats representing districts expressing a negative stance on union–labor issues, reflected in the probability of their yea vote dropping from .969 to .798 as we shift from "favorable" to "unfavorable" districts on labor policy. Similarly, Republicans from diverse districts

TABLE 2

Regression Coefficients for House Roll Calls on Labor Scales, Sixty-Third and Sixty-Fourth Congresses, 1913–17

INDEPENDENT VARIABLE	CONGRESS LABOR SCALES[a] SCALE 1 (63D)[b]	SCALE 2 (64TH)[c]	SCALE 3 (63D–64TH)[d]	SCALE 4 (63D–64TH)[e]
District opinion[f]	.604 (.220)**	1.332** (.624)	1.903* (1.109)	1.360** (.447)
Party of House member[g]	1.537*** (.072)	1.659*** (.112)	3.099*** (.168)	1.171*** (.106)
District economy[h]	.031* (.016)	.147*** (.025)	.170*** (.040)	.115*** (.023)
Constant	−.089 (.219)	−.293 (.621)	−.679 (1.112)	−.288 (.442)
Adjusted R^2	.668	.569	.682	.551

Note: Figures are unstandardized regression coefficients; standard errors are in parentheses.
[a]House roll call votes coded 1 for yea and paired-yea, 0 for nay and paired-nay.
[b]Scale 1 is additive number of yea votes cast for Clayton Act, vocational education, and labor dispute fund legislation for House members voting on at least 50% of scale items, including Clayton Act.
[c]Scale 2 is additive number of yea votes cast for two votes on the Adamson Act, immigration restriction, and unemployment legislation for House members voting on at least 50% of scale items, including Adamson Act.
[d]Scale 3 is additive number of yea votes cast for combined scales 1 and 2 items for House members voting on at least 50% of all scaled votes, including at least one of the "Big Three" (Clayton, Adamson, immigration restriction).
[e]Scale 4 is additive number of yea votes cast for Big Three for House members voting on at least Clayton and Adamson Acts.
[f]District Opinion coded 1 of 50%–100% favor union labor legislation, 0 if less than 50% favor.
[g]Party of House member is coded 1 for Democrat, 0 for Republican.
[h]District economy is coded 1 ($300 or more value added manufacturing), 2 for diverse ($200–$299), 3 (<$200); codes made available by Elizabeth Sanders.
*$p = .10$.
**$p = .05$.
***$p = .001$.

opposing labor policies are almost certain to vote nay, while those representing districts supportive of labor are more likely to vote yea.[22] These findings confirm, therefore, the independent influence of district opinion in multivariate models including partisanship and district economy.

Union Labor Scales: The Sixty-Third and Sixty-Fourth Congresses, 1913–17

Policy congruence research often employs scales measuring what Heinz Eulau terms "policy dispositions," rather than discrete roll call votes (Allen, Clausen, and Clubb 1971; Bartels 1991, 462–63; Clausen 1973; Eulau 1987; Olssen 1980). To expand analysis, I constructed four labor scales for the Sixty-Third and Sixty-Fourth Congresses: (1) a scale composed of votes on the Clayton Act, vocational education, and appropriations for a labor dispute fund (Sixty-Third Congress); (2) a scale composed of votes on the Adamson Eight Hour Day Act, passage of the immigration restriction bill, and unemployment insurance legislation (Sixty-Fourth Congress); (3) scales 1 and 2 combined; and (4) a scale of major pieces of legislation passed in either the Sixty-Third or Sixty-Fourth Congress (Clayton, Adamson, and immigration restriction bills).[23] A House member's scale score is the additive number of yea votes cast on a scale.[24]

Analysis of scaled votes show district opinion and party to be the primary determinants of labor support in the Sixty-Third and Sixty-Fourth Congresses (Table 2). Even when scales include a binding party vote (e.g., scales 1 and 3), the impact of district support remains strong. District opinion and party logistic coefficients approach parity as influences accounting for labor support (1.332 and 1.659, respectively), measured by scale 2 for the Sixty-Fourth Congress, again confirming the impact of district opinion for passage of national legislation.

Scales combining labor votes in both the Sixty-Third and Sixty-Fourth congresses (scales 3 and 4) also show the influence of constituency sentiment. District opinion is two-thirds that of party (1.903 and 3.099, respectively) as a predictor of House roll call voting on scale 3; and its impact on scale 4 (combining the Clayton, Adamson, and immigration restriction votes—the "Big Three") slightly exceeds the potency of party (1.360 and 1.171, respectively). Analysis of scaled labor votes, therefore, conforms to the pattern evident for individual labor votes, confirming the effect of district opinion as a significant influence upon House roll call voting on progressive labor policies at the national level.

Woman Suffrage and Prohibition

The woman suffrage issue has a fascinating legislative history (Flexner 1975). First introduced into Congress as the Susan B. Anthony Amendment in the aftermath of the Civil War in 1868, it was not until 1915, in

TABLE 3

Logit Coefficients for House Support for Woman Suffrage and Prohibition, Sixty-Third Congress, 1913–15

VARIABLE	HOUSE ROLL CALL VOTES[a] WOMAN SUFFRAGE	PROHIBITION
Model 1		
District opinion[b]	1.518	1.404
	(.414)***	(.449)**
Intercept	−1.203	−1.943
	(.501)**	(.736)**
Correctly predicted (%)	65.57	67.39
Model 2		
District opinion[b]	1.250	1.784
	(.429)**	(.583)**
Party of House member[c]	−.140	−1.419
	(.159)	(.356)***
District economy[d]	.664	.992
	(.212)**	(.384)**
Intercept	−2.017	−4.259
	(.601)***	(1.336)***
Correctly predicted (%)	65.50	76.47

Note: Figures are unstandardized logit coefficients; standard errors are in parentheses.
[a]Roll call votes coded 0 for nay, paired-nay; 1 for yea and paired-yea.
[b]For comparability with dichotomous variables, district issue vote coded 1 for (0%–49%), 2 for (50%–100%).
[c]Party of House member coded 1 for Democrat, 0 for Republican.
[d]District economy coded 1 for core ($300 or more value added per capita in manufacturing), 2 for diverse ($200–$299), 3 for periphery (<$200); codes made available by Elizabeth Sanders.
**$p = .05$.
***$p = .001$.

the Sixty-Third Congress, that the first roll call vote on this legislation was held in both the House and Senate. Another amendment also came up for consideration, namely, Prohibition. I examine the impact of district opinion as a sole explanatory variable, as well as the relative impact of district opinion, partisanship, and district economy on roll call votes for woman suffrage and Prohibition in Table 3.[25]

At the outset, it is clear that grassroots opinion is strongly related to both woman suffrage and Prohibition. (Logit coefficients are 1.518 and 1.404 and correctly predict 65.57% and 67.39% of the roll call votes, respectively, as reported in Table 3 for Model 1.) In addition, district opinion remains the strongest relative effect when partisanship and district economy are added (Model 2). For woman suffrage, the logit coefficient for district opinion is 1.250 compared to only −.140 and .664 for partisanship and district economy, respectively; and for Prohibition, the corresponding logit coefficients are 1.784 for district opinion compared to −1.419 and .992 for partisanship and district economy, respectively.

On the basis of the logistic regression models reported in Table 3, we can better understand the failure of the Woman Suffrage Amendment to garner even a majority of votes in 1915. District opinion was virtually the only influence and it was decidedly negative on the suffrage issue. The overall district mean level was a low 44.3% (McDonagh 1992). Little wonder that this legislation went down to such a conclusive defeat on its first House roll call vote in 1915.

The Prohibition Amendment deviates somewhat from the woman suffrage pattern (Table 3). Although district opinion is the strongest effect with a logit coefficient of 1.784, party is also a major influence, evident by the logit coefficient of −1.419 (the negative sign indicating greater Republican support). Analysis of this multivariate model, however, also helps account for the failure to generate the two-thirds majority vote necessary for passage of a constitutional amendment. Constituency opinion, the strongest effect, is insufficient for passage. The overall mean district support for prohibition was only 50.8%; and though Republicans were more likely to support prohibition, they were not the majority party in the Sixty-Third Congress, constituting less than a third of the membership compared to Democrats.[26] Without either broad-based district support or backing from the majority party in the House, we can see why this measure also failed in the Sixty-Third Congress.

Nationalizing the Electorate: Woman Suffrage, 1915–19

Though only 47.6% in the House voted to support the woman suffrage amendment in 1915, just four years later this same legislation received a yea vote from over 77% of House members. Though changes in national legislative policy often are attributed to turnover in House membership (Bartels 1991, 458), the intriguing question from the vantage point of this research is the degree to which the dramatic increase in House support between 1915 and 1919 rests upon a concomitant increase in grassroots sentiment over the same period.

House support patterns, indeed, impressively paralleled those at the district level from 1915 to 1919 (Table 4). While only 15.8% of all House districts supported woman suffrage in 1915, four years later, this figure had climbed to a staggering 57.9%, even while the degree of association between district opinion and House roll call voting on woman suffrage remained virtually constant (.533 in 1915 and .526 in 1919).[27] It was not the case, therefore, that the strength of the linkage itself between district opinion and House voting had increased in the intervening years. Rather, it was that suffrage support had diffused broadly across districts between 1915 and 1919, thereby providing a sufficient grassroots base for nationalization of this policy by 1919.

Though the story of how and why horizontal state-level suffrage diffused between 1915 and 1919 begs to be told,[28] our focus here is the vertical connection between the effect of this local diffusion on passage of national legislation. Specifically, how are longitudinal, diffusion patterns of constituency

TABLE 4

Longitudinal House Support for Woman Suffrage, Sixty-Third to Sixty-Sixth Congress, 1915–19

CONGRUENCY AND SUPPORT	SIXTY-THIRD CONGRESS (1915)[a]	SIXTY-SIXTH CONGRESS (1919)[b]
District opinion and House vote[c]	.533[d]***	.526***
House vote congruent with district opinion (%)	67.2	76.5
Districts supporting woman suffrage (%)	15.8	57.9
House vote supporting woman suffrage (%)	47.6	77.3

[a]First roll call vote on woman suffrage.
[b]Final roll call vote on woman suffrage.
[c]District opinion is the percentage voting for woman suffrage; state-level woman suffrage is used when necessary as a proxy for district measure. House roll call vote coded 1 for nay and paired-nay, 2 for yea and paired-yea.
[d]Correlation coefficient (*r*).
***$p = .001$.

support for woman suffrage linked to voting shifts on this issue in the House? To answer this question, I categorized districts as follows: those supporting woman suffrage as early as 1915 are *stable yes* districts; those converting to suffrage support by 1919 are *conversion yes*; and those remaining negative on the issue through 1919 are *stable no* districts.

I categorized House members in a similar way. Those supporting woman suffrage as early as 1915 are *stable yea* members; those converting to a yea vote by 1919 are *conversion yea*; those who remain opposed through 1919 are *stable nay*; and new House members casting a vote for the first time in 1919 are categorized either as *replacement yea* or *replacement nay* members, depending on their voting orientation.

We see clear evidence that longitudinal district patterns on woman suffrage correspond to longitudinal House voting patterns (Table 5). Members with either stable yea or stable nay voting pattern are more influenced by districts also exhibiting stable opinion than they are by conversion districts. (Logit coefficients are 4.346 and 2.398, respectively, as shown in column 1.)[29]

Similarly, House members who converted to a prosuffrage position by 1919 are most effected by conversion districts, as indicated by the size of the logit coefficient of 2.275 in Table 5, column 2. Stable antisuffrage districts had negative effects on House conversion to a prosuffrage stance (as we would expect), indicated by the size of the constant term (−1.625). The lack of cases of House members converting from stable yes districts most likely occurs because House members from such districts vote yea in the first place (column 1), thereby making options to convert to prosuffrage moot.[30]

Turning to replacement members, we see that the greatest effect stems from districts with stable patterns of support, as indicated by the logit coefficient of 9.423.[31] Districts with a longitudinal pattern of stable suffrage support produce yea votes virtually

TABLE 5

Logit Coefficients for Longitudinal House Support for Woman Suffrage, Sixty-Third to Sixty-Fourth Congress, 1913–21

DISTRICT OPINION TYPOLOGY[b]	LONGITUDINAL HOUSE SUPPORT FOR WOMAN SUFFRAGE[a]		
	STABLE MEMBERS[c]	CONVERSION MEMBERS[d]	REPLACEMENT MEMBERS[e]
Stable support district[f]	4.346*** (1.029)	—[g]	9.423 (39.841)
Conversion support district[h]	2.398*** (.368)	2.275*** (.468)	3.297** (1.072)
Intercept[i]	−.476** (.199)	−1.625*** (.303)	.780** (.364)
Correctly predicted (%)	78.40	78.07	89.29

Note: Figures are unstandardized logit coefficients; standard errors are in parentheses.
[a]House roll call votes coded 1 for yea and paired-yea, 0 for nay and paired-nay.
[b]District opinion defined as opposition = 0%–49%, support = 50%–100% State-level woman suffrage used when necessary as proxy for district measure.
[c]Stable House voters are those voting yea or nay consistently, 63d–66th Congresses.
[d]Conversion House voters are those converting from a nay to yea vote by 66th Congress; for analysis, they are grouped with contrasting stable nay House voters.
[e]Replacement House voters are those voting yea or nay for first time in 66th Congress.
[f]Stable support district is one consistently supporting woman suffrage, 1913–19.
[g]Insufficient case for analysis.
[h]Conversion support district is one opposed in 1915 but converting to support for woman suffrage by 1919.
[i]Intercept term is omitted stable opposed district, defined as a district opposed to woman suffrage, 1913–19.
**$p = .05$.
***$p = .001$.

without exception among House replacement members; and (as we would expect) districts more recently converting to a prosuffrage position also have a positive influence, though not quite as large, evident by the logit coefficient of 3.297. We find confirmation, therefore, that longitudinal constituency support patterns—rather than membership turnover alone—best account for House members' voting patterns on woman suffrage in 1919.

CONCLUSION: CONGRESS AND AMERICAN STATE DEVELOPMENT

Woodrow Wilson viewed Congress as the "central and predominant power" of our federal system (1956, 23); and contemporary research on Congress and legislative processes is a thriving academic industry.[32] Attention to how Congress may effect the operation of the larger political system, however, is less typical; and although the institutional "development of Congress" is a vital area of productive scholarship (Polsby 1968; Price 1971, 1975), development of the American political system itself as a function of congressional lawmaking is a much less common research focus.[33]

One reason why Congress as an institution has been less featured than, say, courts, parties, bureaucracies, interest groups, and the presidency is that scholarship on the Progressive Era customarily analyzes key legislative processes horizontally at either the state (Gray 1973; Skocpol 1992; Valelly 1989) or national level but not at the vertical nexus between the two. Yet it is the representative structure of Congress linking decentralized and centralized loci of political power on an ongoing basis that most distinguishes it from other national-level institutions in our constitutional system (Keller 1985, 1).

Drawing attention to vertical connections between grassroots opinion and national policy elites expands our understanding of how representative processes in the context of the Progressive Era contributed to the development of state capacity at the federal level during that period. By assessing the constituency bases of congressional decision making, we see how House members predisposed to vote congruently with their progressive constituents effectively transferred innovative legislation (once it was diffused across a decentralized, local level) to a centralized national one. As a result, Congress passed landmark regulatory legislation authorizing federal intervention in market relationships, state suffrage qualifications, and life-style behaviors involving intoxicating beverages.

Passage of highly partisan, union–labor measures, for example, depended upon widely diffused labor support at the constituency level, in addition to party and economic interests. Constituency opinion also emerged as a major determinant for a variety of House union–labor scales constructed for the Sixty-Third and Sixty-Fourth Congresses, including such pivotal legislative accomplishments as the Clayton Act, the Adamson Eight Hour Day Act, and immigration restriction.

Nationalizing the Electorate

We also discovered that longitudinal patterns of grassroots opinion, rather than merely turnover in House membership alone, were the most decisive influences accounting for the dramatic rise in House support for woman suffrage from 1915 to 1919. In addition to providing a striking example of the power of grassroots opinion for influencing passage of national legislation, the inclusion of civil rights issues in our analysis reveals new insights about the status of the electorate in state-building models. Though most often depicted as a virtually static feature of American political development (Skowronek 1982, 17–18; Skocpol 1988, 362; Skocpol and Ikenberry 1983, 92), the contested legislative status of woman suffrage suggests, instead, that the electorate is best conceptualized not as a condition of political development taking place at this time but, rather, as an object of that development.

Women's contested voting status in the Progressive Era, far from idiosyncratic, typifies legislation of this period wrestling with new definitions of which groups in the American polity are to have access to formal political power in the form of the vote. The major difference is that women succeeded in winning suffrage guarantees. By contrast, African-American men lost their right to vote on a de facto basis in this period, when, by 1908, every southern state had in place disenfranchising voting laws (Burnham 1987, 29; Kousser 1974). Similarly, the voting of immigrants in the urban North was depressed as a de facto result of "good government" legislation establishing registration and voting regulations (McCormick 1979, 295; Piven and Cloward 1988).[34]

Metaphors and Models of Development: Bulldozers, Laboratories or Coffee Pots?

The research presented here, therefore, suggests yet another metaphor (if not a model) to capture the character of the political development taking place in the Progressive Era in the domain of legislative innovation. In one of the most influential explanations of how the modern American state was built, Stephen Skowronek makes the case that it was necessary to dismantle decentralized loci of administrative state capacity in order to establish in their place strong, centralized agencies. In what might be characterized as a "bulldozer" model of political development, national government authority and offices were "*insulated from the people at large*, [and] governmental forms and procedures necessary for securing order in industrial America emerged through a labored exercise in *creative destruction* . . . by an *extended assault* on the previously established governmental order [i.e.,

decentralized state]" (Skowronek 1982, 9, emphasis added).

Though Skowronek may well be correct about the necessity for decentralized destruction to occur in order for the creation of centralized administrative agencies, surely, as we have seen, the same cannot be said of legislative policies adopted at the national level by Congress. Here, it is evident that passage of innovative legislation rests upon broadly based grassroots support. Without that support, national legislation fails (as we saw in the case of woman suffrage and Prohibition in the Sixty-Third Congress). With that constituency support, sweeping national legislative changes are possible—as we saw in the case of passage of union labor bills (where grassroot support was already widely diffused by 1914) and woman suffrage once prosuffrage opinion had spread across sufficient districts.

Another metaphor is needed, therefore, to portray the legislative domain of American political development. Here, the famous dictum of Justice Louis Brandeis comes to mind, in which he memorialized our federal system as a "laboratory" where a "single courageous State may, if its citizens choose, . . . try novel social and economic experiments without risk to the rest of the country" (*New State Ice Company* v. *Liebmann* 1932, 311).

Yet we must ask, How apt is this? Far from merely offering national political elites a range of legislative innovations from which they may pick and choose, findings here show how constituency opinion, once it is broadly diffused, provides proactive constraints and guides influencing the national adoption of legislative alternatives. Local policies, therefore, are not merely *copied* at the national level but *transferred* to the national level by means of representative institutions drawing on grassroots support for those policies; and this transfer had vital state-building ramifications in the context of the Progressive Era, when local opinion favoring the expansion of state capacity was in advance of national reform.

Furthermore, although I have assessed only direct effects of constituency opinion upon House roll call voting, one can speculate about indirect effects of district opinion also likely to be operating (Overby 1991). It is plausible, for example, that the decision of the Democratic caucus to make the Clayton Act a binding vote rested on Democratic constituencies strongly supportive of the union–labor policies. Similarly, Wilson's backing of this bill is best viewed as the tip of a pyramid of power whose base is grassroots support. His switch to a strongly supportive stance on woman suffrage after grassroots opinion was in place illustrates the same point (Snapp 1975).

Finally, there is the well-worn notion that innovation can "percolate" up from the local to the state to the national level in our federal system. Operationalizing the "coffee pot" that makes that percolation possible identifies district opinion as a source of innovative public policies in the Progressive Era and representative institutions as structures providing effective mechanisms for transferring innovation from the local to the national level. As a result, we see Congress in a new light as a proactive institutional agent in the Progressive Era by virtue of its representative structure using progressive grassroots support to expand legislative federal authority as a key component in the development of the modern American state.

Notes

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1. As Stephen Skowronek puts it, the dramatically accelerated expansion of national administration in the 1930s and again in the 1960s "may seem to dwarf the significance of the state-building episode" of the Progressive Era, but the "course of institutional development during these more recent decades and the governmental problems encountered in these developments are rooted in this turn-of-the-century departure" (1982, 14).

2. While some investigate the leadership characteristics of Progressives (Thelen 1969, 331), others point out that Progressives did not seek to change the free-market system but, rather, to restrict only its most insidious perceived consequences—"externalities," in the language of economists (Beer 1973, 64–67).

3. Some viewed the invention of direct democracy instruments of governance (e.g., the referendum, initiative, recall, direct primaries, and direct election of senators) as means for accomplishing innovative legislative goals (Link and Catton 1967).

4. Brady (1984) carefully delineates distinctions between electoral results and structural effects characterizing realignment periods.

5. Though partisan strength and dominance shifted considerably from Democratic to Republican margins in the 1900–1920 period, no long-lasting reconfiguration of partisanship occurred during this period (Allen and Clubb 1974). David Canon (n.d.), however, does credit the gradual increase in the number of Democrats elected over time as responsible for the erosion of Republican hegemony in the early twentieth century, even in the absence of full-scale realignment.

6. Patricia Hurley cites the importance of the "linkage between election results and congressional output" not only "during the rare periods of critical realignment" but also as a more general phenomenon (1979, 46). The possible relationship between constituency opinions measured here and scaled ideological orientations remains an avenue for future work.

7. William Riker describes this process, in the context of passage of constitutional amendments in the Progressive Era, as one that simply "universalized a situation which a majority of state legislatures had already created" (1955, 468). According to his analysis, therefore, "peripheralized federalism"

was used as a way to increase the strength of "centralized federalism" (468–69).

8. An exception is Amenta and his colleagues, who analyze the 1935 Social Security Act in terms of prior, state-level policies (1987, 139).

9. Those pointing to national forces operating in the Progressive Era often note the power of the presidency along with that of the Democratic caucus ruling the House after the overthrow of Speaker ("Czar") Cannon in 1910 (Blum 1954; Clubb 1963; Holt 1967; Jones 1970; Link and Catton 1967, chap. 5; Noble 1984, 1001; Roady 1951; Silbert 1966; Tulis 1987; see Bensel 1984 and Sanders 1986, 1990a, 1990b, n.d. for insightful analyses of how economic, regional interests influence national roll call voting).

10. The "representative bond" between grassroots voters and elected policy elite officials has been the subject of intense theoretical and empirical concern (Converse and Pierce 1986; Erikson 1978, 526; Eulau 1987, 176; Eulau et al. 1959; Eulau and Karps 1977; Haefele 1972; Pitkin 1967); and many feasible models of the representative bond have been formulated, such as descriptive, trustee, and delegate relationships. While empirical congruence is not identical to representation (Eulau 1987), it remains one of the most fundamental analytical axes for studies of democratic, representative processes, despite measurement difficulties (Converse and Pierce 1986, 564; Jewell 1985).

11. Small samples (necessitated by the expense of public opinion surveys) attenuate coefficients statistically due to means error (Achen 1977, 1978). In addition, a basic incommensurability exists between general psychological attitudes held by constituents and the specificity of behavioral roll call voting acts of legislators to which they are compared (Stone 1979).

12. Referenda votes are preferable to attitudes as measures of constituency opinion because they are behavioral, dichotomous, yea/nay acts on specific legislative issues. For this reason, referenda votes inherently have scales more commensurable with legislators' roll call voting decisions than do attitudes, which are more diffuse and usually measured as ordinal, rather than dichotomous, variables. Also, summing referenda votes across a district yields a central tendency measure—percentage of support—that includes information about the variance, or heterogeneity, in the district. Homogeneous districts will have either a very high or a very low percentage of support, while heterogeneous districts will register support at marginal levels around 50%. The central tendency measure used across a district for attitudinal scales, on the other hand, is most often the "mean district attitude score," which contains no information about level of district variance.

13. David Mayhew makes the telling point that political elites, including those active at the state level responsible for referenda, are the determinants of public opinion (1986, 4–5). However, this study focuses on the relationship between public opinion at the local level and national political elites, drawing upon the research of others to explicate the forces responsible for generating grassroots public opinion in the first place.

14. These votes include state constitutional amendments requiring referenda approval, as well as referendum and initiative votes. States lacking legislative activity on an issue necessarily are excluded from this analysis.

15. Some districts voted on several referenda in the same issue area at the same time, and other districts cast votes on several referenda in the same issue area at different points in time. I did a factor analysis of referenda to be sure they were comparable to each other in issue content, as defined by their loading on the same factor. All issue categorizations of referenda were confirmed by factor analysis. I then chose among referenda, selecting for analysis those closest in time and content to selected House roll call votes. Where several referenda in the same issue domain were held at the same time, I chose referenda similar in content across states to form the pooled district-level measure of constituency opinion. Additional information will be provided on request.

16. I checked state *Proceedings* of the American Federation of Labor (AFL) to be sure votes used were supported by labor at the local level. I did a factor analyses within each state to be sure that all votes identified as union-labor were on the same factor. I also regressed the pooled labor vote index and each state (coded as a dummy variable) on House roll call voting on labor issues to determine whether there might be additional, independent effects stemming from particular state cultures. No state exhibited a statistically significant ($p = .10$) independent regression coefficient on House roll call voting on labor issues.

17. State-level passage of suffrage is used where district-level votes are not available. Districts in 1915 opposed to woman suffrage are coded 0, and those supporting woman suffrage are coded 1; the same convention is used for 1919. Comparing codes in 1915 and 1919 yields information about whether a district maintained a stable position (either supporting or opposition suffrage) or converted from opposition to support by 1919. (No districts converted in the other direction.) Following scholarly convention, a major form of woman suffrage is defined as full, presidential, and primary woman suffrage (Keller 1977, 15–18; Banaszak n.d.). Information about specific votes used in this analysis will be provided on request.

18. Since the size of a logistic regression coefficient can be effected by the scale size of the independent variable, it is preferable, when doing this type of analysis, to keep all independent variables on relatively comparable scales. Since *party* is a dichotomous variable, other independent variables used in logistic regression analysis have been scaled with this reference in mind.

19. Using these criteria, some important labor votes are omitted, such as the seamen's bill protecting sailors on the high seas, which passed in the Sixty-Third Congress in 1913 on the basis of a voice vote, and the Workmen's Compensation Act, which passed in the Sixty-Fourth Congress by such wide margins that there is not enough variation in my sample to analyze it.

20. The assumption is that initial sources of support and opposition may become clouded in the process of a legislative showdown with the executive branch.

21. I extend grateful thanks to Elizabeth Sanders for making available for this research district codes for sectional interests.

22. The probability of a yea vote shifts from .128 to .539 respectively.

23. Together, these Congresses present a good test case for assessing policy congruence effects by introducing the dual conditions of high replacement and significant alteration in partisan strength. Replacement voters in the Sixty-Fourth Congress comprise nearly half my sample (43.1%), reflecting the dramatic drop in Democratic strength in the House from 1913 to 1915. Though still the majority party in 1915, Democrats lost 61 seats, while Republicans gained 69, between the Sixty-Third and Sixty-Fourth Congresses. The 13 Progressive Republicans and one Independent elected to the Sixty-Third Congress are omitted from analysis.

24. House members voting on less than half the scale items are omitted from the analysis of that scale. For each of the four scales, at least one of the votes cast had to include a vote on the Clayton, Adamson, or immigration restriction bills (the "Big Three").

25. Scholars often note the association of woman suffrage with welfare issues in the Progressive Era, bolstered by women's identification with maternal, nurturing values and effectiveness in reshaping social reform policies (Cott 1987; Skocpol 1992). To the extent that labor issues drew upon welfare concerns, we might expect district economic indexes related to labor also to be related to voting patterns on woman suffrage.

26. Republican House support for Prohibition most likely stemmed from its association with rural, midwestern identities also often represented by Republican partisan interests (McDonagh and Price 1985, 423–26).

27. In addition, House members in 1915 and 1919 were

inclined to vote congruently with their districts at about the same rates (67.2% and 76.5%, respectively).

28. See Banaszak n.d.

29. The positive logit coefficient of 2.398 for conversion districts reflects the behavior of House members voting yea on suffrage prior to their districts' conversion to support for suffrage. One might say that such House members correctly anticipated a switch in district support for suffrage.

30. The dependent variable in Table 6, column 2, is defined by House members' either converting to a prosuffrage position by 1919 or stable House members' continuing to vote nay by 1919. Of this group, only one House member represents a district with a stable prosuffrage position, precluding analysis of how stable prosuffrage district opinion might effect conversion versus stable opposition House voting patterns.

31. The high standard error for this coefficient most likely stems from the fact that stable districts both for and against woman suffrage registered high rates of yea voting among House replacement members, though the rate of members from stable districts supporting suffrage was relatively much higher than for those representing stable districts opposing suffrage (100% and 68%, respectively).

32. Writing in 1885, Wilson (1956) attributed the power of Congress to a tightly disciplined committee system. The structural reorganization of the House in 1910, however, points more in the direction of a wider range of factors (e.g., partisan organization, grassroots opinion, and the effects of changing economic interests).

33. New institutionalists focusing on Congress typically use organizational models that draw attention to its administrative operation as an institutional structure, rather than electoral models analyzing connective representational linkages between grassroots constituents and national policy elites (Rohde and Shepsle 1978; Shepsle and Weingast 1981; Weingast 1987). For an insightful review and critique of new institutionalism literature, see March and Olsen 1984.

34. Passage of southern disenfranchisement laws begins in the late nineteenth century and is complete by the early years of the Progressive Era. Congress, though well aware of southern intent to subvert the Fifteenth Amendment through such legislation, nonetheless passes no legislation counteracting southern disenfranchisement in the Progressive Era. As late as 1919, southern representatives boasted about their successful disenfranchisement of African-American men's vote without suffering even verbal reprimands (as reported in the *Congressional Record* for the Sixty-Sixth Congress). In J. Morgan Kousser's view, disenfranchisement is a typical "Progressive reform" (1974, 260).

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SENIORITY IN LEGISLATURES

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We construct a stochastic model of a legislature with an endogenously determined seniority system. We model the behavior of the legislators as well as their constituents as an infinitely repeated divide-the-dollar game. The game has a stationary equilibrium with the property that the legislature imposes on itself a non-trivial seniority system, and that incumbent legislators are always reelected.

Why do legislatures have seniority systems? Why do incumbent legislators tend to be reelected by wide margins? These are questions that have engaged legislative scholars for some time.

On the issue of the incumbency advantage, a large empirical literature has advanced a number of explanations for this effect. Jacobson (1983) gives a good review of this literature. The explanations range from the increased access of incumbents to money and the media (see, e.g., Mayhew 1974a), to the effects of gerrymandering (Erikson 1972; Jacobson 1983, 13–15), to the decline of the party system and consequent increased use of incumbency, rather than party, as a voting cue (Ferejohn 1977), to constituency service and expertise built up by veteran legislators (e.g., Fiorina 1977a, 1977b; Mayhew 1974b).

Although the question of incumbency advantage and its relation to legislative organization have received considerable attention in the empirical literature on Congress, we know of no attempt to see if any of these explanations can be derived from a full-equilibrium, dynamic model. All of these explanations of the incumbency effect are nondynamic, partial equilibrium explanations. In other words, it is not clear that all individuals at all points in time are behaving rationally. For example, the explanations of the incumbency effect in terms of money and the media typically do not explain why voters should be swayed repeatedly by advertising and campaign literature. The explanation based on gerrymandering assumes that voters' behavior can be determined by certain socioeconomic characteristics of the voters, such as party identification, race, sex, income and religion. It ignores the possibility that both voters and candidates may have incentives to alter their behavior based on the new district characteristics. The explanation based on the decline of parties has no well worked-out theory as to why voters should use cues such as party or incumbency in the first place. The explanation based on constituency service has some weaknesses when one considers the timing of voter and candidate decisions. For example, why should voters vote for candidates who have done a lot for them in the past if the voters have already collected the rewards of the candidate's behavior? These models are a rich source of ideas; and some of the ideas could undoubtedly be made part of a consistent theory in which all participants are behaving rationally and timing issues are dealt with explicitly. However, this has not yet been done.[1]

From our perspective, the most interesting observation in this literature is that many of these variables are determined endogenously by the legislature. It has been argued persuasively by Mayhew (1974b) and Fiorina (1977b) that *Congress organizes itself to serve the reelection goals of its members.* Thus, the franking privilege, the specialized committee system, the norm of reciprocity, and so on are all seen as ways in which Congress advances the reelection goals of its members. Fiorina has taken this argument to its extreme in his thesis that big government is partially a result of the fact that members of Congress benefit from the increased opportunities to intervene in the bureaucracy on the behalf of their constituents.

We shall consider one particular aspect of legislative organization, namely, the seniority system, and build a theoretical model connecting the seniority system with the reelection goals of the legislators: we formulate a full-equilibrium, dynamic model of policy formation in a representative system in which a seniority system emerges endogenously.[2] We develop a model in which both voters and legislators are acting rationally both on and off the equilibrium path. Voters take into account the fact that their representative is only a member of a legislative body and legislators realize that their actions will affect voters' behavior in subsequent elections. All agents take into account the dynamic effects of all of their actions.

Our approach is to model the representative process as an $\mathcal{L} + n$–player stochastic game, where $\mathcal{L}$ is the number of legislators, and n is the number of voters, partitioned into $\mathcal{L}$ distinct districts. The game alternates back and forth between the voter game and the legislative game. The voter game will consist of a game in which all the voters in each of the $\mathcal{L}$ legislative districts vote to determine who will be their representative for the next legislative session. The legislative game will consist of a game in which the legislators decide whether or not to have a seniority system for the current session and then proceed to select a policy. The policy selected is a decision on a distribution of a fixed amount of money among the legislative districts. We shall model the legislative

game using the approach of Baron and Ferejohn (1989), who consider the legislative game as a form of a Rubinstein bargaining game: a random recognition rule, which depends on seniority, determines the legislator who makes a proposal. The legislators then vote, by majority rule, whether to accept or reject the proposal. The process continues until the legislature accepts a proposal—at which time the legislature adjourns, and new elections are held (i.e., we return to the voter game).

We show that an equilibrium exists in which the legislature always votes to impose on itself a nontrivial seniority system. In the proposal stage, the proposer selects a minimum winning coalition, retaining $(\mathcal{L} + 1)/2\mathcal{L}$ for its own district and allocating $1/\mathcal{L}$ to the districts of the remaining coalition members. Districts that are not part of the winning coalition get nothing. This proposal passes and the game proceeds to the voter game. Voters always reelect incumbents. The intuition behind the results is that voters, understanding the incentives in the legislative game, realize that their representative will be disadvantaged without seniority.

These results contrast with those found in most formal models of voting. Most formal voting models predict tied elections, with no incumbency effects. In our model the incumbent always wins by a unanimous margin. In addition, we have an endogenously chosen seniority system. These two phenomena are related to each other in that the seniority system and the incumbency effect support each other in equilibrium.

It is tempting to interpret the equilibrium of this model as a situation in which legislators blackmail voters to reelect them through the imposition of the seniority system. However, note that that is not exactly what happens in the model. In our model, the legislators cannot commit future legislatures to adopt a seniority system. The future legislature is free to vote against the seniority system if it is not in the interest of the legislators in that legislature to do so. What drives the incumbency effect in our model is the recognition by voters that self-interested legislators with seniority will vote for a seniority system. If a sufficient number of the other legislators have seniority, then it is in the self-interest of a district to make sure that its legislator does also, since the legislature will undoubtedly impose a seniority system. If all voters think this, it becomes a self-fulfilling prophecy.

THE GENERAL FRAMEWORK

Before introducing the model, we develop some general notation for stochastic games. Our model will be a special case of such a general model. Assume that there is a set N of *players*, a set X of *alternatives*, and for each player $i \in N$, a Von Neumann Morgenstern *utility function* u_i: $X \to \mathbb{R}$ over the set of alternatives. We assume that X contains a *null outcome*, x_0 with $u_i(x_0) = 0$ for all $i \in N$. Let $\mathcal{T}$ be a set of *states*. We now define a *stochastic game*, $\Gamma = \{\Gamma^t: t \in \mathcal{T}\}$ to be a collection of *game elements* $\Gamma^t = (S^t, \pi^t, \psi^t)$. Here $S^t = \Pi_{i\in N} S_i^t$ is an n-tuple of *pure strategy sets*. Next π^t: $S^t \to \mathcal{M}(\mathcal{T}) = \Delta^{|\mathcal{T}|}$ is a *transition function* specifying for each $s^t \in S^t$ a probability distribution, $\pi^t(s^t)$ on $\mathcal{T}$, which determines for each $s^t \in S^t$ and $y \in \mathcal{T}$, the probability $\pi^t(s^t)(y)$ of proceeding to game element Γ^y. Finally, ψ^t: $S^t \to X$ is an *outcome function* that specifies for each $s^t \in S^t$ an outcome $\psi^t(s^t) \in X$. We let $S = \Pi_{t\in\mathcal{T}} S^t$ be the collection of pure strategy n-tuples, one for each game element. We write $\Sigma_i^t = \mathcal{M}(S_i^t)$, where $\mathcal{M}(S_i^t)$ is the set of probability distributions over S_i^t, and then define $\Sigma_i = \Pi_{t\in\mathcal{T}}\Sigma_i^t$ to be the set of *stationary strategies* for player i. Elements of Σ are written in the form $\sigma = (\sigma_1, \sigma_2, \ldots, \sigma_n)$. We also use the abusive notation $\sigma^t(s^t) = \Pi_{i\in N}\sigma_i^t(s_i^t)$ and $\sigma(s) = \Pi_{t\in\mathcal{T}}\sigma^t(s^t)$ to represent the probability under σ of choosing the pure strategy profile $s^t \in S^t$ and $s \in S$, respectively.

For stationary strategies, we can define the payoff function M^t: $\Sigma \to \mathbb{R}^n$ by

$$M_i^t(\sigma) = \sum_{\tau=1}^{\infty} \sum_{r\in\mathcal{T}} \pi_\tau^t(\sigma)(r) \cdot u_i(\psi^r(\sigma^r)), \qquad (1)$$

where $\pi_\tau^t(\sigma)(r)$ is defined inductively by

$$\pi_1^t(\sigma)(r) = \pi^t(\sigma^t)(r) = \sum_{s^t\in S^t} \sigma^t(s^t) \cdot \pi^t(s^t)(r)$$

$$\pi_\tau^y(\sigma)(r) = \sum_{y\in Y} \pi_{\tau-1}^y(\sigma)(y) \cdot \pi_1^y(\sigma^t)(r)$$

and $u_i(\psi^t(\sigma^t))$ is defined by

$$u_i(\psi^t(\sigma^t)) = \sum_{s^t\in S^t} \sigma^t(s^t) \cdot u_i(\psi^t(s^t)).$$

Note that this is only well defined if the sum in equation 1 converges for all σ, t, and i.

A strategy n-tuple, $\sigma \in \Sigma$ is said to be a *Nash equilibrium* if $M_i(\sigma_i', \sigma_{-i}) \le M_i(\sigma)$ for all $\sigma_i' \in \Sigma_i$. Applying Bellman's optimality principle (see, e.g., Sobel 1971, theorem 3), it follows that any stationary Nash equilibrium can be characterized by a collection $\{v^t\}_{t\in\mathcal{T}} \subseteq \mathbb{R}^n$ of values for each game element Γ^t and a strategy profile, $\sigma \in \Sigma$ satisfying two properties.

P1. *For all $t \in \mathcal{T}$, σ^t is a Nash equilibrium to the game with payoff function G^t: $\Sigma^t \to \mathbb{R}^n$ defined by*

$$G^t(\sigma^t) = u(\psi^t(\sigma^t)) + \sum_{y\in\mathcal{T}} \pi^t(\sigma^t)(y) \cdot v^y$$

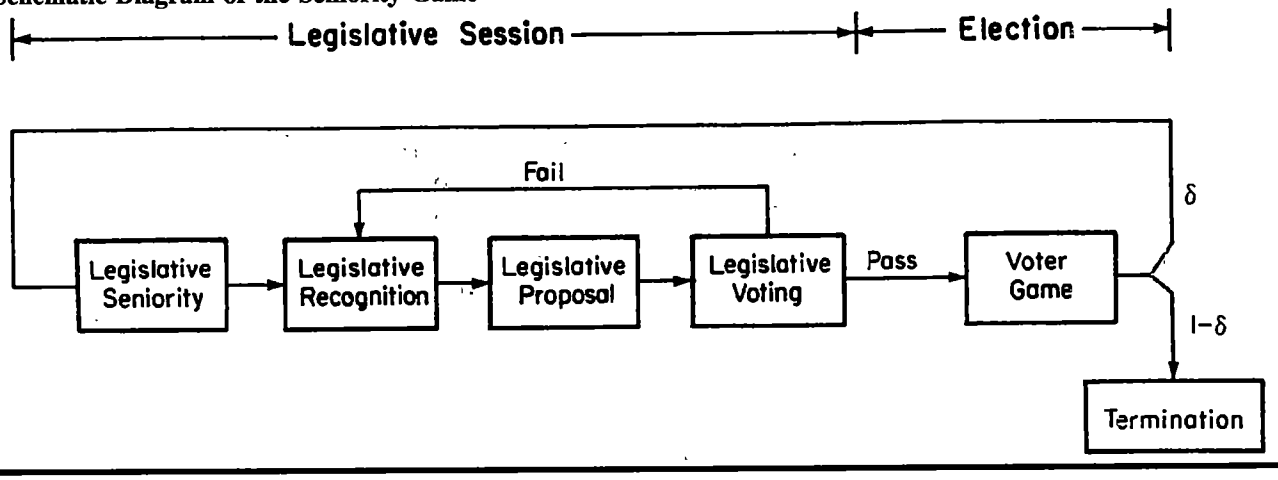
$$= E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y\in\mathcal{T}} \pi^t(s^t)(y) \cdot v^y\right]$$

$$= \sum_{s^t\in S^t}\sigma^t(s^t) \cdot \left[u(\psi^t(s^t)) + \sum_{y\in\mathcal{T}} \pi^t(s^t)(y) \cdot v^y\right].$$

P2. *For all $t \in \mathcal{T}$, $v^t = G^t(\sigma^t)$.*

Figure 1

Schematic Diagram of the Seniority Game



We will use this result to characterize equilibria in the stochastic game we consider. Finally, it also follows from Sobel's (1971) results that a Nash equilibrium in the set of stationary strategies is also a Nash equilibrium in the larger class of nonstationary strategies.

THE GAME

We consider an infinitely repeated game between legislators and their constituents.[3] The game alternates back and forth between a legislative session and an election, as illustrated in Figure 1. In the legislative session, the actors are the legislators (one from each legislative district), and the job of the legislature is to divide a dollar between the legislative districts. The legislative session consists of four parts: a vote on the seniority structure, random recognition of a member, a proposal by the randomly selected member, and a vote on the proposal. In the vote on the seniority structure, if a majority of the legislators vote for a seniority system, a seniority system is imposed; otherwise, there is no seniority system. Next, a random recognition rule, like that of Baron and Ferejohn (1989) is used to select a legislator as a proposer. If no seniority system was passed, all legislators have equal probability of being selected. On the other hand, if a seniority system was passed, then the probability of recognition is an increasing function of i's relative seniority. The proposer then proposes a division of the dollar by legislative district. Finally, the legislature votes on the proposal. If the proposal is defeated, a new proposer is selected and the game continues as before, except that in the second round and thereafter seniority is ignored in selecting the proposer.[4] Once a proposal passes the legislature, the legislative session ends, and the game moves to the election.

In the election, the actors are the voters in the legislative districts. In each district, the voters can choose to reelect their incumbent legislator (in which case the legislator has seniority in the next session and receives a salary c) or not to reelect the incumbent (in which case their legislator receives no salary and goes to the next session with no seniority).[5] After each election the legislative game begins again with the new seniority structure.

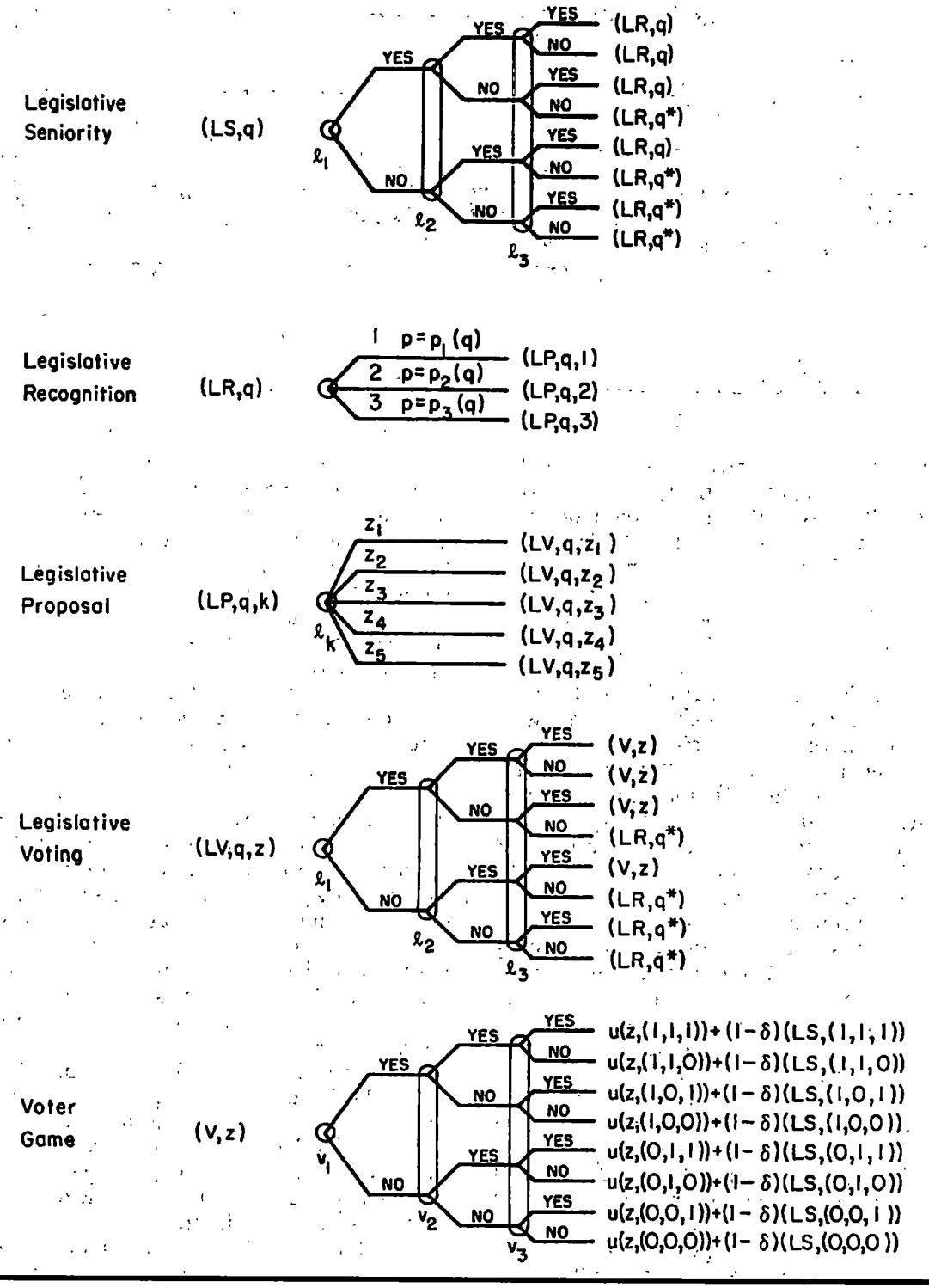
All agents have utility functions that are the discounted present value of their lifetime stream of utility. For the legislators, in each period, payoffs consist of a salary, which depends on whether they are reelected, and a percentage $(1 - \theta)$ of what they secure for their district. Thus, they skim some exogenously given portion of their district's payoff. In each period, the voters get θ times their share of what their legislator is able to secure for the district.

Figure 1 gives a schematic diagram that illustrates the basic components and the sequence of events for the seniority game. We now define the legislative seniority game more formally as a special kind of stochastic game. Each of the aforementioned components will be a game element in the stochastic game. We will specify the strategy sets, transition functions, and outcome functions for each of these game elements. The reader may find it useful to refer to Figure 2 in understanding the following definitions. This figure illustrates the extensive form game tree for each of the game elements in the case in which there are just three legislative districts, one voter per district, and five possible policy choices.

We let $N = L \cup V$, where L is the set of *legislators*, with $\mathcal{L} = |L| \geq 3$ odd, and V is the set of *voters*. We assume that $X' = \Delta^{\mathcal{L}} \times \{0, 1\}^{\mathcal{L}}$, and $X = X' \cup \{x_0\}$. Elements of X' are written in the form $x = (z, q)$, where $z = (z_1, \ldots, z_{\mathcal{L}}) \in Z = \Delta^{\mathcal{L}}$ and $q = (q_1, \ldots, q_{\mathcal{L}}) \in Q = \{0, 1\}^{\mathcal{L}}$. We assume that there is a function $\phi: V \to L$ identifying the legislative districts, such that voter v is in legislator ℓ's district if $\phi(v) = \ell$. We assume that $n_\ell = |\phi^{-1}(\ell)|$ is odd for all $\ell \in L$. We assume that utility functions over X' are of the form $u_i(x) = (1 - \theta)z_i + cq_i$ for $i \in L$, and $u_i(x) = (\theta/n_{\phi(i)})z_{\phi(i)}$ for $i \in V$. So $Q = \{0, 1\}^{\mathcal{L}}$ represents the seniority

FIGURE 2

Extensive Forms for Each Game Element with Three Legislative Districts, and One Voter per District



structure of the legislature, with typical element $q = (q_1, \ldots, q_{\mathcal{L}})$. Thus, $q_i = 1$ indicates that legislator i has seniority, whereas $q_i = 0$ indicates it does not have seniority.

Let $0 < \delta < 1$ be a fixed discount rate and q^* be the element of Q satisfying $q_i^* = 1$ for all i. Let p: $Q \to \Delta^{\mathcal{L}}$ be a function which indicates the proposal power of each legislator as a function of its seniority (if a seniority system is adopted). We assume that p is strictly monotonic in each component: for all $q \in Q$ and $i \in L$, $q_i > q_i' \Rightarrow p_i(q) > p_i(q_i', q_{-i})$ and that $q_i = q_j \Rightarrow p_i(q) = p_j(q)$. Thus, more seniority means a higher

probability that a legislator will be selected as the proposer; and legislators with the same seniority have equal probability of being selected.

There are a total of six game elements, referred to as Termination Game, Legislative Seniority Game, Legislative Recognition Game, Legislative Proposal Game, Legislative Voting Game, and Voter Game. These games are indexed by the corresponding element in the set $\{T, LS, LR, LP, LV, V\}$. Each game element can be further indexed on a state variable. The state spaces for these games are $\emptyset$, Q, Q, $Q \times L$, $Q \times Z$, and Z, respectively. In order to be able to distinguish the difference between the games by means of the state variable we append the identifying element as the first element of the state for each game element. Thus, $\mathcal{T} = (\{T\}) \cup (\{LS\} \times Q) \cup (\{LR\} \times Q) \cup (\{LP\} \times Q \times L) \cup (\{LV\} \times Q \times Z) \cup (\{V\} \times Z)$ is the set of possible states.[6] We now define the strategy sets and transition functions for the game elements.

Termination Game

For $t = T \in \{T\}$,

$$S_i^t = \{0\} \quad \text{if} \quad i \in N.$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(T) = 1$$

and

$$\psi^t(s^t) = x_0.$$

The Termination Game is just a dummy game element that occurs when the game ends. It is indicated by $t = T$. After each election, the stochastic game proceeds to the termination game with probability $1 - \delta$. In the termination game, each agent has a dummy strategy set, consisting of the singelton $\{0\}$; and the transition function π^t specifies that for any strategy choice, we remain in Termination Game with probability 1. In each period, the null outcome occurs, giving all agents utility 0. Hence, once the Termination Game is reached, all agents get 0 ever after. This is a formal way of introducing discounting into the model. In expected value, this makes utility in other parts of the game equal to the discounted current value of the future utility stream, where players discount future payoffs by a factor δ.

Legislative Seniority Game

For $t = (t_0, t_1) \in \{LS\} \times Q$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in L \\ \{0\} & \text{if } i \in N - L. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(LR, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L} s_i^t > \frac{\mathcal{L}}{2},$$

$$\pi^t(s^t)(LR, q^*) = 1 \quad \text{if} \quad \Sigma_{i \in L} s_i^t \leq \frac{\mathcal{L}}{2},$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Seniority Game is the first stage of the legislative session. In this game, the legislature votes whether or not to have seniority for the current session. This game is indexed by $t = (S, t_1)$, where $t_0 = S$ indicates that we are in the Legislative Seniority Game, and t_1 is the current seniority vector. Since this game is a game between legislators, only the legislators have nontrivial strategy sets in the game. The strategy set for a legislator i is the set $\{0, 1\}$, where *0* represents a *no* vote, and *1* represents a *yes* vote. The transition function π^t indicates that we proceed with probability 1 to the Legislative Recognition Game. The vote determines which seniority vector is used in the Legislative Recognition Game. If a majority of the legislators vote for seniority, then the current seniority vector, t_1, is used in the Legislative Recognition Game. If there is not a strict majority for seniority, then the seniority vector q^*, which assigns equal weight to all legislators, is used.

Legislative Recognition Game

For $t = (t_0, t_1) \in \{LR\} \times Q$,

$$S_i^t = \{0\} \quad \text{if} \quad i \in N.$$

For all $s^t \in S^t$ and $y \in L$,

$$\pi^t(s^t)(LP, t_1, y) = p_y(t_1)$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Recognition Game is the second stage of the legislative session. This game is indexed by $t = (LR, t_1)$, where $t_0 = LR$ indicates we are in the Legislative Recognition Game, and t_1 is the current seniority vector. All agents have singleton strategy sets in this game. Thus, this game is just a chance move, which selects the legislator to make a proposal. From this game, we go with probability 1 to the Legislative Proposal Game. The probability that any legislator is selected is determined by the function $p_y(t_1)$. If seniority passed in the Legislative Seniority Game, the argument of p_y is t_1, which is the vector of seniorities of the legislators. If seniority failed, then $t_1 = q^*$ is used for the seniority vector. A legislator is selected by a random recognition rule to make a proposal for consideration by the legislature. This rule is similar to the Baron–Ferejohn recognition rule, except that we let the recognition rule be a function of seniority. Assumptions made above about p_y guarantee that higher seniority leads to higher probability of being selected.

Legislative Proposal Game

For $t = (t_0, t_1, t_2) \in \{LP\} \times Q \times L$,

$$S_i^t = \begin{cases} Z & \text{if } i = t \\ \{0\} & \text{if } i \in N - \{t\}. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(LV, t_1, s_t^t) = 1$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Proposal Game is the third stage of the legislative session. This game is indexed by $t = (LP, t_1, t_2)$, where $t_0 = LP$ indicates that we are in the Legislative Proposal Game; $t_1 \in Q$ is the current seniority structure that has been approved by the legislature; and $t_2 \in L$ represents the legislator who has been selected to make a proposal. In this game element, the only legislator with a nontrivial strategy set is legislator $j = t_2$, whose strategy set is Z, the set of possible divisions of the dollar between the legislative districts. If the legislator proposes the division z, then we proceed with probability 1 to the Legislative Voting Game (LV, t_1, z).

Legislative Voting Game

For $t = (t_0, t_1, t_2) \in \{LV\} \times Q \times Z$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in L \\ \{0\} & \text{if } i \in V. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(V, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t > \frac{\mathcal{L}}{2},$$

$$\pi^t(s^t)(LP, q^*) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t \leq \frac{\mathcal{L}}{2},$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Voting Game is the fourth and last stage of the legislative session. This game is indexed by $t = (LV, t_1, t_2)$, where $t_0 = LV$ indicates that we are in the Legislative Voting Game, $t_1 \in Q$ indicates the current seniority vector, and $t_2 \in Z$ indicates the proposal for division of the dollar that was selected by the proposer in the Legislative Proposal Game. In this game, each legislator has the strategy set $\{0, 1\}$. Here, a zero represents a *no* vote, a one a *yes* vote, on the proposal t_1. If the legislators vote to accept the proposal, the legislative session ends, and we proceed with probability 1 to the Voter Game. If the legislators reject the proposal, then we return with probability 1 to the Legislative Recognition Game, with the exception that seniority is ignored in selecting the proposer. We shall discuss what happens if seniority is used in subsequent proposal rounds. Note that the Legislative Proposal and Legislative Voting Games together are similar to the closed rule version of the Baron–Ferejohn model.

Voter Game

For $t = (t_0, t_1) \in \{V\} \times Z$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in V \\ \{0\} & \text{if } i \in L. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(y) = \begin{cases} \delta & \text{if } y = (S, q(s^t)) \\ 1 - \delta & \text{if } y = T \end{cases}$$

and

$$\psi^t(s^t) = (t_1, q(s^t)),$$

where $q(s^t) = (q_1(s^t), q_2(s^t), \ldots, q_{\mathcal{L}}(s^t)) \in Q$ is defined by

$$q_i(s^t) = \begin{cases} 1 & \text{if } \sum_{j \in \phi^{-1}(i)} s_j^t > \dfrac{n_\ell}{2} \\ 0 & \text{if } \sum_{j \in \phi^{-1}(i)} s_j^t \leq \dfrac{n_\ell}{2} \end{cases}$$

and where $0 < \theta < 1$ and $0 < c$ are constants. The Voter Game consists of a set of simultaneous elections in all of the legislative districts. This game is indexed by $t = (V, t_1)$, where $t_0 = V$ indicates that we are in the Voter Game, and $t_1 \in Z$ represents the outcome of the Legislative Voting Game. In each legislative district, the voters of that district vote whether or not to reelect their legislator. In the version of the game as it is presented here, there is only one legislator in each district and no challenger. So the effect of a negative vote in a given district is that the legislator from that district does not get a salary for the next period and loses seniority.

This completes the description of the stochastic game. Note that there are no payoffs except in the voter game. At that point policy $x = (t_1, q(s^t))$ is implemented. Thus, the pie is divided up among the districts according to $z = t_1 \in \Delta^{\mathcal{L}}$, and $q(s^t) \in Q$ determines which legislators get reelected and which do not. Given the utility functions we have specified, it follows that the output $t_{1\ell}$ to district ℓ is first divided up with $\theta t_{1\ell}$ actually delivered to the voters and $(1 - \theta)t_{1\ell}$ being skimmed off by legislator ℓ. The voters each get an even share of the delivered output. The legislators, in addition to their share of the output, get a salary that is dependent on whether they are reelected or not.

Figure 2 presents a schematic depiction of the game for the case of three legislators, three voters (one per

district), and five possible proposals. The Legislative Seniority vote determines the probabilities in the Legislative Recognition Game. A proposer is selected who makes a proposal in the Legislative Proposal Game. The legislature votes on this proposal. If it passes, the game proceeds to the Voter Game and a new election is held leading to a new session of the legislature. (The discounting comes in here.) If the proposal fails, the game goes back to the Legislative Recognition Game, with seniority not used to determine the probabilities of recognition. From here, the game proceeds as before.

RESULTS

Our main result is that there exists an equilibrium to the stochastic game in which a seniority system is adopted and incumbent legislators always get reelected.

PROPOSITION 1. *The following is a stationary equilibrium to the stochastic game defined in the previous section:*

1. Legislative Seniority Game. For $t \in \{LS\} \times Q$, *and* $i \in L$,

$$\sigma_i^t(t_{1i}) = 1$$

2. Legislative Proposal Game. For $t \in \{LP\} \times Q \times L$,

$$\sigma_i^t = \frac{1}{|\Omega_t|} \sum_{w \in \Omega_t} \delta_{z_i(w)},$$

where $\Omega_t = \{\omega \in \{0, 1\}^{\mathcal{L}}: \Sigma_i \omega_i = (\mathcal{L} + 1)/2, \omega_t = 1\}$, δ_x *is the Dirac delta at* x, *and* z_t: $\Omega_t \to \mathbb{R}^{\mathcal{L}}$ *is defined by*

$$z_{ti}(\omega) = \begin{cases} \dfrac{\mathcal{L}+1}{2\mathcal{L}} & \text{if } i = t \\ \dfrac{1}{\mathcal{L}} & \text{if } i \neq t,\ \omega_i = 1 \\ 0 & \text{otherwise.} \end{cases}$$

3. Legislative Voting Game. For $t \in \{LV\} \times Q \times Z$, *and* $i \in L$,

$$\sigma_i^t(1) = \begin{cases} 1 & \text{if } t_{1i} \geq \dfrac{1}{\mathcal{L}} \\ 0 & \text{if } t_{1i} < \dfrac{1}{\mathcal{L}}. \end{cases}$$

4. Voter Game. For $t \in \{V\} \times Z$, *and* $i \in V$,

$$\sigma_i^t(1) = 1.$$

Proof. The formal proof is in the Appendix. The proof is straightforward, and simply consists of verifying that these strategies satisfy the Bellman conditions that characterize a Nash equilibrium of a stochastic game. These conditions are given as conditions P1 and P2. To verify this, we first specify values, v^t, for each game element. These values represent the value of the game element if equilibrium behavior is followed forever after. The Bellman conditions require that if we replace all game elements by their values, then for each game element, fixing the values of the other game elements, the specified strategies are a Nash equilibrium for that game element (P1), and the computed value of each game element at the equilibrium strategies is equal to the specified value (P2). While the proof is straightforward, it is tedious, since each of these conditions must be verified for all of the six game elements.

Remarks. The proposition gives equilibrium strategies for both the legislators and voters in the stochastic game. We first describe the equilibrium and then provide some intuition for the result. In the Legislative Seniority Game all legislators who have seniority vote in favor of the seniority system, and those who do not have seniority vote against the seniority system. Since in equilibrium all legislators get reelected the, seniority system always passes.

In the Legislative Proposal game, the proposer selects a minimal winning coalition of legislators, which includes itself. The proposer retains $(\mathcal{L} + 1)/2\mathcal{L}$ for its own district, leaving $1/\mathcal{L}$ to be allocated to the districts of each of the remaining members of the coalition. Districts that are not a part of the winning coalition are allocated 0. Thus, the proposer obtains a premium of $(\mathcal{L} + 1)/2\mathcal{L} - 1/\mathcal{L} = (\mathcal{L} - 1)/2\mathcal{L}$ due to its proposal power. As $\mathcal{L} \to \infty$, the premium increases with a limit of one-half.

In the Legislative Voting Game, a legislator votes for a proposal if and only if the legislator receives at least $1/\mathcal{L}$. Thus, if the proposer has proposed an equilibrium proposal, it will pass. Finally, in the Voter Game, the voters always vote to reelect their legislators. It should be noted that although the proof shows only that this is a Nash equilibrium for the voters, in fact, the strategy of voting for the incumbent is a dominant strategy for the voters in any given legislative district, taking the behavior in the remainder of the game as fixed.

Several features of this equilibrium stand in sharp contrast to the results that come out of the traditional voting literature. Most voting models predict tied elections, with no incumbency effects. Here, we obtain instead equilibrium behavior by the voters in which the incumbent wins by a large (unanimous) margin. The intuition behind the result is simple: the voters know that in equilibrium the seniority system will pass and thus that it is in the their best interest to reelect the incumbent, since a senior legislator will be more easily able to serve the constituency than a junior legislator. Note that voters do not know that there will be a seniority system in the next session

but, rather, know that in the steady-state equilibrium, seniority will be voted in each session.

Finally, we emphasize that we have only identified one equilibrium of this model. There may be other equilibria. In fact, we believe that there would also be an equilibrium in which seniority is rejected by the legislature and all legislators are defeated for reelection. This later equilibrium may be able to be refined away by appropriate equilibrium refinements, since voters would all be indifferent, in equilibrium, between voting for the incumbent and voting for the challenger.

EXTENSIONS

The Power of the Initial Proposal

In our model the seniority system works through the proposal stage, by influencing the probability that legislators will get chosen to be the proposer. We assumed that seniority is only used for selecting the proposer in the first round of any legislative session. Thus, if a proposal is turned down in the legislature, seniority is no longer used to select the proposer during that legislative session. In a companion paper we analyze the case in which seniority is in effect throughout the legislative session (McKelvey and Riezman (n.d.). One might think that this system (which, on its face, gives more power to the senior members) would make them better off and hence would be selected by them. However, we show that the opposite is the case. When seniority is in effect for the entire session, there is no symmetric stationary equilibrium in which senior and nonsenior legislators have different values and in which voters always reelect incumbents. In other words, when seniority is used throughout the session, there is no equilibrium in which seniority has benefits for legislators. Thus, legislators would be indifferent between having, and not having, such a seniority system and would hence prefer a seniority system in which seniority is used only for the first proposal in each legislative session. Thus, we get the rather paradoxical result that legislators who have seniority would choose a seniority system that on its face gives less power to senior members.

It is worth pointing out that a seniority system that gives only initial proposal power is a realistic description of the seniority system for the U.S. Congress in the sense that seniority is embodied in the committee system. The committees make proposals by sending bills to the floor. Once the bills go to the floor, the committees lose most of their power, since bills that are amended or defeated generally do not go back to committee in that session. Hence, our model might explain certain features about how seniority systems are set up—in particular, the importance of initial proposal power.

We briefly describe the result from the companion paper. The game is altered by changing the Legislative Voting Game so that when a proposal is rejected, the subsequent Legislative Recognition Game will use the original seniority vector. The rest of the stochastic game is as before. In other words, the Legislative Voting Game is

The Revised Legislative Voting Game

For $t \in \{LV\} \times Z$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in L \\ \{0\} & \text{if } i \in V. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(V, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t > \frac{\mathcal{L}}{2},$$

$$\pi^t(s^t)(LR, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t \leq \frac{\mathcal{L}}{2},$$

and

$$\psi^t(s^t) = x_0.$$

Consider the stochastic game of the previous section, substituting the above game for the previous Legislative Voting Game. Call this the Revised Game. The following proposition is proved in McKelvey and Riezman (n.d.).

PROPOSITION 2. *In the Revised Game there is no symmetric stationary equilibrium with the following two properties:*

1. *Voters always reelect incumbents:*

   For $t \in \{V\} \times Z$, and $i \in V$: $\sigma_i^t(1) = 1$ for all i.

2. *The value of senior and nonsenior members in the Legislative Seniority Game is different.*

The intuition behind this result has to do with how proposers choose coalition partners. Once chosen, proposers want to include in the coalition those with the lowest continuation values, because they can be given less and will still vote for the proposal. It follows, then, when seniority is used throughout the legislative session, that if seniority benefits senior members, they will be less likely to be included in coalitions. Proposition 2 shows that this effect of being chosen less often swamps any other advantages of using seniority throughout the session. Thus, once the proposer is chosen, senior members want to look like nonsenior members so they are as likely to be included in the coalition.

Ordinal Seniority

Our model assumes a binary seniority system: a legislator either has seniority or does not. The next step is to introduce the possibility of ordinal seniority, where a legislator's seniority measures the number of times the legislator has been reelected. The congressional seniority system is an ordinal system.

If each legislator has an independent probability of dying or retiring, then at any point in time, the distribution of seniority, even in equilibrium, could be heterogeneous. Depending on the distribution of seniority and exactly how seniority affects the probability of being selected as the proposer, a seniority system would not necessarily be adopted by the legislature in the first place. Hence, with ordinal seniority, it appears that seniority system may occasionally be defeated. We plan to explore this issue in future work.

CONCLUSION

We have developed a formal model of voter behavior and legislative decision making in which the seniority system and the incumbency effects emerge as an equilibrium. There are a number of weaknesses in this model. We have assumed an unrealistically simple model of the legislative session and of how seniority plays a role; that is, the legislative session is characterized by a random recognition voting game similar to the Baron–Ferejohn model. Second, we assume that the only decision made by the legislature is a decision on the division of a fixed pie. We also assume that legislators' preferences are a function of how much they get for their constituents, rather than just whether they are reelected. We hope to remedy some of these weaknesses in the future. Despite these obvious weaknesses of the model, the model illustrates that it is possible to construct consistent formal models that connect legislative organization with reelectoral goals of legislators. Within the context of this model we have shown that when the effect of seniority is to change the probability of recognition on the first round, seniority has value. If seniority changes the probability of recognition on every round, then senior members get no benefits from seniority.

APPENDIX: PROOF OF PROPOSITION 1

We first specify the values, v^t, associated with these strategies. We then verify that for these values, property P1 and P2 are satisfied. For the following equations, we set $w_1 = (\mathcal{L} + 1)/2\mathcal{L}$, and $w_2 = 1/\mathcal{L}$. Also, we define $Z^1 = \{z \in Z: |\{j \in L: z_j \geq w_2\}| > \mathcal{L}/2\}$, *and* $Z^0 = Z - Z^1$. Similarly, define $Q^1 = \{q \in Q: |\{i \in L: q_i = 1\}| > \mathcal{L}/2\}$, and $Q^0 = Q - Q^1$.

The values of the games are defined below. To interpret these values, go to the definitions of the individual games. For example, for $t \in \{LS\} \times Q$, you are in the Legislative Seniority Game. $v_i^t = v_i^{(R, t_1)}$ means that the value of the seniority game given that seniority has passed ($t_1 \in Q^1$) is the value in the Legislative Recognition Game with seniority vector t_1. If seniority does not pass ($t_1 \in Q^0$), then the value of the game is given by the value in the Legislative Recognition Game with seniority vector q^*. Other values are defined in a similar way.

For $t \in \{T\}$,

$$v_i^t = 0 \text{ for all } i \in N.$$

For $t \in \{LS\} \times Q$,

$$v_i^t = v_i^{(R, t_1)} \text{ if } t_1 \in Q^1,$$

$$v_i^t = v_i^{(R, q^*)} \text{ if } t_1 \in Q^0.$$

For $t \in \{LR\} \times Q$,

$$v_i^t = (1 - \theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)w_2\right] + c + \delta v_i^* \quad \text{if } i \in L,$$

$$v_i^t = \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{\phi(i)\}} p_y(t_1)w_2\right] + \delta v_i^* \quad \text{if } i \in V,$$

where

$$v_i^* = v_i^{(R, q^*)} = \begin{cases} \dfrac{1}{1 - \delta}\left[\dfrac{1}{\mathcal{L}}(1 - \theta) + c\right] & \text{if } i \in L, \\ \dfrac{1}{1 - \delta}\left[\dfrac{\theta}{\mathcal{L} n_{\phi(i)}}\right] & \text{if } i \in V. \end{cases}$$

For $t \in \{LP\} \times Q \times L$,

$$v_i^t = (1 - \theta)w_1 + c + \delta v_i^* \quad \text{if } i = t,$$

$$v_i^t = \frac{1}{2}(1 - \theta)w_2 + c + \delta v_i^* \quad \text{if } i \neq t,$$

$$v_i^t = \frac{\theta}{n_{\phi(i)}} w_1 + \delta v_i^* \quad \text{if } \phi(i) = t,$$

$$v_i^t = \frac{1}{2}\frac{\theta}{n_{\phi(i)}} w_2 + \delta v_i^* \quad \text{if } \phi(i) \neq t.$$

For $t \in \{LV\} \times Q \times Z^1$,

$$v_i^t = (1 - \theta)t_{1i} + c + \delta v_i^* \quad \text{if } i \in L,$$

$$v_i^t = \theta t_{1\phi(i)} + \delta v_i^* \quad \text{if } i \in V.$$

For $t \in \{LV\} \times Q \times Z^0$,

$$v_i^t = v_i^* \quad \text{if } i \in N.$$

For $t \in \{V\} \times Z$,

$$v_i^t = (1 - \theta)t_{1i} + c + \delta v_i^* \quad \text{if } i \in L,$$

$$v_i^t = \frac{\theta}{n_{\phi(i)}} t_{1\phi(i)} + \delta v_i^* \quad \text{if } i \in V.$$

The next step in the proof is to verify property P2, which requires that for each game and each player the payoffs correspond to the values we have specified. To do this, we start with the definition of G and

then, using the definitions of the game elements and the equilibrium strategies, show that the payoffs equal the appropriate values.
For $t \in \{T\}$,

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v^y\right]$$
$$= u(x_0) + \pi^t(\sigma^t)(t)v^t = v^t.$$

For $t \in \{LS\} \times Q$,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_i^y\right]$$
$$= u_i(\psi^t(t_1)) + \sum_{y \in \mathcal{T}} \pi^t(t_1)(y)v_i^y$$
$$= \begin{cases} v_i^{(LR,\, t_1)} & \text{if} \quad \sum_{i \in L} t_{1i} > \dfrac{\mathcal{L}}{2} \\ v_i^{(LR,\, q^*)} & \text{if} \quad \sum_{i \in L} t_{1i} \le \dfrac{\mathcal{L}}{2} \end{cases}$$
$$= \begin{cases} v_i^{(LR,\, t_1)} & \text{if} \quad |\{i \in L\colon t_{1i} = 1\}| > \dfrac{\mathcal{L}}{2} \\ v_i^{(LR,\, q^*)} & \text{if} \quad |\{i \in L\colon t_{1i} = 1\}| \le \dfrac{\mathcal{L}}{2} \end{cases}$$
$$= \begin{cases} v_i^{(LR,\, t_1)} & \text{if} \quad t_1 \in Q^1 \\ v_i^{(LR,\, q^*)} & \text{if} \quad t_1 \in Q^0. \end{cases}$$
$$= v^t.$$

For $t \in \{LR\} \times Q$,

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(\sigma^t)) + \sum_{y \in \mathcal{T}} \pi^t(\sigma^t)(y)v^y\right]$$
$$= u(x_0) + \sum_{y \in L} p_y(t_1)v^y.$$

So, for $i \in L$,

$$G_i^t(\sigma^t) = p_i(t_1)v_i^i + \sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)v_i^y$$
$$= p_i(t_1)[(1-\theta)w_1 + c + \delta v_i^*]$$
$$+ \sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)\left[\frac{1}{2}(1-\theta)w_2 + c + \delta v_i^*\right]$$
$$= (1-\theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)w_2\right]$$
$$+ c + \delta v_i^* = v_i^t$$

and for $i \in V$,

$$G_i^t(\sigma^t) = p_{\phi(i)}(t_1)v_i^{\phi(i)} + \sum_{y \in \mathcal{L} - \{\phi(i)\}} p_y(t_1)v_i^y$$
$$= p_{\phi(i)}(t_1)\left[\frac{\theta}{n_{\phi(i)}}w_1 + \delta v_i^*\right]$$
$$+ \sum_{y \in \mathcal{L} - \{\phi(i)\}} p_y(t_1)\left[\frac{\theta}{2n_{\phi(i)}}w_2 + \delta v_i^*\right]$$
$$= \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(t_1)w_1 + \sum_{y \in \mathcal{L} - \{\phi(i)\}} \frac{1}{2}p_y(t_1)w_2\right]$$
$$+ \delta v_i^* = v_i^t.$$

For $t \in \{LP\} \times Q \times L$,

$$G^t(\sigma^t) = E_{\sigma^t}[u(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v^y]$$
$$= u(x_0) + E_{\sigma^t}[v^{(LV,\, s_t^t)}].$$

But since $\sigma_t^t(Z^1) = 1$, we have, for $i \in L$,

$$G^t(\sigma^t) = E_{\sigma^t}[(1-\theta)s_t^t + c + \delta v^*]$$
$$= (1-\theta)E_{\sigma^t}[s_t^t] + c + \delta v^*.$$

But

$$E_{\sigma^t}[s_t^t] = E_{\sigma^t}\left[\frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} \delta_{z_t(w)}\right] = \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} E_{\sigma^t}[\delta_{z_t(w)}]$$
$$= \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} z_t(\omega).$$

So

$$E_{\sigma^t}[s_{ti}^t] = \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} z_{ti}(\omega) = \frac{1}{|\Omega_t|} \cdot |\Omega_t| \frac{\mathcal{L}+1}{2\mathcal{L}}$$
$$= \frac{\mathcal{L}+1}{2\mathcal{L}} = w_1 \text{ if } i = t$$

$$E_{\sigma^t}[s_{ti}^t] = \frac{2\left[\dfrac{\mathcal{L}-1}{2}\right]!}{(\mathcal{L}-1)!} \cdot \frac{(\mathcal{L}-2)!}{2\left[\dfrac{\mathcal{L}-3}{2}\right]!} \cdot \frac{1}{\mathcal{L}}$$
$$= \frac{\left[\dfrac{\mathcal{L}-1}{2}\right]}{(\mathcal{L}-1)} \cdot \frac{1}{\mathcal{L}} = \frac{1}{2\mathcal{L}} = \frac{1}{2}w_2 \quad \text{if} \quad i \neq t.$$

Thus,

$$G_i^t(\sigma^t) = \begin{cases} (1-\theta)w_1 + c + \delta v_i^* & \text{if} \quad i = t \\ \dfrac{1}{2}(1-\theta)w_2 + c + \delta v_i^* & \text{if} \quad i \neq t \end{cases}$$
$$= v_i^t$$

and for $i \in V$,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[\frac{\theta}{n_{\phi(i)}} s_{ti}^t + \delta v_i^*\right] = \frac{\theta}{n_{\phi(i)}} E_{\sigma^t}[s_{ti}^t] + \delta v_i^*$$

$$= \begin{cases} \dfrac{\theta}{n_{\phi(i)}} w_1 + \delta v_i^* & \text{if} \quad \phi(i) = t \\ \dfrac{1}{2}\dfrac{\theta}{n_{\phi(i)}} w_2 + \delta v_i^* & \text{if} \quad \phi(i) \neq t \end{cases}$$

$$= v_i^t.$$

For $t \in \{LV\} \times Q \times Z^1$, since $t_1 \in Z^1$, it follows that

$$\sigma^t\left(\sum_{i \in L} s_i^t > \frac{\mathcal{L}}{2}\right) = 1.$$

So $\pi^t(\sigma^t)(V, t_1) = 1$. Hence,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y) v_i^y\right]$$

$$= u(x_0) + \pi^t(\sigma^t)(t_1, V) v_i^{(V, t_1)} + \pi^t(\sigma^t)$$

$$(q^*, 2) v_i^{(LR, q^*)}$$

$$= v_i^{(V, t_1)} = \begin{cases} (1-\theta) t_{1i} + c + \delta v_i^* & \text{if} \quad i \in L \\ \theta t_{1\phi(i)} + \delta v_i^* & \text{if} \quad i \in V \end{cases}$$

$$= v_i^t.$$

For $t \in \{LV\} \times Q \times Z^0$, since $t_1 \in Z^0$, it follows that

$$\sigma^t\left(\sum_{i \in L} s_i^t \leq \mathcal{L}/2\right) = 1,$$

so $\pi^t(\sigma^t)(q^*, 2) = 1$. Hence,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y) v_i^y\right]$$

$$= u(x_0) + \pi^t(\sigma^t)(t_1, V) v_i^{(V, t_1)}$$

$$+ \pi^t(\sigma^t)(q^*, 2) v_i^{(LR, q^*)}$$

$$= v_i^{(LR, q^*)} = v_i^* = v_i^t.$$

For $t \in \{V\} \times Z$,

$$\pi^t(s^t)(y) = \begin{cases} \delta & \text{if} \quad y = (S, q(s^t)) \\ 1 - \delta & \text{if} \quad y = T, \end{cases}$$

$\sigma_i^t(1) = 1$ for all i.

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y) v^y\right]$$

$$= E_{\sigma^t}[u(t_1, q(s^t))] + \delta \pi^t(\sigma^t)(q(s^t), 1) v^{(S, q(s^t))}$$

$$= u(t_1, q^*) + \delta v^{(S, q^*)} = u(t_1, q^*) + \delta v^*$$

So, for $i \in L$,

$$G_i^t(\sigma^t) = (1-\theta) t_{1i} + c q_i^* + \delta v_i^*$$

$$= (1-\theta) t_{1i} + c + \delta v_i^* = v_i^t$$

and for $i \in V$,

$$G_i^t(\sigma^t) = \frac{\theta}{n_{\phi(i)}} t_{1\phi(i)} + \delta v_i^* = v_i^t.$$

We next verify that property P1 is satisfied, that is, that σ^t is a Nash equilibrium. For each game element we show that no player can benefit from playing a different strategy.

For $t \in \{V\} \times Z$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, where $\sigma_i^t(1) = 1$ for all $i \in V$. It suffices to show that for each $i \in V$, σ_i^t is at least as good as any pure strategy $s_i' \in S_i^t$. So,

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y) v_i^y\right]$$

$$\geq E_{\sigma_{-i}^t}\left[u_i(\psi^t(s_i', s_{-i}^t)) + \sum_{y \in \mathcal{T}} \pi^t(s_i', s_{-i}^t)(y) v_i^y\right]$$

for all $s_i' \in S_i^t$. Writing $\underline{1}$ for the $|V|$ component vector of ones, we can rewrite this inequality as

$$u_i(\psi^t(\underline{1})) + \sum_{y \in \mathcal{T}} \pi^t(\underline{1})(y) v_i^y$$

$$\geq u_i(\psi^t(s_i', \underline{1}_{-i})) + \sum_{y \in \mathcal{T}} \pi^t(s_i', \underline{1}_{-i})(y) v_i^y$$

$$\Leftrightarrow \delta v_i^{(S, q(\underline{1}))} \geq \delta v_i^{(S, q(s_i', \underline{1}_{-i}))} \Leftrightarrow v_i^* \geq v_i^{(S, q(s_i', \underline{1}_{-i}))}$$

But $q(s_i', \underline{1}_{-i}) \geq \underline{1} - \varepsilon_\ell$, where $\ell = \phi(i)$, and ε_ℓ is the ℓth standard basis vector. Further, since $\underline{1} - \varepsilon_\ell \in Q^1$, we have $q(s_i', \underline{1}_{-i}) \in Q^1$. So $v_i^{(S, q(s_i', \underline{1}_{-i}))} = v_i^{(LR, q(s_i', \underline{1}_{-i}))}$. Hence, the above inequality can be written

$$v_i^* \geq \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(q(s_i', \underline{1}_{-i})) w_1 + \frac{1}{2} \sum_{y \in \mathcal{L} - \{\phi(i)\}} p_y(q(s_i', \underline{1}_{-i})) w_2\right] + \delta v^*{}_i$$

$$\Leftrightarrow \frac{\theta}{\mathcal{L} n_{\phi(i)}} \geq \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(q(s_i', \underline{1}_{-i})) w_1 + \frac{1}{2}(1 - p_{\phi(i)}(q(s_i', \underline{1}_{-i}))) w_2\right]$$

$$\Leftrightarrow \frac{1}{\mathcal{L}} = p_i(q_0) w_1 + \frac{1}{2}(1 - p_i(q_0)) w_2$$

$$\geq p_i(q(s_i', \underline{1}_{-i}))w_1 + \frac{1}{2}(1 - p_i(q(s_i', \underline{1}_{-i})))w_2$$

$$\Leftrightarrow [p_i(q_0) - p_i(q(s_i', s_{-i}))]\left(w_1 - \frac{1}{2}w_2\right) \geq 0.$$

Now if $n_{\phi(i)} = |\phi^{-1}(\phi(i))| > 1$ (there is more than one voter in district i), then $q_0 = q(s_i', \underline{1}_{-i})$; thus, one voter changing a vote does not affect the outcome. Hence, the above expression equals 0, and it follows that σ^t is a Nash equilibrium for G^t. If $n_{\phi(i)} = 1$ (there is a single voter in district i), then since $\sigma_i^t(1) = 1$ and $s_i' \leq 1$, it follows that $q_{0i} = \underline{1}$ and $q_i(s_i', \underline{1}_{-i}) = s_i' \leq 1$. In this case, one voter changing a vote changes the outcome. Hence, monotonicity of p implies that $[p_i(q_0) - p_i(q(s_i', \underline{1}_{-i}))] \geq 0$, and the last inequality holds if and only if

$$\left[w_1 - \frac{1}{2}w_2\right] = \frac{\mathcal{L}+1}{2\mathcal{L}} - \frac{1}{2\mathcal{L}} = \frac{1}{2} \geq 0.$$

Since all terms in the last expression are positive, this inequality holds; and it follows that s^t is a Nash equilibrium for G^t. This demonstrates that from the voter's point of view, changing a vote either does not change the outcome or changes the outcome in such a way as to make that voter worse off.

For $t \in \{LS\} \times Q^1$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, where $\sigma_i^t(t_{1i}) = 1$ for all $i \in L$. It suffices to show that for each $i \in L$, σ_i^t is at least as good as any pure strategy $s_i' \in S_i^t$. So,

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow E_{\sigma^t}[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_i^y]$$

$$\geq E_{\sigma_{-i}^t}[u_i(\psi^t(s_i', s_{-i}^t)) + \sum_{y \in \mathcal{T}} \pi^t(s_i', s_{-i}^t)(y)v_i^y]$$

for all $s_i' \in S_i^t$. Using $\sigma_i^t(t_{1i}) = 1$, this can be reduced to

$$u_i(\psi^t(t_1)) + \sum_{y \in \mathcal{T}} \pi^t(t_1)(y)v_i^y \geq u_i(\psi^t(s_i', (t_1)_{-i})) + \sum_{y \in \mathcal{T}} \pi^t(s_i', (t_1)_{-i})(y)v_i^y.$$

Since $t_1 \in Q^1$, it follows that $\Sigma_{j \in L} t_{1j} > \mathcal{L}/2 \Rightarrow \pi^t(t_1)(LR, t_1) = 1$. So we get

$$v_i^{(LR, t_1)} \geq \pi^t(s_i', (t_1)_{-i})(LR, t_1)v_i^{(LR, t_1)} + \pi^t(s_i', (t_1)_{-i})(LR, q^*)v_i^{(LR, q^*)}.$$

Clearly, if $\pi^t(s_i', (t_1)_{-i})(LR, t_1) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', (t_1)_{-i})(LR, t_1) \neq 1$. In this case, we must have $\Sigma_{j \in L} t_{1j} > \mathcal{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}} t_{1j} < \mathcal{L}/2$. So $t_{1i} = 1$, and $s_i' = 0$. Thus, $\pi^t(s_i', (t_1)_{-i})(LR, q^*) = 1$, and the above inequality can be rewritten

$$v_i^{(LR, t_1)} \geq v_i^{(r, q^*)}$$

$$\Leftrightarrow (1 - \theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)w_2\right] + c + \delta v_i^*$$

$$\geq (1 - \theta)\left[q_i^* w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} q_y^* w_2\right] + c + \delta v_i^*$$

$$\Leftrightarrow p_i(t_1)w_1 + \frac{1}{2}(1 - p_i(t_1))w_2 \geq q_i^* w_1 + \frac{1}{2}(1 - q_i^*)w_2$$

$$\Leftrightarrow p_i(t_1)\left(w_1 - \frac{1}{2}w_2\right) \geq q_i^*\left(w_1 - \frac{1}{2}w_2\right)$$

$$\Leftrightarrow p_i(t_1) \geq q_i^*.$$

Now $p_i(t_1)$ is the probability that i will be selected, given that seniority is used and that $(\mathcal{L} + 1)/2$ members (including i) have seniority and $(\mathcal{L} - 1)/2$ do not have seniority. The seniority assumption implies that for all $q \in Q$, and $i, j \in L$, $q_i > q_i' \Rightarrow p_j(q) < p_j(q_i', q_{-i})$, $i \neq j$; that is, higher (lower) seniority for legislator i means that every other legislator now has a lower (higher) probability of being selected than the proposer. Now begin at q^* (assume every legislator has seniority) and remove seniority for $(\mathcal{L} - 1)/2$ legislators (not including i). At each step, p_i increases. Therefore, the last inequality is satisfied. Hence, σ^t is a Nash equilibrium for G^t.

For $t \in \{LS\} \times Q^0$, as above, we have

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$u_i(\psi^t(t_1)) + \sum_{y \in \mathcal{T}} \pi^t(t_1)(y)v_i^y \geq u_i(\psi^t(s_i', (t_1)_{-i})) + \sum_{y \in \mathcal{T}} \pi^t(s_i', (t_1)_{-i})(y)v_i^y$$

for all $s_i' \in S_i^t$. Since $t_1 \in Q^0$, it follows that $\Sigma_{j \in L} t_{1j} \leq \mathcal{L}/2 \Rightarrow \pi^t(t_1)(LR, q^*) = 1$. So we get

$$\Leftrightarrow v_i^{(LR, q^*)} \geq \pi^t(s_i', (t_1)_{-i})(LR, t_1)v_i^{(LR, t_1)} + \pi^t(s_i', (t_1)_{-i})(LR, q^*)v_i^{(LR, q^*)}.$$

Clearly, if $\pi^t(s_i', (t_1)_{-i})(LR, q^*) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', (t_1)_{-i})(LR, q^*)) \neq 1$. In this case, we must have $\Sigma_{j \in L} t_{1j} \leq \mathcal{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}} t_{1j} > \mathcal{L}/2$. So $t_{1i} = 0$, and $s_i' = 1$. Thus, $\pi^t(s_i', t_{1-i})(LR, t_1) = 1$, and the above inequality can be rewritten

$$v_i^{(LR, q^*)} \geq v_i^{(LR, t_1)}$$

$$\Leftrightarrow (1 - \theta)\left[q_i^* w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} q_y^* w_2\right] + c + \delta v_i^*$$

$$\geq (1 - \theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)w_2\right] + c + \delta v_i^*$$

$$\Leftrightarrow q_i^* w_1 + \frac{1}{2}(1 - q_i^*)w_2 \geq p_i(t_1)w_1 + \frac{1}{2}(1 - p_i(t_1))w_2$$

$$\Leftrightarrow q_i^*\left(w_1 - \frac{1}{2}w_2\right) \geq p_i(t_1)\left(w_1 - \frac{1}{2}w_2\right)$$

$$\Leftrightarrow q_i^* \geq p_i(t_1).$$

But $p_i(t_1)$ is the probability that i will be selected, given that seniority is used and that $(\mathcal{L} - 1)/2$ members have seniority and $(\mathcal{L} - 1)/2$ (including i) do not have seniority. Using reasoning similar to that above, begin at q^* (assume no legislator has seniority) and add seniority for $(\mathcal{L} - 1)/2$ legislators (not including i). At each step, p_i decreases. Therefore, the last inequality is satisfied. Hence, σ^t is a Nash equilibrium for G^t.

For $t \in \{LV\} \times Q \times Z^1$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, where

$$\sigma_i^t(1) = \begin{cases} 1 & \text{if} \quad t_{1i} \geq \dfrac{1}{\mathcal{L}} \\ 0 & \text{if} \quad t_{1i} < \dfrac{1}{\mathcal{L}} \end{cases}$$

for all $i \in L$. It suffices to show that for each $i \in L$, σ_i^t is at least as good as any pure strategy $s_i' \in S_i^t$. So,

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow E_{\sigma^t}[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_i^y]$$

$$\geq E_{\sigma_i'}[u_i(\psi^t(s_i', s_{-i}^t)) + \sum_{y \in \mathcal{T}} \pi^t(s_i', s_{-i}^t)(y)v_i^y]$$

for all $s_i' \in S_i^t$. Since $t_1 \in Z^1$, $|\{j \in L: t_{1j} \geq w_2\}| > \mathcal{L}/2$. But $\sigma_i^t(1) = 1$ if $t_{1i} \geq 1/\mathcal{L} = w_2$. So, define $r \in \{0, 1\}^{\mathcal{L}}$ by $r_i = 1$ if $t_{1i} \geq w_2$ and $r_i = 0$ if $t_{1i} < w_2$. Then $\sigma(r) = 1$ and $\sigma_{-i}(r_{-i}) = 1$. Since $\Sigma_{i \in L} r_i > \mathcal{L}/2$, $\pi^t(r)(t_1, V) = 1$, and the above equation can be reduced to

$$u_i(x_0) + \sum_{y \in \mathcal{T}} \pi^t(r)(y)v_i^y \geq u_i(x_0) + \sum_{y \in \mathcal{T}} \pi^t(s_i', r_{-i})(y)v_i^y$$

$$\Leftrightarrow \sum_{y \in \mathcal{T}} \pi^t(r)(y)v_i^y \geq \sum_{y \in \mathcal{T}} \pi^t(s_i', r_{-i})(y)v_i^y$$

$$\Leftrightarrow v_i^{(V, t_1)} \geq \pi^t(s_i', r_{-i})(V, t_1)v_i^{(V, t_1)} + \pi^t(s_i', r_{-i})(LR, q^*)v_i^{(LR, q^*)}.$$

Clearly, if $\pi^t(s_i', r_{-i})(V, t_1) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', r_{-i})(V, t_1) \neq 1$. In this case, we must have $\Sigma_{j \in L} r_j > \mathcal{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}} r_j < \mathcal{L}/2$. So $r_i = 1$, and $s_i' = 0$. Thus, $\pi^t(s_i', r_{-i})(LR, q^*) = 1$, and the above inequality can be rewritten

$$v_i^{(V, t_1)} \geq v_i^{(LR, q^*)} \Leftrightarrow (1 - \theta)t_{1i} + c + \delta v_i^* \geq v_i^*$$

$$\Leftrightarrow t_{1i} \geq \frac{1}{(1 - \theta)}[(1 - \delta)v_i^* - c].$$

$$\Leftrightarrow t_{1i} \geq \frac{1}{(1 - \theta)}\left[\left[\frac{1}{\mathcal{L}}(1 - \theta) + c\right] - c\right].$$

$$\Leftrightarrow t_{1i} \geq \frac{1}{\mathcal{L}}.$$

But $s_i^t = 1 \Rightarrow t_{1i} \geq w_2 = 1/\mathcal{L}$. Hence, the above inequality holds; and we have shown that $G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$, so σ^t is a Nash equilibrium for G^t.

For $t \in \{LV\} \times Q \times Z^0$, define r as above. Since $t_1 \in Z^0$, $|\{j \in L: t_{1j} \geq w_2\}| \leq \mathcal{L}/2$, we get $\Sigma_{i \in L} r_i \leq \mathcal{L}/2$, implying $\pi^t(r)(LR, q^*) = 1$. Then arguing as above,

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow v_i^{(LR, q^*)} \geq \pi^t(s_i', r_{-1})(V, t_1)v_i^{(V, t_1)} + \pi^t(s_i', r_{-i})(LR, q^*)v_i^{(LR, q^*)}$$

for all $s_i' \in S_i^t$. Clearly, if $\pi^t(s_i', r_{-i})(LR, q^*) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', r_{-i})(LR, q^*) \neq 1$. In this case, we must have $\Sigma_{j \in L} r_j < \mathcal{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}} r_j > \mathcal{L}/2$. So $r_i = 0$, and $s_i' = 1$. Thus, $\pi^t(s_i', r_{-i})(V, t_1) = 1$, and the above inequality can be rewritten

$$v_i^{(LR, q^*)} \geq v_i^{(V, t_1)} \Leftrightarrow v_i^* \geq (1 - \theta)t_{1i} + c + \delta v_i^*$$

$$\Leftrightarrow \frac{1}{(1 - \theta)}[(1 - \delta)v_i^* - c] \geq t_{1i}.$$

$$\Leftrightarrow \frac{1}{(1 - \theta)}\left[\left[\frac{1}{\mathcal{L}}(1 - \theta) + c\right] - c\right] \geq t_{1i}.$$

$$\Leftrightarrow \frac{1}{\mathcal{L}} \geq t_{1i}.$$

But $s_i^t = 0 \Rightarrow t_{1i} < w_2 = 1/\mathcal{L}$. Hence, the above inequality holds; and it follows that σ^t is a Nash equilibrium for G^t.

For $t \in \{LP\} \times Q \times L$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, in which

$$\sigma_t^t = \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} \delta_{z_t(w)},$$

where $\Omega_t = \{\omega \in \{0, 1\}^{\mathcal{L}}: \Sigma_i \omega_i = (\mathcal{L} + 1)/2, \omega_t = 1\}$ and $z_t: \Omega_t \to \mathbb{R}^{\mathcal{L}}$ is defined by

$$z_{ti}(\omega) = \begin{cases} \dfrac{\mathcal{L} + 1}{2\mathcal{L}} & \text{if} \quad i = t \\ \dfrac{1}{\mathcal{L}} & \text{if} \quad i \neq 1,\ \omega_i = 1 \\ 0 & \text{otherwise} \end{cases}$$

for all $i \in L$. It suffices to show that σ_t^t is at least as good as any pure strategy $s_t' \in S_t^t$. So, for all $s_t' \in S_t^t$,

$$G_t^t(\sigma^t) \geq G_t^t(\sigma_t', \sigma_{-t}^t)$$

$$\Leftrightarrow E_{\sigma^t}[u_t(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_t^y]$$

$$\geq E_{\sigma_{-t}'}[u_t(\psi^t(s_t', s_{-t}^t)) + \sum_{y \in \mathcal{T}} \pi^t(s_t', s_{-t}^t)(y)v_t^y]$$

$$\Leftrightarrow u_t(x_0) + \frac{1}{|\Omega_t|} \sum_{w \in \Omega_t} \sum_{y \in \mathcal{T}} \pi^t(z_t(\omega), \underline{0}_{-t})(y)v_t^y$$

$$\geq u_t(x_0) + \sum_{y \in \mathcal{T}} \pi^t(s_t', \underline{0}_{-t})(y)v_t^y$$

$$\Leftrightarrow \frac{1}{|\Omega_t|} \sum_{w \in \Omega_t} v_t^{(LV, z_t(\omega))} \geq v_t^{(LV, s_t')}.$$

But now, for all $\omega, \omega' \in \Omega_t$, $z_{tt}(\omega) = z_{tt}(\omega')$. So, writing $z_{tt} = z_{tt}(\omega)$, then the above inequality becomes

$$\frac{1}{|\Omega_t|} \sum_{w \in \Omega_t} v_t^{(LV, z_t(\omega))} = (1 - \theta)z_{tt} + c + \delta v_t^* \geq v_t^{(LV, s_t')}$$

Now if $s_t' \in Z^0$, which means that the proposal will not pass, then $v_t^{(LV, s_t')} = v_t^*$. So the above inequality becomes

$$(1 - \theta)\frac{\mathcal{L} + 1}{2\mathcal{L}} + c \geq (1 - \delta)v_t^* = (1 - \theta)\frac{1}{\mathcal{L}} + c$$

$$\Leftrightarrow \frac{\mathcal{L} - 1}{2\mathcal{L}} \geq 0 \Leftrightarrow \mathcal{L} \geq 1.$$

Since this inequality holds, σ^t is a Nash equilibrium for G^t in this case. On the other hand, if $s_t' \in Z^1$, the proposal is one that will pass; then in order to have $|\{j \in L: s_{tj} > w_2\}| \geq \mathcal{L}/2$, we must have $s_{tt}' \leq s_{tt}^t$. But then

$$\frac{1}{|\Omega_t|} \sum_{w \in \Omega_t} v_t^{(LV, z_t(\omega))}$$

$$= (1 - \theta)s_{tt}^t + c + \delta v_t^* \geq (1 - \theta)s_{tt}' + c + \delta v_t^* = v_t^{(LV, s_t')}.$$

Hence, σ^t is a Nash equilibrium for G^t.

Notes

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1. There have been partial attempts in this direction. Austen-Smith and Banks (1988) develop a full-equilibrium model of voter and legislative behavior in a parliamentary system. However, their model is not dynamic, since it deals with a one-shot game. Kramer (1977), Baron (1989), and Ferejohn (Baron and Ferejohn 1989) have developed dynamic models of policy formation and legislative organisation; but these models are not full-equilibrium, since they do not explicitly consider voter and legislative interactions.

2. On the issue of seniority, there has been remarkably little formal work in the political science literature. One exception is Shepsle and Nalebuff (1990), which develops a model explaining the existence of seniority systems in the group provision of public or private goods. Their explanation is based on a model of overlapping generations, in which agents need to have incentives to participate throughout their lifetime. This explanation does not depend on any characteristics of the group that are unique to legislative bodies and hence is equally applicable to firms and legislative bodies. Although there has not been a lot of explicit work on seniority, there has been a substantial body of formal work looking at the role of specialized committees in legislative organization (e.g., Gilligan and Krehbiel 1990; Shepsle 1979).

3. We model the infinitely repeated game as a stochastic game and only consider stationary solutions, thus avoiding the usual problems associated with the folk theorems in repeated games.

4. It is important to note that in our formulation, the seniority system only matters on the initial proposal. An interesting variation to consider would be the case in which seniority counts not only on the first proposal but on all successive proposals, as well. We believe that our formulation makes sense for two reasons. First, it captures an aspect of how congressional rules operate—namely, seniority is embodied in the committee system, which gives higher-than-average influence to ranking committee members to specify the proposed legislation. But if a majority of the legislators oppose a committee proposal on the floor, then the committee effectively loses its power, and the proposal of the committee can be amended by the full legislature at will. Second, suppose one defines the status quo to be fair division (each district gets $1/\mathcal{L}$). Then our model is equivalent to a model in which the failure of a proposal leads to a reversion to the status quo.

5. While this is not completely realistic, it captures the idea that voters can punish their representatives who they feel are not acting in their best interests. Our formulation allows more limited punishments than would be the case if voters could remove the legislator from office permanently.

6. Elements of $\mathcal{T}$ will be denoted by t. If t has two components, then we write $t = (t_0, t_1)$. The reader is cautioned that t is a variable indexing the games and thus can represent different things in different games.

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THEORY AND PRACTICE: ALFARABI'S *PLATO* REVISITED

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According to Leo Strauss, knowledge of the best way of life is crucial to political philosophy. In "Farabi's Plato," *Strauss asks, assuming that the theoretical life can be known to be the best way of life, what is the status of this knowledge? Is the knowledge of the best way of life itself theoretical knowledge or practical knowledge? Without a coherent answer to this question, we cannot be certain that we know what we mean when we claim to know that philosophy is the best way of life. Strauss answers clearly the question about the status of the knowledge of the best way of life by affirming that it is practical, not theoretical, knowledge. For a variety of reasons, this answer is not persuasive in the form in which Strauss gives it.*

One of the most conspicuous themes of the work of Leo Strauss is the relationship between theory and practice. Whether Strauss regards this relationship as the highest, or one of the highest, themes of philosophy, he certainly sees it as one of the most urgent or necessary (1953, 162–63). In a recent book on Strauss, Heinrich Meier draws attention to the relatively early article "Farabi's *Plato*" (Strauss 1945), in which Strauss directly and specifically addresses this subject (Meier 1988a, 95–96). Despite the title, the article gives a strikingly explicit account not only of the views of Farabi, or Alfarabi, but also of Strauss himself, at least at that time, on what we can know about the dignity of philosophy as a way of life—the theoretical life—and about the practical alternatives to it.

Alfarabi (870–950 C.E.) was an Islamic philosopher with whom Strauss concerned himself for at least 20 years.[1] Strauss was interested in Alfarabi as a predecessor of Maimonides and as a representative of a kind of rationalism distinct from modern rationalism. While Strauss saw modern rationalism as somehow in the service of the conquest of nature and, as such, having an essentially practical aim, Alfarabi, as interpreted by Strauss, presented an alternative rationalism that was essentially theoretical or contemplative. Strauss, like Nietzsche, seems to have diagnosed a crisis of modern rationalism; but where Nietzsche is forced to turn away from reason in search of a cure, Strauss seeks a remedy in a return to an earlier form of rationalism. Meier finds in "Farabi's *Plato*" a significant statement of the philosophical position Strauss continued to elaborate over the following quarter of a century. Since Alfarabi's philosophy is of interest in its own right, we have two reasons for examining Strauss's early essay.

The full title of the short work that I, following Strauss, shall call the *Plato* is *The Philosophy of Plato, Its Parts, the Ranks of Order of Its Parts, from the Beginning to the End.* It is part 2 of the three-part *Philosophy of Plato and Aristotle,* part 1 being *The Attainment of Happiness* and part 3, *The Philosophy of Aristotle.*[2] Strauss focuses his attention on the *Plato* because he believes that Alfarabi has been more candid in that work (where Alfarabi speaks through the mouth of Plato) than in works where he speaks in his own name—for example, *The Attainment of Happiness* (Strauss 1945, 371–75).

I shall consider three topics, with a view to reflecting on the proper relation between knowledge or science on the one hand and human life or practice and its aims, on the other: (1) Does the knowledge of the best way of life belong to what Strauss calls philosophy "in the precise sense" (1945, 365) or does it belong to moral and political philosophy? (2) How do Alfarabi and/or Strauss view the status of Plato's philosopher–king? Is political ability ultimately identical with philosophic ability? Could the two ever be combined in one individual? (3) Can philosophy change the world, as Marx hoped? Or does the philosopher necessarily exist in an imperfect world, now and always, as Strauss's Alfarabi contends? Is philosophy essentially practical or theoretical?

While I try to review several aspects of Strauss's argument, it may be helpful to state the most significant conclusion of the inquiry at this point. For Strauss, much depends on the question, What is the right way of life? The standard by which to judge the best political order depends on the right, or best, way of life. The practical question about the best political order can be answered only by answering the question about the best way of life. The question about the best way of life is also a practical question. It is a deeper—a more fundamental—practical question than the question about the best political order. For Strauss, the right or best way of life turns out to be philosophy "in the precise sense" of theoretical, not practical, inquiry. Philosophy as the right or best way of life is not a value. It is not an arbitrary belief, conviction, preference, or life-style. According to Strauss, philosophy can be known to be the best way of life. In "Farabi's *Plato,*" Strauss does not try to prove that philosophy is the best way of life. Rather, he devotes his attention to the question, Given that philosophy can be known to be the right way of life, what is the status of this knowledge? Is this knowledge itself theoretical knowledge or practical knowledge? Without a coherent answer to this question, we cannot be certain that we know what we mean when we claim to know that philosophy is the best way of

life. This knowledge is crucial for political philosophy as it is understood by both Strauss and Alfarabi. Strauss answers the question clearly by affirming that the knowledge of the best way of life is itself practical, not theoretical, knowledge. I give reasons why this position is not persuasive.

In Strauss's classification of the forms of knowledge, practical knowledge is of lower rank or dignity than theoretical knowledge. It is hard to avoid the conclusion that practical knowledge is of lower cognitive status. Given that Alfarabi treats theoretical knowledge as certain knowledge, one must wonder whether practical knowledge is not less than certain. The consequences are obviously serious if the claim that philosophy is the best way of life is not more certain or true than rival claims on behalf of piety, honor, or pleasure. We are forced at least to raise the question how the classification of the knowledge of the best way of life as merely practical avoids decaying into the view that such "knowledge" is in fact a value judgment, preference, or mere conviction. It may seem ludicrous to suggest that Strauss ascribes to Alfarabi—and himself subscribes to—a position that collapses into a value judgment; but it may be equally ludicrous not to raise this question. Nietzsche would have us believe that all philosophers make such judgments, and Rosen singles out Strauss as being on this point a Nietzschean who regards philosophy as an act of the will (Rosen 1987, 110–11, 122–23, 127, 137). Clearly, Strauss does not intend to make philosophy an act of the will (see my earlier work, Colmo 1990). It is equally clear, however, that Strauss's own position collapses if he cannot give a coherent account of the knowledge by which he knows that philosophy is the best way of life. That account, as Strauss presents it in the totality of his writings, is by no means obvious. This problem is the motive for the critique of Strauss's view I now offer, of which the first section contains the most important results. I subsequently draw out further implications of Strauss's position and, in some cases, point to ways in which alternatives to Strauss's interpretation of Alfarabi suggest alternative answers to the basic philosophic questions at issue.

PHILOSOPHY PROPER AND POLITICAL PHILOSOPHY

Alfarabi makes it plain that the practical or political art (we would call it political philosophy) is concerned with discovering and providing for the "desired way of life" (1969, 60). Let us begin with the question whether the desired way of life, the life that makes man perfect or happy, can be the virtuous way of life (p. 54)? More specifically, can the virtues practiced in mosques and temples (i.e., the virtues inculcated by religion) make human beings perfect and happy (p. 61)?

Strauss argues persuasively that for Alfarabi, religion and the virtues based on religion do not provide the desired way of life (1945, 373). Strauss contends, more openly than Alfarabi, that the moral life (including, of course, the moral life based upon religion) cannot be the desired way of life because the moral life as such does not allow one to ask why one should be moral: "The moral life consists of the submission to the demands of honor and duty without reasoning why" (p. 388). The premise of Strauss's Alfarabi is that the desired way of life must be a reasoned way of life or a way of life of which a reasoned account can be given. The desired way of life cannot be based on mere choice be that choice inspired, mysterious, arbitrary, or irrational. The way of life acceptable to philosophy must be justified by reason. But morality that is rationally justified is no longer morality. Moral actions must be "choiceworthy for their own sake," that is, because they are moral, not because they are rationally dictated (Strauss 1964, 27). The virtuous way of life cannot be the desired way of life because morality as such is inherently unphilosophic. Moral philosophy, if such a thing is possible, cannot be a part of philosophy in the precise sense. Of course, it is important to add that nothing stops Strauss or anyone else from moving outside the assumption made here in order to seek a morality that is rationally grounded.

The moral life by itself cannot be the best way of life because it is unphilosophic. That the best—or, as Alfarabi says, desired—way of life, is the philosophic becomes plain in Alfarabi's discussion of the Socratic way of life. Alfarabi asserts, in terms much harsher than those used by Plato's Socrates, that the unexamined life is not worth living (1969, 63–64). The philosophic life is the only desirable life. But what is the philosophic life? According to Socrates, philosophy includes the study of the moral, political, or practical things. For this reason, even Socrates does not escape Alfarabi's implicit criticism, according to Strauss (1945, 363–65, 382–83). The best way of life is not the one that examines practical matters. Man's perfection is to be found in the knowledge of "the substance of each of the beings," "the beings," or the "natural beings" (Alfarabi 1969, 54, 56, 60, 65). The way of life devoted to the pursuit of such knowledge is what Strauss calls philosophic in the precise sense (1945, 365). Whereas the moral or practical things are the products of human action or choice, philosophy in the strict sense seeks knowledge only of the natural things, which are not products of any human knowing, making, action, or choice. Philosophy in the precise sense is strictly theoretical.

If we go this far with Alfarabi, then we arrive at a pressing difficulty. The "difference between the truly virtuous way of life and all other ways of life is based, not on a difference of purpose, of quality of the will, but on a difference of knowledge" (Strauss 1945, 388–89). Philosophy, as the life of reason, cannot be based upon an arbitrary choice or an act of the will. The philosophic way of life must be *known* to be superior to the moral or political life. The difficulty

arises when we try to classify the kind of knowledge by which we know that philosophy is the right, or desired, way of life. Alfarabi carefully distinguishes between the knowledge of the beings (whatever these may be) and the knowledge of the ways of life (1969, 56; Strauss 1945, 365). A way of life is not a being (Strauss 1945, 389). Strauss does not try to avoid the obvious conclusion that even knowledge of the best way of life is not philosophic or theoretical knowledge; knowledge of the best way of life is supplied by the highest practical art (p. 365).

Strauss's interpretation would eventually have to be brought into harmony with three works in which Alfarabi asserts or implies that "only the theoretical rational faculty can attain real knowledge of what happiness is" (cited in Galston 1990, 69). This amounts to saying, contra Strauss, that knowledge of the best way of life is theoretical knowledge. In general, Galston seems to take the view that Alfarabi blurs the traditional distinction between theoretical and practical knowledge so that the former includes much of the latter.

The assertion that the knowledge of the best way of life is not itself philosophic knowledge (strictly speaking) goes well beyond the assertion, discussed earlier, that the virtuous or moral way of life is not, as such, philosophic. This assertion is clearly based on philosophic knowledge. This knowledge in no way implies, however, that philosophy itself cannot be philosophically known to be the best way of life. Nor, certainly, does it imply that knowledge of philosophy as the best way of life is itself moral "knowledge," rather than philosophic knowledge.

Strauss moves in a different direction. In keeping with the differentiation of theoretical and practical knowledge, he suggests that it would be foolish to assume that philosophy exhausts itself in the investigation of the relation between philosophy and happiness (1945, 363). In other words, it would be foolish to assume that philosophy exhausts itself in the quest for the best way of life or that the philosopher has no leisure for anything other than this quest (1964, 21, 29). Indeed, the quest for the best way of life and the knowledge of that way of life are "strictly speaking merely preliminary" (1945, 366). The study of "the human or political meaning of philosophy" does not belong "to the same level" as philosophy proper (pp. 366, 368). One is even a little surprised to find that in the course of explaining Alfarabi's remarks on the limitations of Plato's Socrates as compared with Plato himself, Strauss says that Socrates was "merely a moral philosopher" who "neglected natural philosophy" (1945, 383; see Alfarabi 1969, 66–67). The moral philosophy that is disparaged here seems to include reflection on philosophy as the best way of life; Socratic reflection surely included this subject.

Since it is not philosophy strictly speaking, what is the status of political philosophy understood as the investigation of the human meaning of philosophy? Strauss cannot deny that practical knowledge in this extended sense is really knowledge. The discovery of the philosophic life as the best way of life cannot be reduced to the level of an arbitrary choice or an act of the will. The superiority of the philosophic life must be known, not willed (1945, 389). This point is central to Strauss's position.[3] Hence, Strauss's final formulation of the problem in "Farabi's *Plato*" seems to be that knowledge of the right way of life can "be described as philosophic since only the philosopher is competent to elaborate that question and to answer it" (p. 366). Such knowledge, however, is of a lower level than theoretical knowledge; even the highest practical knowledge is merely preliminary. Moreover, that the knowledge of the ways of life is not a proper subject of theory (because, to repeat, ways of life are not beings) is itself a theoretical insight (p. 389). To summarize in a way that may be more precise than clear, the knowledge that the theoretical life is the best way of life can be theoretically known to be merely practical knowledge.

Is merely practical knowledge certain knowledge? According to Alfarabi, theoretical knowledge is certain knowledge (Alfarabi 1969, 13; Mahdi 1973, 7–9). Strauss says that Alfarabi identifies philosophy with "the art of demonstration" (1945, 364). But in his *Philosophy of Aristotle*, Alfarabi calls the art of demonstration "the art of certainty" (1969, 87). If the distinguishing feature of theoretical knowledge is its certainty, is practical knowledge somehow uncertain? If it is, then how does philosophy ward off the rival claims of other ways of life? Ways of life that are openly based on faith, belief, or conviction would seem to be, at any rate, more consistent than a philosophic life dedicated to knowledge but itself based on faith or conviction (Strauss 1953, 75). The least one can say is that in "Farabi's *Plato*," Strauss defines a position that entails this serious dilemma without, however, acknowledging it or dealing with it.

It is interesting to compare Strauss's 1945 essay with his later writings. In *The City and Man*, Strauss raises the question whether it is possible to know that the philosophic life is the best life:

> Socrates could not know this if he did not know that the only serious alternative to the philosophic life is the political life and that the political life is subordinate to the philosophic life: political life is life in the cave which is partly closed off by a wall from life in the light of the sun; the city is the only whole within the whole or the only part of the whole whose essence can be wholly known. (1964, 29)

Knowledge of the best way of life comes from understanding the relationship between the philosophic life and political life, or life in the cave. But Strauss does not tell us whether this understanding is theoretical or practical, though the last clause in the passage just quoted seems to invite the conclusion that understanding of politics is a very high, if not the highest, kind of understanding. Does Strauss here mean to suggest the possibility that practical knowledge is more certain than theoretical knowledge? From this perspective, there would seem to be no longer any reason to exclude practical knowledge (as

being the most certain knowledge) from philosophy in the precise sense.

In an essay first published toward the end of his life (1971), "Philosophy as Rigorous Science and Political Philosophy," Strauss returned to the issue of this relationship (1983, chap. 1). The closest equivalent to what Strauss calls moral philosophy in "Farabi's *Plato*" is what he calls *Weltanschauungsphilosophie* (ibid.)—assuming it is not closer to what Farabi calls the religious investigation of the beings (1945, 373, n. 42). *Weltanschauungsphilosophie* conceptualizes religious, aesthetic, ethical, political, and practical–technical experience. Although this kind of philosophy "presents the relatively most perfect solution of the riddles of life and the world," it is nevertheless to be distinguished from philosophy as rigorous science for the same reason that Alfarabi distinguishes between moral philosophy and theoretical philosophy. *Weltanschauungsphilosophie* is the "philosophy" of the cave because in one way or another it presents, as true, things about which we cannot be certain, no matter how much we may want that certainty. If this is so, then the thought from *The City and Man* can be restated by saying that Socrates can know that the philosophic life is the best way of life only because he can know that philosophy as rigorous science is unqualifiedly superior to *Weltanschauungsphilosophie*. Moreover, the superiority of philosophy as rigorous science seems to depend on its more stringent standard of certainty.

Our primary question now becomes whether knowledge of the competing claims of the two kinds of philosophy is itself practical knowledge or theoretical knowledge. This time Strauss gives us a fairly straightforward answer:

> Reflection on the relation of the two kinds of philosophy obviously belongs to the sphere of philosophy as rigorous science. It comes closest to being Husserl's contribution to political philosophy. . . . In order to see the relation between philosophy as rigorous science and the alternative to it clearly, one must look at the political conflict between the two antagonists, i.e., at the essential character of that conflict. (1983, 36–37)

I take it that knowledge of the essential character of the conflict between the two kinds of philosophy includes knowledge of the superiority of philosophy as rigorous science. According to Strauss, it is only through such knowledge that Socrates could know the philosophic life to be the best life. Finally, Strauss makes it clear that such knowledge (knowledge of the best way of life) belongs to the sphere of philosophy as rigorous science. But it is equally clear that in "Farabi's *Plato*" this is not the case. There, reflection on the practical issue of the relation between theoretical philosophy and practical philosophy or reflection on the human meaning of philosophy does not belong to philosophy strictly speaking or to philosophy as rigorous science.

"Farabi's *Plato*" leaves the reader in some uncertainty concerning the status of reflection on the best way of life. Knowledge of the best way of life, the philosopher's self-knowledge (1945, 366), seems to be neither knowledge strictly speaking, nor religion, nor morality. But if it is none of these, what is it? Plato suggests that what lies between knowledge and ignorance is opinion or right opinion. There is considerable doubt as to whether even right opinion—much less, mere opinion—"can nevertheless be described as philosophic" (ibid.). Yet Strauss cannot settle for less if the superiority of the philosophic life is to be known, rather than merely willed. Such difficulties may have moved Strauss to elevate political philosophy—understood as comprehending the question of the best way of life—to the level of philosophy as rigorous science. While this clarifies the epistemological status of political philosophy, it seems paradoxical. How can the knowledge of the best way of life fail to be a kind of practical knowledge? Alfarabi, for one, always identifies the knowledge of the desired way of life as practical knowledge. Strauss insists upon this point and Miriam Galston reinforces it when she argues that for Alfarabi, the practical differs from the theoretical not in subject matter but in purpose, namely, a concern with human happiness (1990, 55, 69). Even if we reclassify knowledge of the best way of life as theoretical knowledge, some problems remain.

In making use of "Farabi's *Plato*," Meier runs the risk (as he is no doubt aware) that Strauss may have partially changed his mind in later years. But the risk may not be as great as we have made it seem. The difference between "Farabi's *Plato*" and "Philosophy as Rigorous Science and Political Philosophy" hides a basic agreement concerning one of the problems Strauss must surely have had in mind when he spoke of "the fundamental and permanent problems" (1959, 39). What is consistent in the two essays is the supremacy of theoretical knowledge over self-knowledge or practical knowledge.[4] The knowledge of the best way of life is either practical and therefore the lowest level of philosophy (as in the former essay) or given full theoretical status (as in the latter essay).[5] Strauss never adopts the view that philosophy proper consists of theoretical and practical philosophy on an equal level (1945, 366). He never suggests that if knowledge of the best way of life is to be ranked higher among levels of knowledge then this should be accomplished by elevating the rank of practical knowledge, rather than by removing the knowledge of the best way of life from the level of the practical altogether. The two essays share a fundamental assumption or tendency.

THE PHILOSOPHER–KING

Strauss's interpretation is based on the distinction that Alfarabi obviously makes between the science of the beings and the science of the ways of life (Alfarabi 1969, esp. 56). But Alfarabi makes another statement that is difficult to reconcile with the implications of this distinction. Alfarabi explicitly identifies philosophy with the royal art. Indeed, he follows Plato in making human happiness dependent on the exist-

ence of a philosopher–king. The passage in question is worth quoting in full as Strauss translates it:

> [According to Plato,] the *homo philosophus* and the *homo rex* are the same thing. (1) [According to Plato,] each of the two (*sc.* the philosopher and the king) is rendered perfect by one function and one faculty. (2) [According to Plato,] each of the two (*sc.* the philosopher and the king) has one function which supplies the science desired from the outset and the way of life desired from the outset; (3) each of the two (*sc.* functions) produces in those who take possession of it, and in all other human beings, that happiness which is truly happiness. (Strauss 1945, 367)

For Strauss, none of the statements in the passage he quotes strictly identifies philosophy with kingship or the royal art. The statements do no more than assert that philosophy and the royal art exist together in the same man; the two things are not identical.[6] Because the third statement goes so far, however, as to assert that philosophy produces the happiness of all human beings, it does "practically" identify philosophy with the royal art (p. 367). But Strauss regards this "extravagantly philanthropic remark," when taken literally, to be a "sheer absurdity" (p. 378). According to Strauss, such an extravagant remark on Alfarabi's part has three purposes.

First, if philosophy is the best way of life or the only true happiness, most human beings would be excluded from happiness (Strauss 1945, 381). The identification of philosophy with kingship helps to avoid or hide this conclusion, since the philosopher–king makes possible the happiness of "all," or of the nonphilosophic many.

Second, the royal art is the practical art that provides the desired, or right, way of life (Alfarabi 1969, 60). From this point of view, the identification of philosophy with kingship is a "pedagogic device" leading the reader toward the view that philosophy supplies not only the science of the beings but also the right way of life (1945, 370). Philosophy is self-sufficient. If philosophy is itself the royal or political art, then it is in no way dependent upon politics for guidance toward the desired way of life.

Alfarabi's third point, as Strauss sees it, derives from the fact that the philosopher necessarily lives in political society and that his relations with his nonphilosophic fellow citizens are "naturally difficult" (1945, 382). The philosopher needs a kind of political art in order to deal with these difficulties. Another way to say this is that philosophy, or wisdom, on the one hand and self-knowledge, or moderation (*sophrosyne*), on the other "cannot be separated from each other" (p. 366). The recognition that philosophy and the royal art cannot be separated does not require, however, that we take the identification of philosophy with kingship literally.

Alfarabi, Strauss says, "leaves no doubt . . . that philosophy and the royal art are coextensive" (1945, 368). Philosophy and the royal or political art necessarily exist together in the same human being: "A human being cannot acquire the specific art of the philosopher without at the same time acquiring the specific art of the king and *vice versa*" (p. 367). Strauss thinks that this view is an acceptable *alternative* to the view that philosophy and kingship are identical.

Strauss provides the following gloss on statements 2 and 3 of the quoted passage in which Alfarabi describes the relationship between philosophy and the royal art:

> The function of the philosopher supplies by itself both the science of the beings and the right way of life and thus produces true happiness in both the philosophers and all other human beings; the function of the king supplies by itself both the science of the beings and the right way of life and thus produces true happiness in both the kings and all other human beings. (1945, 367)

This restatement of Alfarabi's text brings out the fact that the text is quite compatible with the view that the function of the philosopher and the function of the king are two separate functions. Since there are two distinguishable functions, Strauss is able to preserve "the difference of level between philosophy proper and moral or political investigations" (p. 368). The reference to "all human beings" falls under the heading of philanthropic extravagance explained above.

Strauss's restatement, however, does not bring out the fact that the function of the philosopher is "one function"—or "a single skill," as Mahdi translates it (Alfarabi 1969, 60)—just as the royal function is "one function." Even allowing for some kind of distinction between the philosopher and the king, it is still the case that *one* function or a *single* skill supplies both the desired science (the science of the beings, according to Strauss) and the desired way of life.[7] We would have to admit that the one function or the single skill somehow combines theory and practice. Since Strauss sees theory as being on a different level from practice, the notion that one function or a single skill supplies the desired theory and the desired practice requires clarification.

Strauss offers the following explanation. Philosophy, while primarily and essentially directed toward the science of the beings, "cannot be exercised fully without producing the right way of life"; kingship cannot be exercised fully without producing the science of the beings, but it remains "primarily and essentially concerned with the right way of life" (1945, 368). In the best case, philosophy and kingship cannot be separated; but they are not identical.

It is certainly not unfair to ask why, if philosophy is essentially concerned with one thing and kingship is essentially concerned with another thing, Alfarabi emphasizes that the single function–philosophy–is concerned with two things (one theoretical and one practical) and the single function–kingship–is likewise concerned with two things? To speak only of philosophy, it can supply both the desired knowledge and the desired way of life only if it supplies both theory and practice. Strauss himself draws the necessary conclusion from his own premises: "The identification of philosophy as the highest theoretical art with the royal art as the highest practical art can be

literally valid . . . if contemplation itself is the highest form of action" (1945, 386).

Strauss does not indicate whether the single function or art that supplies both the desired science and the desired way of life also supplies the knowledge of the desired way of life: With this question, I return to the first topic of the essay. Alfarabi's (1969) ambiguous expression *desired science* or *desired knowledge* certainly seems to suggest the possibility that the desired science might include not only the science of the beings, mentioned by Strauss in his gloss to the text but also "the science of the ways of life," which Strauss mentions elsewhere (1945, 365). The knowledge of the right way of life would certainly seem to be some part of the desired science. The least one could say is that while Strauss emphasizes the difference of level between the theoretical and the practical, Alfarabi emphasizes the unity of the function or skill that supplies both theory and practice.

For Strauss, a unity of theory and practice can occur only on the plane of the theoretical: theory or contemplation itself is the highest form of action. There is a place where Alfarabi, in a somewhat different way, considers the possibility of a man who combines theoretical science and practical science; but he does not there say whether the combination itself ought to be regarded as theoretical or practical (1969, 66). On the latter point, there is only one explicit statement in the *Plato*: Alfarabi treats the practical arts as the arts that combine knowledge and action (p. 58). He is silent about the possibility of a combination of knowledge and action on the level of the theoretical. Indeed, shortly before the passages suggestive of a philosopher–king, Alfarabi lists three possibilities: a scientific art that supplies the desired knowledge, a practical art that supplies that knowledge, or a practical art that supplies the desired way of life (p. 59). While Alfarabi at least entertains the possibility of a practical art that supplies the desired knowledge, he does not entertain the possibility of a scientific or theoretical art that supplies the desired way of life.[8]

Statements by Alfarabi relevant to the subject of the philosopher–king do more than open the question whether theory is not itself the highest form of practice. As we have seen, under Strauss's scrutiny, Alfarabi's statements also raise the question whether philosophy is coextensive with practice in the sense of political kingship. Could both functions exist in the same human being? Would the excellence of the philosopher complement that of the king and vice versa? Or would the attempt to perform both functions diminish the performance of each in itself? In the latter case, is division of labor—one person, one function—superior to any attempt at the coexistence of the two functions in one human being? Or does the perfection of any one function fall short of being the perfection of the whole person?

As Strauss explains (and as we have seen), Alfarabi "leaves no doubt" that in the best case philosophy and kingship coexist in the same human being. This does not mean that the best philosopher must rule a specific city or nation but, rather, that the best human being combines the two capacities. Strauss goes so far as to suggest that the philosopher and the king have the same nature (1945, 368, n. 28).

Here, again, it is instructive to compare "Farabi's *Plato*" with a later statement by Strauss. In "Xenophon's *Anabasis*," Strauss makes the following observation:

> Xenophon stands somewhere in between the older Cyrus [a manly or political man] and Socrates. By this position he presents to us not a lack of decisiveness but the problem of justice: justice requires both the virtue of a man . . . and the virtue of Socrates; the virtue of the [manly or political] man points to Socratic virtue and Socratic virtue requires as its foundation the virtue of the man; both kinds of virtue cannot coexist in their plenitude in one and the same human being. (1983, 128)

In "Farabi's *Plato*," Strauss presents the coexistence of political and philosophic excellence in one human being as an alternative to the identification of those two excellences. One cannot acquire the one "without at the same time" acquiring the other (1945, 367). In his essay on Xenophon, he presents the two virtues as incompatible "in their plenitude." These two statements contradict each other. What remains constant between the two essays is the superiority of the virtue of Socrates, the theoretical human being, to the virtue of Cyrus, the political being. Indeed, the gulf between the two has widened from the first essay to the second; theory remains supreme.

Galston presents evidence that Alfarabi shared the view espoused by Strauss in his article on Xenophon (1990, 84). The activities presupposed by practical perfection could be understood "to be incompatible with, and possibly to undermine, the activity comprising theoretical perfection." This problem arises because of passages in Alfarabi in which theoretical perfection requires the complete transcendence of the body. On balance, Galston (1990, 53–94, esp. 56 and 91, n. 69) rejects this view in favor of what she calls the comprehensive understanding of happiness, wherein theoretical and practical activity are both part of the essential nature of happiness.

My own view is that practical activity is somehow fundamental—though by this I do not mean to imply that public service for the benefit of others is necessarily fundamental (see Galston 1990, 87). Nor do I at all mean that one must actually win an election and hold public office in order to actualize one's full potential as a human being. Rather, the effectual truth (to borrow a phrase from Machiavelli) of the notion that theoretical perfection requires the transcendence of the body is that one must write (Walzer 1985, 261). Writing is a practical activity in that it both aims at the author's own happiness and is a creative activity. I can think of no compelling reason why legislative activity (writing the laws of a people) or, at any rate, preparing the ground for such legislative activity should not be included in the kind of writing that is essential to the philosopher's practical activity. Nor do I see this practical activity as ultimately incompatible with theoretical perfection. On the con-

trary, if we know only what we make, then some kind of creative activity is essential to theoretical activity.

I am aware that for many scholars, the notion that we know only what we make is a distinctively modern notion, certainly not one to be found in Plato or Alfarabi. For reasons that go well beyond the scope of this paper, I reject this view. Plato and Alfarabi (in contrast to Socrates, who wrote nothing) show by their deeds the importance of creativity to the highest human life. It is not necessary to separate knowing from making, theory from practice, in order to discover the difference between ancients and moderns. The distinction between ancients and moderns would be sufficiently established if it could be shown that only for the moderns does the knowability of what we make lead to the possibility of wisdom. Nowhere do Plato or Alfarabi reach the conclusion that through knowing (so far as we are able) the things that we have made, we may thereby hope to know everything, because we have in principle made everything. On the contrary, the admission that we know *only* what we have made is a recognition of human finitude. There is no necessity to follow Strauss in separating theory from practice, knowing from making, in order to avoid the hubris of trying to turn philosophy into wisdom (see Colmo 1990, 158).

THE JUST CITY

According to Strauss, Alfarabi clearly distinguishes between theory and practice, with theory ranking higher. The goal of the best way of life is essentially theoretical, not practical. Yet Strauss also insists that philosophy cannot be separated from self-knowledge or from moderation. Self-knowledge includes the realization of the need for the truth about the whole, as well as "of the difficulties obstructing its discovery and its communication" (1945, 366). Self-knowledge is essentially practical or political knowledge. Strauss seems to separate the theoretical and the practical while at the same time tying them together.

The contradiction in Strauss's position (at least within the Alfarabi essay) is merely superficial. With the problem of communication, which Strauss raises in this context, we are clearly in the realm of politics—not in the esoteric sense of the practical knowledge that leads to philosophy but in the ordinary sense of one's political relations with one's fellow human beings. The philosopher's self-knowledge is essentially practical or political knowledge in a sense that includes knowing how to get along with one's fellow citizens. The philosopher knows that his knowledge as a philosopher is strictly theoretical; for it is knowledge about the things that are independent of human choice or action, not about the objects of choice and action that concern the vast majority of the philosopher's fellow citizens. The philosopher is not an expert of the sort that knows how to get things done. Only the fellow citizen who is a potential philosopher might benefit from the philosopher's practical knowledge that philosophy is the best way of life. But this very knowledge might be harmful to most citizens, since it "is tantamount to closing the very prospect of happiness to the large majority of men" (1945, 378). Most human beings cannot be philosophers and are, hence, barred from the best way of life. But the philosopher must not communicate this knowledge, lest he drive his fellow citizens to anger or despair. For this reason, the philosopher's self-knowledge, in so far as it governs his relations with other citizens, is primarily a knowledge of the need for moderation.

Strauss does not go so far as to advocate, in the name of moderation, a philosophic withdrawal from political life, although he does mention this alternative (1945, 362). What he suggests is the abandonment of what he calls Socrates' "revolutionary" quest for the just city. It is not necessary for the philosopher openly to challenge the city, as Socrates did, thereby incurring, as Strauss puts it, "persecution and violent death" (p. 383), a phrase that seems to owe more to Hobbes than to Alfarabi. Alfarabi's Plato offers an alternative to the revolutionary way of Socrates:

> Plato substituted for [the revolutionary quest] a much more "conservative" way of action, *viz.*, the gradual replacement of the accepted opinions by the truth or an approximation to the truth. The replacement, however gradual, of the accepted opinions is of course a destruction of the accepted opinions. But being emphatically gradual, it is best described as an undermining of the accepted opinions. (ibid.)

In keeping with this "conservative" line of action, the philosopher–king who rules openly is replaced by the "secret kingship of the philosopher who lives privately as a member of an imperfect community" (p. 384). Although the philosopher lives privately, this does not necessarily mean that he altogether withdraws from politics. He will not openly confront the city, but he still may exercise a kind of "secret kingship"—through his writings, for example.

Strauss and Alfarabi are in full agreement that any political influence exercised by this private or secret rule will be "emphatically gradual," but it is still necessary to ask what the goal is of this gradual philosophic political action. Strauss answers,

> The goal of the gradual destruction of the accepted opinions is the truth, as far as the elite, the potential philosophers, is concerned, but only an approximation to the truth (or an imaginative representation of the truth) as far as the general run of men is concerned (1945, 384).

The aim of the philosopher's gradual political action with respect to the potential philosophers is to undermine—and ultimately to destroy—their belief in the accepted opinions of their community. But why does Strauss call this way of acting "conservative"? The fact that it is gradual and secretive merely makes it conspiratorial, not conservative. It is conservative, however, with respect to the accepted opinions of the vast majority of citizens. These opinions can be at best only approximations to the truth. This might

suggest that the activity of liberating the philosophers is merely a more extreme version of the activity of approximating the truth for the majority of people. The two activities might seem to go hand in hand; but in fact, they are opposed to one another. No matter how close the accepted opinions might be brought to the truth, these opinions are false. For example, the political truth is that one should act morally because moral action is good for its own sake—or so says Strauss, though Galston reports that "the doctrine that actions must be chosen for their own sake to qualify as moral does not appear in Alfarabi's writings" even where one would expect it for moral and political reasons (1990, 172).[9] For the philosopher, however, morality is, in Strauss's words, "merely a means toward" the true happiness of the philosophic life (1945, 387). The potential philosopher must somehow be taught the philosophic truth about morality in such a way that teaching does not corrupt the nonphilosophic many. A parallel situation exists with respect to religion: "Conformity with the opinions of the religious community in which one is brought up is a necessary qualification for the future philosopher" (p. 383)—even though an equally necessary qualification is that these opinions not retain their grip on the mind. Certainly, no philosopher would expose the religious errors of his community in an irresponsible attempt to create a secular society in which error does not exist. Every philosopher recognizes the need for moral and religious belief in a healthy political community. He also recognizes that these beliefs can never be converted into rational knowledge. The philosopher, as secret king, will do his best publicly to promote the accepted opinions with respect to morality and religion while esoterically working through his books to destroy the hold of those opinions over the minds of potential philosophers. This is the goal of the gradual, secret political activity of the philosopher as understood by Strauss.

The political activity of the philosophers aims at the benefit of potential philosophers, but what of the philosopher's duty to the community as a whole? Certainly, the philosopher tries to avoid hurting the nonphilosophers through any action that would undermine the moral and political opinions of the community; but does the philosopher have a desire or an obligation to benefit the community as a whole in a positive way? Reforming the tax structure or improving the school system certainly are not the duty of the philosopher as philosopher; nor are they goals that are amenable to the secret, gradual activity of the philosopher. Strauss is altogether silent about this kind of political reform. Gradual improvement of the tax structure or of the school system are certainly possible; but they are not the business of the philosopher, and it is not likely that they could be brought about through esoteric writing (unless we are to interpret the Platonic dialogues as a massive contribution to school reform). Such reforms require just the kind of open, public participation in political affairs that Strauss's philosopher eschews. Gradual reform of the moral and religious beliefs of his community might be more in the philosopher's line of work, but this could only be done for the ultimate benefit of the potential philosopher. Altering the religious beliefs held by the vast majority of citizens can only mean the exchange of one darkness for another, for these beliefs can never be converted into rational knowledge.

In the light of this philosophic conservatism with respect to political action, how are we to understand Plato's claim that the just city comes into being through the rule of the philosopher (*Republic* 473d–e)? Alfarabi tells us that the rule of the philosopher is not possible in imperfect cities (1969, 62), and Strauss adds that imperfect cities constitute "the world as it actually is and as it always will be" (1945, 381). In other words, "there are examples of men of the highest excellence whereas there are no examples of cities of the highest excellence" (1964, 49). Hence, the philosopher must learn to live in the nonphilosophic city, a place in which he is always "in grave danger" (Alfarabi 1969, 67; Strauss 1945, 382). Part of the philosopher's self-knowledge involves learning to cope with this danger. He must learn to "adjust himself to the requirements of political life, or to the ways and opinions of the vulgar" (1945, 383). To some extent, then, the philosopher must, after all, assimilate himself to the ways of the vulgar (see p. 362). The philosopher's self-knowledge is moderation. Precisely because the theoretical way of life can never be translated directly into moral or political practice, it is necessary that theory not be separated from practical wisdom (though Strauss does not say whether philosophy and moderation can "coexist in their plenitude"). Strauss's moderate conclusion is that there can be no revolution leading to the just city.

I shall try to summarize Strauss's interpretation of Alfarabi on the subject of the political activity of the philosopher. The philosopher will not confront the nonphilosophic community in which he lives. Indeed, because he is a philosopher, he is in danger from the moral and believing community. He may consider himself lucky just to be left alone. Moreover, his greatest pleasure and happiness is not political activity of any kind but, rather, theoretical contemplation. For these reasons, the complete withdrawal from political life must be tempting. If he does not succumb to this temptation, it can only be because of his interest in and care for those in the community who are potential philosophers (but see Galston 1990, 87). His attempt to communicate with these future philosophers poses a grave threat to the moral and religious beliefs of the community. Hence, the philosopher must undertake to communicate the most daring or radical thoughts in a way that minimizes their political impact.[10] In this respect, the political action of the philosopher can correctly be described as conservative.

It is probably not possible in this case to repeat my approach of contrasting these sentiments with anything Strauss said later. He did not change his views.

It is, however, instructive to compare Strauss's statement with the last page of Alfarabi's *Plato*, upon which he bases it. Alfarabi says that Plato

> mentioned the Athenians (his own people) and their ways of life. He described how to abolish their laws and how to turn them away from them. He described his view regarding the way in which they could be moved gradually, and he described the opinions and the laws toward which they should be moved after the abolition of their ways of life and laws. (1969, 67)

Nothing could be more striking than the contrast between Strauss's "conservative" way of action (which is not to be confused with a conservative way of thinking) and the revolutionary tone of Alfarabi's closing paragraph.[11] Strauss and Alfarabi agree that change must be gradual, but Alfarabi mentions as the ultimate goal much more sweeping changes than anything even hinted at by Strauss. Indeed, Strauss does what he can to soften Alfarabi's language. For example, while Alfarabi refers, in the full passage, three times to the destruction or abolition of the generally accepted ways of life and corrupt laws, Strauss says that this destruction is "best described as an undermining" (1945, 383). The best description, it would seem, was not Alfarabi's. Strauss's wording made it less shocking. Instead, Alfarabi goes out of his way, as it were, to alarm the reader with the thought that at least one philosopher, Plato, sought to abolish the way of life of his own people. Moreover, Plato sought to act as a legislator, replacing the old laws with new laws.

Strauss's esoteric reading of the passage just quoted tends to make it more conservative, more moderate, than it appears to be on the surface. By Strauss's own account of esoteric writing, however, one would expect the surface of a statement to be more moderate than the hidden truth. At least one detail of the passage suggests that Alfarabi's true intention is more radical than what he says on the surface. We notice that Alfarabi explains that the Athenians are Plato's own people. Is it possible to conceive of a student of the *Plato* who did not know this? And if this knowledge is helpful, why withhold it until the end of the book? Strauss is surely right in thinking that the *Plato* is an esoteric work; it teaches by implication and insinuation. Under the circumstances, the notion that a philosopher might attempt to destroy or abolish the way of life and the laws of his own city or nation can only be taken as a subtle (but not too subtle) challenge by Alfarabi to his own people and to the Islamic law, the *shari'a* (1945, 372, n. 40). Naturally, in relation to Plato, Alfarabi speaks of changing the Athenian *nomoi* (laws); but he also uses the comprehensive expression "ways of life," which in the case of his own people would surely extend to the *shari'a*.[12] I do not think that Strauss's overall interpretation of the *Plato* allows him to notice the possibility that Alfarabi may here be making an amazingly bold declaration of his own political purpose (see Mahdi 1973, 25, n. 10). Strauss certainly does not mention this point.

As we have seen, Strauss does notice and make use of a statement in the *Attainment of Happiness* in which Alfarabi counsels that "conformity with the opinions of the religious community . . . is a necessary qualification for the future philosopher" (Strauss 1945, 383–84 and 373, n. 41). The philosopher accepts the world as he finds it; and this, certainly, is in some sense true. Strauss supports his "conservative" interpretation of Alfarabi by citing a reference in the *Attainment of Happiness* to outward conformity in matters of religion. Strauss interprets the last page of the *Plato* in the light of a statement made in the *Attainment of Happiness* and uses that statement to make the conclusion of the *Plato* seem more moderate or conventional than it might otherwise seem. This is surprising, to say the least, given the "canon of interpretation" that Strauss wishes the reader "scrupulously to follow": "Apart from purely philologic and other preliminary considerations, one is not entitled to interpret the *Plato*, or any part or passage of it, by having recourse to Farabi's other writings. One is not entitled to interpret the *Plato* in the light of doctrines, expounded by Farabi elsewhere, which are not mentioned in the *Plato*" (p. 375). Strauss explicitly mentions that one must favor the *Plato* in any place where its teaching conflicts with the *Attainment of Happiness*. Moreover, it is clear that Strauss particularly has in mind the priority of the *Plato* on religious matters. He is aware of the "deep silence" of the *Plato* with respect to the other life, the immortality of the soul, the soul itself, and even the Platonic ideas and the *nous* (intellect) by which the ideas are known (pp. 364, 371–72).[13] He is aware—and he has made us aware—that by this silence Alfarabi rejects these ideas or beliefs. But the *Plato* observes a deep silence (or, at any rate, a direct or explicit silence) on the issue of religious conformity. It is hard to avoid the conclusion that by introducing the idea of the necessity of religious conformity into his interpretation of the *Plato*, Strauss does not pursue the implications of his own canon of interpretation.[14]

What are those implications? How will we understand the last page of the *Plato* if we do not read it in the light of pious statements that Alfarabi makes elsewhere? Certainly, the *Plato* does not deny that the philosopher must be circumspect; he is, after all, in grave danger. Indeed, Alfarabi makes it clear that the art of Thrasymachus (i.e., the art of the sophist) may be most useful to the philosopher in speaking to the multitude (1969, 66; Strauss 1945, 383). Alfarabi translates the title of the Platonic dialogue *Sophist* as "falsifier." One of the most necessary aspects of such falsification in Alfarabi's circumstances must surely have been outward conformity in matters of religion.[15]

Even so, it is clear that the *Plato* is much more reticent than the *Attainment of Happiness* in stating the need for conformity in religious matters. I do not believe that this difference of emphasis can be understood on the basis of Strauss's interpretation of the *Plato*. Strauss is primarily concerned, in "Farabi's *Plato*," with the fact that most people "are eternally

barred, by the nature of things," from philosophy (1945, 381). Hence, if philosophy is to be possible at all, then it must be possible in imperfect, nonphilosophic cities. According to Strauss, Alfarabi's "last word" on this subject is that philosophy and the perfection of philosophy do not require the establishment of the perfect political community (ibid.). Consequently, Strauss turns the focus of his attention away from the question of how to improve political life and toward the question of how the philosopher can survive and continue to philosophize in imperfect cities taken as they are. In trying to deal with this problem, Strauss pursues his inclination or tendency to separate the philosophic, or theoretical, from the practical, or political. Indeed, separation becomes opposition as philosophic radicalism is juxtaposed with political conservatism. Turning to Alfarabi, we see that he also recognizes the severe limits of political change (see, e.g., p. 384, n. 69). He recognizes that the philosopher will never be glorified or exalted by the citizens (i.e., will never rule openly or officially) and that all change must be gradual (1969, 67). Strauss's interpretation notwithstanding, however, the *Plato* emphasizes the political change that is possible.[16] Outward conformity is not always necessary, at least not to the same degree or to the same kinds of laws. If anything, the *Plato* makes clear that Alfarabi may have overestimated, in a particular case, his ability to abolish the ways of life and the laws of "his own people."

Strauss suggests that when Alfarabi talks about the legislator who will bring into being the just city of the philosopher, Alfarabi means, by *legislator* a prophet, "the founder of a revealed religion" (Strauss 1945, 380; see Alfarabi 1969, 66). On this interpretation, the just city would not be possible except on the basis of revelation. Strauss leaves no doubt that since the just city is possible only on this basis, it is not possible as an actual city, or the just city is possible only "in speech" (1945, 379).

My own suggestion is that Alfarabi himself is an example of the kind of legislator he has in mind as the founder of what he always calls "the other city"—that is, other than the cities existing in his time but certainly not otherworldly.[17] If this makes Alfarabi a prophet—a subject nowhere explicitly mentioned in the *Plato* (but see Alfarabi 1969, 61)—then so be it. Alfarabi's "prophecy," or legislation, is clearly not based on divine revelation. The only plausible source of Alfarabi's legislative authority is his philosophic insight. Alfarabi's efforts to be the founder of "the other city" presuppose less opposition and more cooperation between the theoretical life and the practical, political life than Strauss seems to grant.

Notes

1. Published works dealing with Alfarabi span the period from Strauss 1935 to Strauss 1957.

2. I have used the English translation of all three works by Mahdi (1969). Unless otherwise indicated, all parenthetical citations in the text are to pages of Mahdi's translation of Alfarabi *with the exception* of references between 357 and 393, which refer to pages in Strauss 1945.

3. See, e.g., Strauss 1965, 29–30. I have no explanation for Strauss's statement, in the last paragraph of "Farabi's *Plato*," that philosophy is animated by a "conviction" about the life worth living—a word that seems to imply belief and will, rather than knowledge.

4. Meier correctly represents Strauss's theoretical emphasis when he quotes Strauss's reference to "the necessarily anonymous truth" (Meier 1988b, 765; cf. Strauss 1945, 377). On Farabi's view of the "anonymous truth," contrast the relatively lengthy account of Protagoras' view (i.e., that the knowledge natural to man is not anonymous but relative to each individual) with the brief and obscure account of Plato's rejection of Protagoras' view (Alfarabi 1969, 54). This is one of only two places in the *Plato* (not counting the title) where Plato is mentioned by name. In the place where the anonymity of thinking is the issue, Alfarabi does not allow the philosopher to remain anonymous.

I am reminded that the Socratic ideas make the human good anonymous. It is indeed strange that Socrates and Descartes should share in their thinking the common feature that what is truly intelligible (the ideas in one case, body as extension in the other) is not alive. It is not surprising that a rift between the intelligible and the good has become a problem (*the* problem?) in Western thought. The nature Strauss points out a similar or related "defect" in the nature Socrates (1966, 59). In my view, one would expect Strauss to share this "defect" and hence not to notice it in Socrates. Obviously, my net does not catch all the fish in Strauss's waters.

5. These two alternatives are, in a way, present within "Farabi's *Plato*." Early in that essay, realization of the need to adjust to the requirements of political life belongs to "self-knowledge" (Strauss 1945, 366), while, later, that realization seems to derive from "insight into the nature of beings" (p. 383). The latter view seems very close to the position Strauss takes in "Philosophy as Rigorous Science and Political Philosophy."

6. While she does not offer an interpretation of the passage just quoted, Galston cites with approval the passage in which Strauss says that philosophy and kingship are not the same art. But this observation is appended to a paragraph stating that "Alfarabi appears to maintain the more extreme thesis that [practical rational excellence and moral virtue] are constitutive parts of philosophy itself" (1990, 64).

7. Alfarabi's *Enumeration of the Sciences* confirms the thought that the highest royal craft is supplied by one faculty, not two. See Mahdi 1975, 131–37.

8. Alfarabi says that the practical arts are not adequate for obtaining the desired knowledge or the desired way of life (1969, 59). But the desired way of life is by definition the product of a practical art (p. 60). The first statement makes sense only if it is limited to the generally accepted practical arts or to the practical arts practiced by the multitude (p. 59).

9. See n. 13.

10. Strauss's own publications from the last decade of his life might be thought to exemplify this kind of writing.

11. The contrast between the end of the *Plato* and Maimonides' *Guide of the Perplexed* 2.11 is also striking. Maimonides presents himself as building "a great wall" around the Law to protect it, a project that certainly sounds conservative. Strauss tells us that he studies Alfarabi in order to have the necessary background for understanding Maimonides (1945, 357, 393). Is it not possible, however, that Strauss's understanding of Alfarabi is colored by reading Alfarabi under the influence of a knowledge of Maimonides' intention to protect and preserve the Jewish law? Does Maimonides in fact become the background for Strauss's interpretation of Alfarabi? However that may be, the contrast I am suggesting between Alfarabi and Maimonides need not be explained in terms of basic philosophic differences. It is one thing to attack the *shari'a* at a time of Muslim power; it would have been quite another thing to attack the halakha during the Diaspora. On Alfarabi and Maimonides, see Berman 1974, esp. 163.

12. On the distinction between *nomoi* and *shari'a*, see Maimonides' *Guide to the Perplexed* 2.40. Galston comments that there is no reason "to assume that Alfarabi must have viewed the conventions of his time as disparagingly as Socrates regarded the way of life of the Athenians," but she offers no Alfarabian text as support for this claim or for her own view that "many features of Islam were a vast improvement over Greek morality and opinions" (1990, 173).

13. As Strauss points out, Alfarabi is silent about immortality while mentioning the *Phaedo*, in which immortality is, of course, a major theme. Strauss concludes, rightly I think, that this silence amounts to a rejection of the doctrine of immortality. According to Alfarabi's summary, the *Phaedo* seems to be a book about whether a man should be willing to die for the sake of the practice of the true moral virtues, this alone being sufficient reason to sacrifice one's life if need be (1969, 63). I take it that there is a connection between Alfarabi's rejection of immortality and his refusal to give a clear affirmative answer to the moral question he raises here. Alfarabi and Kant seem to agree that if morality is to be treated as an end in itself and if human beings are to be expected to make sacrifices commensurate with such an end, then it is necessary to assume that the soul is immortal. Alfarabi does not agree, however, in making this last assumption.

14. One could avoid this conclusion if one assumed that Strauss is here writing esoterically. Given that Strauss is an esoteric writer, does that fact dispose of the present case? In this instance, we could avoid what seem to be contradictions in Strauss's argument by ignoring his "conservative" remarks about political action, just as we are inclined to ignore Alfarabi's admonitions to religious conformity or, at any rate, to minimize their significance. But the weight of the evidence is against this view of Strauss's intention in the case in point. For one thing, it is possible to discover in Alfarabi a political alternative to the acceptance of the imperfect communities existing in his time. Strauss's "conservative" remarks, however, seem to be his "last word" on the subject. I have placed the word *conservative* in quotation marks as a reminder that it applies only to Strauss's attitude toward political action; there is nothing conservative about Strauss's thinking.

15. This certainly does not mean that one compartmentalizes one's thinking so that one is a believer on holy days and a philosopher on other days (Strauss 1945, 374). See Alfarabi's criticism of the sophist Hippias, whom Alfarabi speaks of as if he were in fact two men (1969, 59). This passage helps us to understand how Alfarabi would distinguish between the philosopher and the sophist. The sophist is somehow a man in contradiction with himself. But then, we may ask, does anyone ever fully escape contradiction?

16. Muhsin Mahdi sees the realization of the political good as being Alfarabi's "central concern." Concerning the view that philosophers "should tend only their private gardens," Mahdi writes: "Perhaps there are times and places which necessitate these views. But one need not make a virtue out of necessity" (1981, 19–21).

17. According to Alfarabi, the title of the book dealing with the legislator (the *Epinomis*) means "investigator." In the *Plato*, the one who is repeatedly said to investigate is Plato himself. Alfarabi is, of course, also such an investigating legislator.

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FREEDOM, COMMUNITY AND FUNCTION: COMMUNITARIAN LESSONS OF MEDIEVAL POLITICAL THEORY

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The communitarian critique of liberal individualism is presently experiencing something of a decline. One of the reasons for the failure of communitarians to make a lasting impact on liberal theory may be the historical precedents to which communitarians have turned, such as Aristotle and civic republicanism. I argue that communitarian theory may more fruitfully draw upon a model of the relation between individual and community derived from the Latin Middle Ages. This approach—communal functionalism—claims that the community is essentially composed neither of individuals nor of citizens but, rather, of functional groups or parts arranged according to their contribution to the community. I consider two variants of this framework, John of Salisbury's "physiological" version and Marsiglio of Padua's "civic" account. I close with an evaluation of the relevance—and also some of the limitations—of the communal functionalist model in relation to the requirements of contemporary communitarian theory.

One of the most striking features of the debate between communitarians and liberal individualists that has dominated Anglo-American political theory during the last decade or more is the extent to which it has been conducted through the medium of the history of Western political thought. In particular, the communitarians, arguing for what has been called "the politics of the common good" (Sandel 1984; Taylor 1986), have often turned to traditional models of civic-based virtue.[1] Some have followed Aristotle's defense of a community which is composed of citizens who are competent both to rule and be ruled and in the public realm of which all are accorded an appropriate share of burdens and rewards (Beiner 1983; MacIntyre 1985; Nussbaum 1990). Others have looked to civic republicanism (as espoused by, e.g., Machiavelli, Rousseau, and Hegel) for its devotion to political participation as the expression of a uniquely civic mode of virtue (MacIntyre 1985, 236–38; Oldfield 1990a, 181–85; Taylor 1989, 165–72).

In all too many cases, however, such attempts to formulate communitarian principles along lines derived from earlier political theories have been faulty. Serious questions have been posed about the historical accuracy and philosophical fidelity of communitarian interpretations of Aristotle and the "civic republicans" (Mulgan 1990; Newell 1987; Oldfield 1990b; Yack 1985). Certain communitarian thinkers have merited the accusation of "romanticism" on the grounds that the earlier social practices and doctrines which they laud as built upon a substantive notion of the common good actually reflected a failure to appreciate alternative conceptions of that good and consequently mirrored the hegemony of one set of racial, gender, or class interests (Gutmann 1985, 318–29; Holmes 1989, 239–40; Kymlicka 1990, 225–27).

Behind the complaints raised by liberal critics with regard to the communitarian appropriation of the past appears to be the assumption that classical Greek and republican notions of the common good were so thickly defined as to exclude a realm of individual, independent judgment about social (or personal) goods or virtues from emerging. While this seems to me to be essentially true (neither Aristotelian nor republican conceptions of the good permitting much room for a diversity of viewpoints about basic ends or definitions of a "way of life"), it does not justify the further conclusion that *all* traditional accounts of the public good are guilty of the same "thickness." Liberal theory seems to take largely for granted that every conception of the communal good that moves beyond the minimal (or thin) requirements of public order and tolerance falls prey to the same charge: it imposes an "exclusivist," quasi-totalitarian conception of a common life upon individuals, who are thereby prevented from choosing their own life plans. In sum, it is presumed that all versions of communitarianism are antiindividualistic and are so in the same way.

As a purely philosophical claim, this view is suspect; for the liberal understanding of the common good seems to trade on an ambiguity that has become attached to previous communitarian usages of that phrase. Communitarians have generally sought to argue that a community is defined in terms of a substantive good or set of goods that generate a fundamental civic identity within which all members are embedded and to which they ought to be socialized. Liberals have been prone to construe this as equivalent to the rejection of individual diversity in relation to human goods. But it is one matter to claim the unity of the common good and another to assert the impossibility of different interpretations of the *same* common good (or different ways of realizing that good) according to the range of roles or positions within society. Unquestionably, communitarianism is committed to the view that a single common good exists; it need not (logically speaking) also be committed to the view that this good will be understood

or applied identically by all persons. Instead, it seems plausible to adopt the view that the realization of any common good is conditioned by circumstance, in the sense that different persons occupying divergent life situations will conceive of the common good in a manner relevant to their social needs and surroundings. This is not to say, however, that such persons adopt substantively differently or incompatible conceptions of that common good. It is rather to assert that their public selves are not formed abstractly or generally (by reference to a mode of civic virtue identical for all citizens) but are shaped in a concrete and mediated fashion by way of a determinate role or position within the overall communal structure. The communitarian supposition that there *is* a common good is thus upheld in a manner consonant with the existence of diverse points of view regarding that good.

Nor does this alternate version of the communitarian thesis lack historical precedent; for if one looks not to the ancient Greeks or the modern republicans but, instead, to the Latin Middle Ages (a period too often ignored entirely by political theorists) one discovers a theoretical framework, that I label *communal functionalism,* which deflects some of the more serious charges against communitarianism. At the heart of communal functionalism is the claim that the community is, in the first instance, composed neither of individuals nor of citizens but, rather, of functional groupings or parts, arranged according to the nature of their contribution to the communal whole. This is essentially an organic conception of social life but one with certain distinctive features; for it defines membership in the community as a direct result of one's contribution, through the performance of a given function, to the well-being or health of the whole. Yet although types of contribution to the community are distinguished—and specialization of function (or division of labor) thereby posited—there is no attempt to postulate a hierarchy of functions in order to exclude some part or parts from a place in public life or to cast aspersions on the qualifications of certain groups to exercise a basic role in government. On the contrary, any person from any segment of society that contributes in any way to the welfare of the communal unit is thereby accorded an equal capacity to gage the needs of the community and to perform one's functions accordingly. This means that the community must guarantee a basic liberty of judgment and expression for all its members, regardless of the tasks that they perform.

In contrast with the mode of specialization typical of Plato's ideal polis, the division of responsibilities according to the communal functionalist model entails the diffusion of concern about the good of the whole. Communal functionalism does not transform ruling into a specialized art, nor does it lead to a corporate pluralism that limits the participation of the various parts of society to promotion of their narrow self-interests within a public forum. Rather, it maintains that each segment of the community, precisely insofar as it contributes to the good of the whole in a particular fashion (by performing some specific function), also thereby acquires a general duty toward the maintenance of the social organism. There are no grounds upon which the exercise of this general duty may legitimately be suppressed; the maintenance of the common good, as opposed to rule in accordance with some private or partial set of interests, is deemed to depend upon the ability of each and every member to determine autonomously the standards by which the healthy community is to be governed.

The approach to the relation between the community and the individual afforded by communal functionalism seems to resolve many of the valid concerns voiced by liberal individualists about the sort of holism implicit within communitarianism, since it acknowledges the indispensability of individual rational judgment in regard to the public welfare. Claims of superior status or authority in themselves carry no preeminent weight in the process of public deliberation and decision making. Communal functionalism resists efforts to override or undercut personal choice. The lack of respect for individual intellect and volition of which communitarianism sometimes stands accused is not a factor. The terms of the model are violated by the denial to any segment of the community of the opportunity to dissent or to offer rational arguments about matters touching upon the interpretation of the public good.

Expression of the basic principles of communal functionalism abounded during the Middle Ages.[2] Generally speaking, however, the doctrine was articulated within either an organic or a civic framework. The first view is found in the mid-twelfth-century philosopher and churchman John of Salisbury, who concentrates upon the interrelation of parts constituting the "physiology" of the body politic. The second is expressed by Marsiglio of Padua, an early fourteenth-century scholastic philosopher and antipapal polemicist, who appeals to the formal consent of the constituent parts of the community. I shall examine both the diversity and unity present within communal functionalist discourse: communal functionalism is not a single, narrowly specified set of doctrines but a general matrix, a paradigm, within which much creative theoretical discussion occurred during the Middle Ages. I also seek to evaluate some of the lessons that communal functionalism may be capable of contributing to current communitarian debate, as well as to assess potential limitations within the framework.

COMMUNAL PHYSIOLOGY

Although the organic metaphor comparing the political community to a living creature was widely employed during the Middle Ages, it is generally agreed that John of Salisbury's version of the analogy, presented in his major treatise, the *Policraticus* (completed in 1159), was among the most innovative and influential (Dutton 1983; Struve 1978). John begins with the simple observation that the commonwealth

may be likened to a "body which is animated" (1909, vol. 1, 282; idem 1990, 66).[3] The differentiation of the offices of political society may thus be represented in a manner analogous to the distinction of the parts of the human anatomy. Like all bodies, the commonwealth is guided by a soul; John asserts that the place of the soul belongs to "those who direct the practice of religion" (1909, vol. 1, 282; idem 1990, 67). Yet the clerical soul of the polity is not, strictly speaking, a member of the commonwealth, just as the eternal human soul is not coextensive with the mortal physical organism it directs. The political creature is an essentially secular entity. Thus, the body politic, properly speaking, is ruled by the prince, who "occupies the place of the head" (1909, vol. 1, 298; idem 1990, 69). At the heart of the commonwealth lies the senate, composed of the counselors whose wisdom the ruler consults. The senses correspond to the royal judges and local agents such as sheriffs and bailiffs who exercise jurisdiction in the king's name. The financial officers constitute the stomach and intestines of the body, while the two hands are formed by the tax collector and the soldier. Finally, John compares the feet to the artisans and peasants "who erect, sustain and move forward the mass of the whole body" (1909, vol. 1, 282–83; idem 1990, 67).[4] Each of the parts of the organism, according to the *Policraticus*, has its own definite function, which is fixed by its location within the overall scheme of the body. None of the members may be excluded or removed without serious damage to the whole.

John's functional depiction of the community was not, as is sometimes supposed, primarily intended to justify hierarchy and division within society. Rather, the body politic is invoked in the *Policraticus* as the expression of a principle of cooperative harmony through which otherwise disparate individuals and interests are reconciled and bound together (see my earlier study, Nederman 1987a). John adopts a "physiological" approach to the political organism in the sense that he emphasizes "organs cooperating by virtue of voluntaristic categories such as common purpose" (Jung 1985, 172). Throughout his writings, he regards "reciprocity" as the salient characteristic of natural and social systems (John of Salisbury 1929, 5–8; idem 1955, 181). The *Policraticus* insists that "there can be no faithful and firm coherence where there is not a tenacious unity of wills and a virtual joining together of souls themselves. If this is missing, men harmonize their works in vain, since deceit advances into catastrophe without an attitude of helpfulness" (1909, vol. 1, 308–9; idem 1990, 77). All the parts of the body, in other words, must be truly dedicated to a common or public welfare which supersedes the aggregate private goods within the polity. The ruler and magistrates are advised to attend "to the utility of all" (1909, vol. 2, 73; idem 1990, 136); the feet are counseled "to concentrate on the public utility in all matters" (1909, vol. 2, 59; idem 1990, 126); and in general, "all the members" are expected to "keep watch over the public advantage" (1909, vol. 2, 71; idem 1990, 135). The security of the body politic can only be maintained by means of a joint commitment to a public good that benefits every part without distinction, so that "each individual may be likened to a part of the others reciprocally and each believes what is to his own advantage to be determined by that which he recognizes to be most useful for others" (1909, vol. 2, 59; idem 1990, 126). John's political body is one in which, beyond all social differentiation, there is "mutual charity reigning everywhere" (1909, vol. 2, 86; idem 1990, 142), because all segments are attuned to the same enduring common purpose encompassing the valid interests of the whole. Unity follows from cooperation, and cooperation stems from the existence of a good shared by the entire community and each of its members.

What ensures the presence of a cooperative spirit within diverse sections of the community? How is fragmentation to be avoided? The answer stems from John's identification of the "health" of the body politic with the practice of justice by the organs and members. Indeed, his definition of the common good in terms of justice contains a recognition of a correlative obligation on the part of all members of the commonwealth: "Inasmuch as the duties [*officia*] of each individual are practised so that provision is made for the corporate community, as long as justice is practiced, the ends of all are imbued with the sweetness of honey" (1909, vol. 2, 63; idem 1990, 131). Every organ of the body must conduct itself according to the dictates of justice if the polity is to exist as a corporate whole. This is implicit in John's conception of justice. Following Cicero's formula in *De officiis* (1913, 25), the *Policraticus* asserts: "Justice consists chiefly in this: do not do harm and prevent the doing of harm out of duty to humanity. When you do harm, you assent to injury. When you do not impede the doing of harm, you are a servant of injustice" (1909, vol. 1, 277; idem 1990, 62). The essence of justice pertains to a responsibility toward others; this responsibility is not simply constituted by a negative obligation to refrain from the commission of injury but entails a positive duty to protect others from harm as well. Justice is inherently productive of social cooperation, whereas injustice necessarily tends to human disharmony and social disintegration. Thus, John believes that justice is a generalized virtue; for since justice is the salient characteristic of the common good, it determines how each of the bodily members performs its functions. Not only kings and magistrates but even the lowliest of parts are viewed by John as potential agents of public justice (see Nederman 1987a, 220–21). This is established by the very fact that the function of each segment of the organism is absolutely necessary for the well-being of all the others.

BODY AND INDIVIDUAL

Supporting John's argument for the wide diffusion of responsibility for the maintenance of justice is the view that all members of the body politic possess the

basic faculties necessary for guarding and protecting the common good. John upholds the ability of each part of the community to discover for itself what is beneficial and just and to act thereupon. This principle accords with the general terms of his account of moral decision making. The *Policraticus* claims that "discretion with regard to place, time, manner, person and cause . . . most readily separates out" virtue from vice (1909, vol. 2, 316; idem 1990, 187). In deciding how to conduct oneself, each individual must consider all relevant conditions and choose the course of action which is appropriate in its context. Thus, John believes that moral reasoning is ultimately a matter of individual calculation and determination (Nederman 1986, 133–37). To do what is just, one must apply one's general knowledge of justice to the particulars of the case and decide upon the correct action. As applied to the political community, John's prescription of moral autonomy means that all organs and limbs (rather than simply a specialized few) enjoy the capacity to engage independently in the process of moral reasoning about matters of justice.

In turn, independence of moral judgment depends on personal freedom of thought and expression. The *Policraticus* claims that the very precondition for rational decision making is personal liberty. Consequently, John posits an intimate connection between virtue and liberty. He remarks that "virtue does not arise in its perfection without liberty and that the loss of liberty demonstrates irrefutably that virtue is not present and therefore that anyone is free according to the virtue of his dispositions" (1909, vol. 2, 218; idem 1990, 176).

Specifically, the relation of liberty to virtue stems from the need for individuals to be free to make their own circumstantial determinations about morally good actions. Liberty thus, according to John, consists in the careful formulation of sound rational judgments about the most appropriate route to virtue: "Liberty, therefore, judges in accordance with the free will of the individual" (1909, vol. 2, 217; idem 1990, 175). As applied to matters of justice, this doctrine teaches that only people able to make free use of their rational faculties can perform their duties toward the body politic; and since the health of the organism requires that every limb and organ must share full membership in the body, the free judgment of each segment of the community must be encouraged and defended. Hence, one of the prime functions of the good prince is the defense of the liberty of his subjects; and the tyrant is characterized as the ruler who enslaves the other parts of the community by denying to them their independence of judgment (1909, vol. 2, 345; idem 1990, 191).

John takes this freedom of judgment quite seriously. He argues that liberty of expression and action is only meaningful when the opportunity exists to make it effective. Therefore, he encourages virtuous men to denounce the wrongs and injuries committed by others: "Man is to be free and it is always permitted to a free man to speak to persons about restraining their vices" (1909, vol. 2, 224–25; idem 1990, 180). Likewise, he urges wise men to heed patiently the criticisms offered by their fellows (1909, vol. 2, 219, 223; idem 1990, 177, 179). Liberty of judgment is to be extended without regard for rank: the *Policraticus* praises rulers and great men who allow social inferiors to vocalize condemnations of the vices of their superiors (1909, vol. 2, 219–23; idem 1990, 177–80). John even recounts his own highly censorious comments to Pope Adrian IV about the evil conduct of the Church and the papal curia (1909, vol. 2, 67–71; idem 1990, 132–35). In sum, John ties the good order of the community to the personal freedom of its members. In his view, the protection of liberty of thought safeguards against the rise of tyrants and enemies of the common good.

John is willing, at least under exceptional circumstances, to extend this individual discretion into the realm of direct political action against evil rulers. He distinguishes between two different circumstances that warrant different types of public concern, namely, the commission by rulers of minor offenses (which can be tolerated by the community) and flagrant crimes, which endanger the well-being of the body politic (1909, vol. 2, 78–79; idem 1990, 140–41). In the first instance, citizens should freely express their disapproval and recommend corrective measures but should not challenge the position of the governor. In the second case, when the common good is threatened by a tyrant, members of the political organism ought—indeed, are bound by the functionalist terms of their inclusion within the political order—to take it upon themselves to deflect injury to the public welfare. This claim forms the basis for John's famous doctrine of tyrannicide (Nederman 1988). The *Policraticus* regards tyranny to be a crime against "the body of justice" (*corpus justitiae*). Consequently, inasmuch as one enjoys membership within the political community, one has an ultimate responsibility to eradicate the incorrigible tyrant. John asserts that "whoever does not prosecute [the tyrant] transgresses against himself and against the whole body of the secular republic" (1909, vol. 1, 233; idem 1990, 25). No overt limits are set on the social position or station of the tyrant slayer. Rather, because action against a tyrant stems from the general duties grounded in one's functional position within the community, any of the members is equally authorized to act. The example of a duty to kill the tyrant may be the most extreme case of a participatory duty that nonetheless requires individual freedom of judgment; but it illustrates the strength of the link between personal liberty and the active involvement of each and every member of the body in defending the good of the political organism.

That John seeks to allow for meaningful individual choice in the context of an argument for a high degree of political cooperation is perhaps the most striking feature of his argument. This is rooted firmly in his communal functionalist outlook. Inasmuch as everyone has a task within the body politic, each must be free to act as is seen fit for the sake of—or at least in a manner commensurate with—the common good.

Likewise, one is prohibited from acting against the public welfare. Liberal individualists should derive some satisfaction from this approach because it permits each person to deliberate autonomously about how the common good is best to be served and leaves it to each to act accordingly; yet this approach vindicates the central tenet of the communitarian cause by insisting that members of the body politic have a duty (founded on the reciprocal benefits that they and the community receive from each other) to exercise their discretion with regard to the utility of the whole. Members are not free to abdicate their responsibility to judge for themselves. Indeed, the abdication of such autonomy is for John the sign of a servile populace, one incapable of virtuous conduct (which must be freely chosen) and ripe for submission to a tyrant. John shares with liberal thought a strong distaste for tyranny but views tyrannical government itself as often the product of tolerance for withdrawal from the public sphere and refusal to consider the common good. On John's physiological account, such repudiation of responsibility toward the community is not merely self-regarding. It means that the functions one performs may no longer be counted on to augment the welfare of the rest of the community. It is exactly in this sense that the primacy of function serves to mediate between individual and community: it fixes both the legitimate limits of the public authority over personal choice and the nature of the contribution that the parts of the body may rightfully expect of one another.

A COMMUNITY OF PARTS

It should not disturb us too greatly that John of Salisbury does not employ a more overtly civic language in his account of the body politic and its parts. The vocabulary of citizenship was not so familiar to him as it would be to thinkers in later centuries. It seems clear that the features of membership in the organic community as described by the *Policraticus* are largely coextensive with a conception of civic identity. More disturbing, perhaps, is the absence from John's theory of any formal or regularized institutional mechanisms for the expression and discussion of individual judgments in regard to the common good. He takes for granted an entirely personalized framework within which individual discretion translates solely into individual action. Collective decision making, based on rational discourse or persuasion, does not enter directly into his scheme.

By contrast, the version of communal functionalism advocated by Marsiglio of Padua in his major political work, the *Defensor pacis* (completed in 1324), is not susceptible to the same criticism. With the benefit of more thorough knowledge of ancient political ideas and practices, as well as direct experience with the communal governments of his native Italy, Marsiglio presents a theory oriented more self-consciously toward the concepts and language associated with the civic body (*civitas*); yet Marsiglio shares certain fundamental premises about the composition of the community with John of Salisbury. In particular, Marsiglio's *Defensor pacis* gages the level of the "development" or "perfection" of a community according to the extent of the differentiation of its parts, where such parts are understood to be the specialized functional divisions within society. It describes a gradual process through which human beings, acquiring experience and improving their reason, discover all the tasks necessary for a satisfactory existence (1927, 10–11). Once this is achieved, it is possible to speak of the "perfect community, called the civic body, with the differentiation of its parts" (p. 11). The variegation of functions typifies the mature community in comparison with more primitive forms of association.

Marsiglio thus places a premium on reciprocity. The intercommunication of functions should be the goal of the multiplicity of parts: "Since diverse things, which cannot be procured by men of one order or office, are necessary to those who desire to live sufficiently, there needed to be diverse human orders or offices within this association, exercising or procuring such diverse things which humans require for sufficient life. And these diverse human orders or offices are none other than the many and distinct parts of the civic body" (1927, 14). The final cause of each member is the well-being of the whole community, rather than simply its narrow self-interest (p. 25). Marsiglio enumerates (in imitation of Aristotle's division in *Politics* 7) the six basic segments of the perfected community, namely, the agricultural, artisan, military, commercial, religious, and judicial (or ruling) parts (p. 15). Every one of these functions is revealed to be indispensable for the sufficient communal life which all human beings naturally desire (pp. 15–20). The parts are consequently regarded as parts *of the civic body*: they "are properly called offices [*officia*], as though services, since, considering that they are established in the civic body, they are ordered toward human service" (p. 27). These parts do not perform their functions in isolation but, rather, in the context of, and in relation to, the other elements of the civil community (pp. 8, 100–101).

Therefore, Marsiglio declines to draw the Aristotelian conclusion that the performance of certain types of functions (i.e., the manual tasks associated with farming, the crafts, and commerce) disqualifies persons from membership and participation within the civil order (Aristotle *Politics* 1328a; cf. Marsiglio of Padua 1927, 15). Instead, he infers from the need for diversity within the community that all the parts must be accorded a basic civic identity: this is the only way to avoid the disordering of the communal whole. The status of citizen encompasses "mechanics," craftsmen, and artisans just as surely as it does the leisured and educated elites (Marsiglio of Padua 1979, 182; see also Black 1984, 92–93).[5] The salient criterion for citizenship is one's contribution to the community judged along functional lines.

In order to ensure the smooth and harmonious

operation of the various parts, it is necessary to establish laws and to appoint rulers who will enforce the standards which have been legislated; for otherwise, should conflict occur among the parts of society, there would be no fixed procedure for resolving it, and the community would dissolve into quarreling factions (Marsiglio of Padua 1927, 13, 73–74). Such laws and their enforcement must be entirely conducive to the common benefit lest some segment of the communal whole regard itself to be slighted or oppressed and withdraw its contributions. This means that every effort must be made to guarantee that public decision making is not inclined to favor or advantage one part at the expense of others (pp. 53, 58). In Marsiglio's view, the appropriate means of assuring this equity (and thus of harmonizing the disparate interests within the community) is the active participation of all citizens in the selection of rulers and the authorization of laws. Of course, the *Defensor pacis* does not deny that some parts of the civic body are better prepared to perform certain public tasks. For instance, he leaves the framing of laws to those whom he terms *prudentes*, that is, wise men of leisure (pp. 48–49); yet he explicitly rejects arguments that such men are competent to impose laws upon or govern the community in the absence of the universal assent of the citizens.

COMMUNAL CONSENT

The exercise of consent forms the very heart of the Marsiglian conception of citizenship (Nederman 1990, 1991). The community must be the final and binding determinant of its own welfare; thus, it must ultimately approve all laws by which it is governed, as well as the rulers who enforce those statutes. When the body of citizens enacts a law communally, through the consent of every (or virtually every) citizen, there is no question about whether the legislation expresses and accords with the public welfare. Marsiglio's theory of consent leads him to deny the claim that the vast mass of the citizen body is bound to be utterly vicious and incapable of discovering its own best interests. On the contrary, the *Defensor pacis* insists that "the entire assembled multitude can discern and will the common justice and benefit with greater prudence than any part taken separately" (1927, 58–59). There are no grounds for circumventing the mechanisms of consent: all decisions about public affairs, such as the implementation of new laws, must be placed before the whole citizen body in order to receive authorization. While acknowledging the usefulness of the law-framing function of the *prudentes*, Marsiglio regards this skill as a specialized one and therefore not comparable to the general capacity of each citizen to review proposed legislation for himself (p. 60). The citizen's place in the legislative process is comparable to the audience's position in relation to a work of art or technical endeavor. While only a few enjoy the creative or intellectual prowess and skill required to paint a canvass or design and build a house, it is within the grasp of almost every person to appreciate and judge the final product (p. 56). Although most individuals possess neither the leisure nor the education to discover and frame legal standards, Marsiglio believes that they "nevertheless participate in the understanding and judgment of practical matters" (p. 58). To deny such a full rational capacity to citizens would be to subscribe to the view (denounced by Marsiglio) that some segment or group enjoys privileged access to knowledge of the common good and thus is competent to rule and legislate without general consent.

The exercise of this basic power of reason must be matched by the presence of personal liberty if the consent of the civic community is to be effective. For consent itself assumes that the citizen is completely free to judge for himself the potential defects or inadequacies of proposed laws or magistrates. If a citizen is denied such liberty to formulate his own opinions and to express them, then genuine consent will not occur and statutes will be enforced that do not reflect the actual determinations of the community. The *Defensor pacis* proclaims: "Every citizen must be free and not endure the despotism of another, that is, servile lordship. This would not happen if some one citizen or a few of them made the laws according to their own authority above the corporation of citizens" (1927, 52). In the absence of consent, the governance of the polity would be reduced to the domination of some partial segment of the community and the freedom of citizens would be denied. Indeed, civic status disintegrates in the absence of freedom, given that citizenship itself for Marsiglio is constituted primarily by the active exercise of the public duty of consent. Each citizen is the necessary bearer of a capacity for autonomous rational judgment that must be freely exercised if the community is to be properly and peacefully ordered.

At the same time, the liberty underlying civic assent is not purely arbitrary: when approving law, citizens must consult not their own private wills but only the good of the communal body. Here, the duty of citizens becomes more obvious in Marsiglio's account. He views the original creation of civic institutions as a cooperative process in which humans came together in express recognition of their own naturally sociable propensities (1927, 353). Because political society was created by such consent, its members must perpetually accede to the terms of their own continued association (including their laws and rulers) in a sort of recapitulation of their initial entry into society (Nederman 1991, 32). Thus, the duty to engage actively in the consenting process stands at the heart of the existence and perpetuation of civil institutions and relations. Should citizens permit their private wishes to take precedence over the needs of the community, the very fabric of the political order itself would begin to come unraveled. Indeed, a "citizen" who prefers private aims to the public welfare ceases to be a citizen at all, by refusing to acknowledge that basic duty toward the common good that is the defining quality of citizenship.

Hence, just as in the organic account of John of Salisbury, Marsiglio posits a notion of liberty that is inalienable yet communally grounded. No one may deny freedom of judgment to the individual citizen. To do so would be to harm the well-being of the community by excluding some of the contributions that are constitutive of its perfection. At the same time, however, the liberty that rests on function is not essentially private or self-regarding. The exercise of free choice, instead, reflects public needs and goods, so that citizens are not free to remove themselves from the political realm while still enjoying the other advantages of civil life. In a sense, there would be no liberty if there were no community; but the reverse is also true.[6] Thus, personal freedom of judgment and communal duty are both complementary and mutually reinforcing in Marsiglio's thought. They receive a formalized and institutionalized expression in the process of civic consent to laws and rulers. This seems to be the crux of the divergence between a Salisburian and a Marsiglian account of the functionally based community. Where John leaves the decision-making process entirely up to the individual's reflection and discretion, Marsiglio permits—indeed, encourages—interaction and discussion as a prelude to consent. The members of John's polity are essentially moral agents whose scope of concern is personal virtue as it impinges on the body politic. Marsiglio's parts are more fully citizens in the sense that they assemble to speak and listen to one another prior to determining matters of public welfare.

LESSONS AND LIMITATIONS

In their careful attention to the functional bases of human diversity, both John and Marsiglio are attuned to the important implications of difference for the life of the community. They realize how easily the distinct elements of the communal whole can lose sight of the common good that binds them and begin to concentrate upon the realization of purely private goods, which inevitably has a disintegrative effect on civic identity. On the one hand, their essential approach admits of the permanence of diverse experiences and orientations. Distinctions of function (coextensive with divisions of wealth, status, leisure, etc.) are deemed to be fixed features of human existence, underlying all "ways of life" that a community might seek. Those who look to the community to break down the division of labor (or the social consequences of that division) will not find the communal functionalist framework rewarding. On the other hand, by starting with the concrete basis for the life experiences of most people, John and Marsiglio are better prepared than many communitarians to address the real foundations of divergence between differing perceptions of interest (common or private) within a community. After all, "the citizen" is as capable of being rendered an abstraction as "the individual." By occasionally overlooking the extent to which the boundaries of the self are set by functional circumstances (modes of employment, education, status and culture), communitarians readily open themselves to the charge of formulating "civic virtue" or "citizenship" as a universal category standing outside the historical and empirical situation of individuals within specific civic bodies.

Communal functionalism resists this tendency to abstraction by taking as its point of departure the tasks and activities which make possible a given society at its particular level of development. Citizenship is rooted in what individuals *do*, rather than what they are. A person is a citizen by virtue of the performance of certain functions that contribute to the welfare of the whole. (A person is a *good* citizen when these functions are performed in accordance with the needs of the community and when concern is evinced for the communal good by way of other sorts of public behaviors.) The duties of citizens are consequently not static, but are contextualized according to the immediate requirements of the public utility as recognized and accepted by their own free judgment. The community (and therefore its good) is not a simple unity but is, instead, a multiplicity ordered toward a single end—as Marsiglio, indeed, tries to explain (1927, 94–96). Without surrendering a doctrine of substantive common good, communal functionalism permits the communitarian to construct a framework within which to understand the diversity of interpretations of that good and to appreciate the necessity of the diversity itself.[7]

Thus, a robust theory of participation is implied by communal functionalism; for since every distinct function within society generates a unique and valuable point of view regarding the common good, there must be some mechanism for the expression and harmonization of these visions—in essence, an outlet for sharing perspectives and appreciating the totality. Communal functionalism counsels tolerance for divergent interpretations but also an openness to the realization of one's interdependence in relation to other segments of the community. The participatory process ensures some exchange of alternate visions in a fruitful manner, in the sense that it will encourage a cooperative consensus, to which the viewpoint of every part or member contributes. Such participation may be informal (the freedom of speech and criticism proposed by John of Salisbury) or rigorous (Marsiglio's insistence on individualized consent); but it cannot be foresworn. The liberty of judgment founded on the diversity of functions relating to the common good necessitates, for both John and Marsiglio (and surely for all accounts of communal functionalism), some effective avenue for its communication and realization.

It follows that communal functionalist theory simultaneously strengthens the concepts of the individual and the community. This may appear self-contradictory, in light of our automatic tendency to view any reinforcement of the status of the individual as a challenge to the community and any bolstering of communal values as an infringement on the individual; but for the medieval world, these two factors

were not mutually exclusive. We have seen that John and Marsiglio conceived of the governance of the well-ordered community as subject to evaluations of goodness, knowledge of which was accessible to every member of the community in virtue of one's function therein. Consequently, it followed for them that the more freedom enjoyed by individuals and the more they were allowed to employ their powers of reason, the stronger would be their commitment to the communal good and to the duties entailed thereby. In other words, John and Marsiglio were satisfied that free individuals, left to the devices of their own rational powers, would generally reach conclusions consistent with, and conducive to, the public welfare.

The contrast between these medieval ideas and the liberal individualist framework is not only striking but instructive. Beyond the inclination of liberalism to doubt the existence of a determinate common good apart from the aggregate goods of individuals, the liberal emphasis upon private rights and rational egoism reverses the medieval concentration upon public duties and moral reason. It might be argued that liberalism is better equipped to protect the individual from domination by the community; but the case for this is weak—empirically, as well as theoretically. On the communal functionalist account, a well-ordered political system must protect and encourage the liberty of its subjects to reason and judge for themselves—and even to judge wrongly, provided that the public good is not harmed thereby. By contrast, for a philosophy that lauds the individual, liberalism demonstrates a remarkable lack of confidence in the ability of each person to employ reason in order to form political judgments. In sum, liberalism's claim to be the unique defender of the individual against the community is difficult to sustain in light of the medieval stress on personal liberty and individual judgment as necessary concomitants of civic life.

It may still be objected that in contrast with liberal individualism, the communal functionalist approach remains far too restrictive in its conception of the diversity of human goods. For example, both John and Marsiglio adopt purely public definitions of function: what counts as a function is whatever contributes to the socioeconomic well-being of the community. This would seem to exclude *private* roles—in particular, activities associated with the family—from the arena of the common good and thus to separate those who perform private functions from a place within the political arena. That the upholding of the public/private distinction and, with it, the maintenance of patriarchal beliefs both about the family and the role of women within it was intended by both John and Marsiglio is clear (Nederman 1990; Nederman and Lawson, 1987). Thus, to make communal functionalism viable in a late-twentieth-century context, it is necessary to rethink the distinction between public and private realms as it relates to the definition of the common good. My own judgment is that one can construct a plausible case that so-called private functions in fact fall within the ambit of the common good and hence that they ought to be included within the range of functions that form the basis for the assignment of civic identity.

More troublesome, perhaps, is the specification of how a function contributes to the common good and whether ostensive absence of function is adequate to remove a person from involvement in communal life. Most obviously, this pertains to the person who is unemployed or homeless or who otherwise does not perform any of the functions necessary for the sustenance and health of the social unit. Is such an individual thereby rendered unfit for the exercise of civic responsibilities? The same sort of question can be posed for severely impaired or disabled persons who are unable to undertake the activities which contribute to the common good. Just as significantly, by what or whose standards are determinations to be made as to whether a person (or the function a person performs or can perform) contributes to the benefit of the community?

No more than a tentative response can be offered at present to this complex set of problems. It seems unreasonable to maintain the view that only when actually engaged in some activity that contributes to communal well-being is a person to be counted as a member of the community. Indeed, this would be rather like saying that one only possesses mathematical knowledge when one is performing some mathematical operation. Knowledge is more profitably regarded as a disposition to know rather than as an act of the knower. In an analogous manner, one's status as a member of a political community may be treated as a disposition to engage in some function that contributes to the common good. Indeed, Marsiglio of Padua expresses the matter in essentially this way when he defines the different parts of the civic body as *habitudinem*, that is, dispositions to perform the various functions necessary for the health of the community (1927, 26–27). Consequently, anyone who has a *capability* or *potential* to add in any way to the good of society cannot be placed outside the processes attached to the "politics of the common good." Moreover, only the community itself (either directly or by means of its authorized government) can decide which functions contribute to its well-being. Generally speaking, those which have not been deemed illegal constitute the functions that a communal body accepts as conducive to its own good. This is not to say that mistakes cannot be made in the enforcement of the values common to the society, but such errors have a way of being rapidly overturned and the functions associated with them reinstated. (One thinks of Prohibition Era America, where the legal abolition of the activity of liquor production did not conform to the public perception of its own good and was ultimately negated—in practice by bootlegging and in law by repeal of the Twentieth Amendment to the Constitution.) Communities and their members are quite able to judge the functions necessary for their own welfare. Ironically, it tends to be liberalism, the self-proclaimed

proponent of the individual, that doubts this competence.

CONCLUSION

The Latin Middle Ages was by no means the only period of Western history to make use of a functionalist approach to political community. Functionalism is at the core, for instance, of the venerable "guild" tradition of thought (Black 1984; Nederman 1987b). But concentration on diversity has tended to lead in one of two directions antithetical to community: (1) the conflation of function with narrow self-interest, so that different parts are regarded to be essentially in conflict, or (2) the equation of function with hierarchy, so that distinction of task is employed as a principle of subordination and rule. By contrast, medieval communal functionalism as articulated by John of Salisbury and Marsiglio of Padua looks to the diversity of activities as the logical foundation of a community of free individuals. They view function as neither inherently self-regarding nor hierarchical. Rather, reciprocity and interdependence, the necessary foundations of a self-sustaining community, flow from a diversity of members or "offices" within the polity.

That this position forms a distinctive feature of medieval thought may result from the conjunction of a series of largely incongruent intellectual and historical factors. By the twelfth century, the classical and "Aristotelian" (MacIntyre 1985, 146) notion of the common good as the primary and animating feature of public life was in wide circulation among medieval thinkers, one by-product of a general reawakening of interest in ancient philosophy, history, and law (Benson and Constable 1982). This was only reinforced by the recovery and translation of Aristotle's *Politics* in the midthirteenth century. The learned person of the Middle Ages confronted a range of classical sources that focused upon communal identity and the dependence of virtue upon community. At the same time, Christian teaching about the individual's will and the centrality of one's personal relationship with God pulled in the opposite direction: ultimate virtue and happiness arose out of individual judgment and choice, rather than out of the dictates of any common good. Thus, a command based on the public welfare to violate the tenets of one's religious faith was to be met with refusal and (passive) resistance. The individual's will or discretion was deemed to be sovereign in relation to the external pressures and dictates of the community. As MacIntyre has recognized, medieval authors were confronted with "a culture in which human life was in danger of being torn apart by the conflict of too many ideals, too many ways of life" (1985, 165). This cultural situation was compounded by the diversity of social and political arrangements, of economic activities and legal powers (the particularization, or "parcellization," of authority, as historians often call it), which produced tensions that no thinker of the Latin Middle Ages could ignore (Fasolt 1991, 80–83). It was unthinkable to sweep away the divisions within the medieval world; rather, if a civic identity was to be fashioned, it had to start with the historical reality of disparity.

The attraction of the communal functionalist framework was its capacity to cope coherently with these demands upon political and social thought. It inserted the individual into a communal setting while maintaining a meaningful role for personal volition and achieved this precisely by turning diversity of function (divergence in "ways of life") into a virtue—indeed, a necessity. In a certain fashion, the problems confronted by contemporary political theory are not that distant from those which faced John or Marsiglio. Individualism (albeit of a secular sort) constantly resists efforts to identify a common good capable of stimulating expressions of civic virtue; yet the appeal of individualism is spurred on by the unhappy collectivist consequences of too great a concentration upon communal needs at the expense of personal choice and judgment. This similarity of predicament, as well as the scope and power of the communal functionalist model itself, commends a reading of authors such as John of Salisbury and Marsiglio of Padua to current communitarian theorists. Such a reading may bear surprising fruit.

Notes

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1. The term *communitarian* has been applied to theorists whose ideological and philosophical commitments vary so widely that those included under the label resist treatment as members of a "school of thought" advocating a core set of doctrines. See the overviews by Gutmann (1985) and Lasch (1988). Communitarianism most accurately denotes a theoretical concern for the community as a significant force in the political experience of individuals or as a constitutive feature of human identity. Beyond this and a consequent aversion or opposition to liberal individualism, few generalizations seem possible. Indeed, Charles Taylor suggests that it will probably be necessary to dispense entirely with such labels as *communitarianism* (as well as *liberalism*) before the range of philosophical issues at stake can become fully transparent (1989, 163).

2. Essentially the same doctrine, for instance, has recently been ascribed to William Durandus the Younger, an early fourteenth-century theorist, by Fasolt (1991, 179–84).

3. The first citation is to the Latin text, the second to my translation of it.

4. John, in fact, recognizes that the comparison of the feet to the trades and professions associated with artisans, craftsmen, and workers generally is imperfect if only because of the diversity of these activities: "There are so many of these occupations that the number of feet in the republic surpasses not only the eight-footed crab, but even the centipede; one cannot enumerate them on account of their large quantity . . . because there are so many varied kinds that no compiler of duties ever produced special precepts for each individual type" (1909, vol. 2, 59; idem 1990, 126). He is thus far more appreciative of, and sympathetic to, the actual functions and importance of the lesser members than is sometimes credited.

5. Part of the explanation for this may be Marsiglio's class self-identification. Trained as a physician, he counts his own occupation among the trades associated with the "mechanical" or "artisan" part of the civic body, albeit perhaps the "architectonic" of that part (1927, 17). In turn, mechanics fall within the Aristotelian classification of the *vulgaris*. Since Marsiglio deems himself to be competent to give advice about the good order of the community, it seems unlikely that he would place the *vulgaris* outside of the body of those qualified for citizenship.

6. As Quentin Skinner has argued, some such notion of the connection between liberty and the maintenance of community was at the core of much medieval political discussion (1978, vol. 1, 49–65; idem 1990, 128–34). But Marsiglio perhaps needs to be distinguished here, since he regards liberty in a quite specifically individual sense that is not fully compatible with the more communal, republican-oriented meaning of his contemporaries.

7. Thus, in some ways, communal functionalist theories take over aspects of the pluralist project. The potential for a fruitful relation between pluralist and communitarian thought has been examined by Eisenberg (1991a, 1991b).

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RESEARCH NOTES

POLITICAL BUSINESS CYCLES IN THE PUBLIC MIND

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Numerous studies have sought to discover political business cycles in macroeconomic variables. Although voters' subjective economic expectations have been shown to influence their electoral decisions, no existing research has attempted to uncover cyclical patterns in citizens' economic expectations. Using survey data, I seek to determine whether expectations shift to benefit the incumbent president's electoral interest. The analyses show that the percentage of the public predicting an economic upturn increases before a presidential election. One explanation for the findings is that voters might extrapolate cyclical expectations from macroeconomic conditions that contain election-driven cycles. Yet the analyses show that expectational cycles still appear when the macroeconomic conditions are held constant. I conclude by drawing an explanation without recourse to macroeconomic cycles.

The topic of the political business cycle (PBC) has attracted continuous attention in the field of political economy. Analysts have examined whether and how the incumbent president manipulates the macroeconomy in order to maximize his (or his party's) probability of reelection.[1] Yet few have analyzed whether there are election-driven cycles in voters' expectations that political scientists think have direct effects on their electoral decisions (Kuklinski and West 1981; Miller and Wattenberg 1985). Many economists dismiss analyses of expectations based on the belief that expectations are not directly observable. Thus, they typically work with *assumptions* about expectations. This tendency has also been apparent in existing PBC studies that have focused primarily on election-driven changes in macroeconomic variables, or macroeconomic PBCs. In contrast, political scientists believe that voters make electoral judgments based on expectations that are subjective and more complex than a single assumption postulates. From this perspective, the PBC literature has offered very few clues to understand the linkage between political manipulation and voters' economic expectations. Thus, by examining expectations directly, we can hope to improve our understanding about the public's expectational behavior pertaining to a presidential election.

My chief objective here is therefore to ask whether there are electoral cycles in citizens' economic expectations, or expectational PBCs. I begin by outlining prominent PBC models and show the dynamics of voters' economic expectations under alternative rationality assumptions. Then, I offer empirical analyses of voters' economic expectations, including unemployment and personal financial expectations and consumer sentiment. The analyses find that expectational PBCs do exist and behave in such a way to benefit the incumbent party.

EXPECTATIONS AND POLITICAL BUSINESS CYCLE MODELS

While the casual idea of political manipulation had been around for some time, Nordhaus (1975) was the first to formalize a nonrational model of the PBC on the bases of traditional Keynesian macroeconomic theory and the median voter theorem. The Nordhaus model assumes that the incumbent administration tries to maximize votes and that voters are not fully rational in evaluating the performance of the incumbent administration. Since voting has little economic value to the individual and the reliable forecasts about the future are costly to acquire, retrospective evaluation based on simple facts (e.g., unemployment, GNP growth, and inflation) may be a reasonable way for many voters to make electoral decisions (Nordhaus 1989, 39). In an election, voters reward the incumbent administration for low unemployment (or high growth) and punish it for high unemployment (or low growth).[2]

Given these assumptions, the model predicts that the incumbent administration engages in antiinflation policy early in the electoral term and stimulates the economy as an election approaches. The results are four-year cyclical patterns for key economic variables: unemployment decreases prior to a presidential election and increases early in the next term, whereas inflation rises only after an election (Thus, such inflation has no adverse electoral consequence for the incumbent.) In the Nordhaus model, voters' economic expectations move in parallel with cycles in macroeconomic variables as voters extrapolate "adaptive" expectations from current and immediate past conditions. Because adaptive expectations adjust slowly to changes in environments, expectations revert gradually to the mean from a cyclical peak and trough.

Notwithstanding the popularity of the Nordhaus model, its nonrationality assumption has been called into question by scholars who believe that voters are essentially prospective and sophisticated and use all available information in evaluating the incumbent and forming expectations. According to Chappell and Keech's (1985) path-breaking study on the subject, voters are able to infer a government's economic policy stance from economic outcomes and judge whether policy is too tight (deflationary) or too loose (inflationary). Such policy inferences can help voters

punish the incumbent if preelection policy is too loose. This sensible evaluation of incumbent behavior can effectively eliminate an incentive to manipulate the economy because a cyclical boom manufactured by policy manipulation will be viewed as transitory and will not be rewarded by sophisticated voters.

Yet several theorists have shown that PBCs may not be inconsistent with voter rationality. Although a number of models have been developed in line with rational expectations, the assertion common to these models is that a PBC may occur stochastically if informational deficiency prevents the electorate from correctly forecasting political manipulation. For example, Backus and Driffill (1985) argue that there are two types of administrations defined in terms of the proclivity to stimulate the economy: a "wet" administration that stimulates from time to time and a "hard-nosed" administration that never stimulates. The electorate tries to judge the true preference of the incumbent administration based on its past policy behavior (or reputation). Under these circumstances, the incumbent administration has an incentive to appear to be hard-nosed by not stimulating early in its term and then stimulate in a preelection period. This "unanticipated" policy can generated significant effects on employment and real output prior to an election. Since the electorate learns about the stimulative proclivity of an administration from being tricked once, the administration cannot produce effective political manipulation in two consecutive elections. Hence, a macroeconomic PBC can occur probabilistically, rather than systematically. Another feature of the Backus–Driffill model is that unlike adaptive voters assumed in the Nordhaus model, rational voters can perceive an economic boom as transitory immediately after the actual growth rate accelerates above the natural (or equilibrium) growth rate. Thus, the expectation of a cyclical boom, if it occurs because of stochastic manipulation, will not persist and will quickly dissipate.

Similarly, Rogoff (1990) and Rogoff and Sibert (1988) have developed alternative rational expectations models in which a PBC may occur in government spending and taxes if the incumbent administration has an incentive to manipulate the public's view of the administration's *competency* (defined as the efficiency of providing large amounts of government service at low tax rates). Because such competency can be observed by the public only after a time lag, the incumbent administration has a chance to manipulate fiscal policy before an election in order to appear more competent than it really is. The Rogoff models imply that there may be a real expected (transitory and noninflationary) boom in proximity to an election explainable by a real supply-side effect of a marginal tax cut.

In sum, these PBC models present different scenarios of expectational dynamics under alternative assumptions about voter rationality. Assuming systematically deficient voter information, the Nordhaus model predicts that macroeconomic PBCs may occur frequently in election years. And expectational PBCs may be extrapolated from macroeconomic cycles. In the rational models where voter information is incomplete but only stochastically deficient, macroeconomic PBCs may occur probabilistically, depending on the availability of the information regarding the administration's true objective function. Accordingly, stochastic and transitory cycles may arise in expectations.

EMPIRICAL ANALYSES

Survey data are used to observe citizens' economic expectations directly. The Survey Research Center (SRC) at the University of Michigan regularly asks respondents questions concerning their expectations about future economic conditions.[3] For instance, the SRC has asked the question "Now looking ahead—do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?" and "How about people out of work during the coming 12 months—do you think that there will be more unemployment than now, about the same, or less?" Also, the SRC has produced the Index of Consumer Sentiment, a composite indicator of citizens' general economic outlook, based on multiple questions about their consumption attitudes and various aspects of economic conditions. (The larger the value of the index, the better the economic outlook.) These surveys yield aggregate time series data on citizens' economic expectations, enabling one to observe whether expectational PBCs exist in the recent past.[4]

These expectational series have been chosen because of their electoral significance. Such macroeconomic variables as real personal income and unemployment corresponding to the SRC financial and unemployment expectations series are found to be significantly related to votes in presidential elections or to presidential popularity closely associated with votes (Chappell 1990; Erikson 1989; Fair 1978; Monroe 1984; Hibbs 1987). Also, the consumer sentiment index is shown to be related to presidential popularity (MacKuen, Erikson, and Stimson 1989). Should there be expectational PBCs, such cycles would appear in these three expectational series.

The general equation used to analyze expectations contains predictor variables, including (1) the lagged dependent variable, (2) a partisan variable, (3) economic variables, and (4) an election variable. The lagged dependent variable represents various inertial forces in expectation formation and also justifies serial independence in estimation. The inclusion of a partisan dummy variable is intended to distinguish an election-driven cycle in expectation from a cycle that may be created through partisan alteration in the presidency. Conventional wisdom states that a Democratic administration prefers to reduce unemployment rather than inflation and is willing to accept higher rates of the latter to achieve the former, whereas a Republican administration prefers lower rates of inflation and is willing to pay the price in

terms of higher unemployment (Hibbs 1977, 1987). Such a partisan effect is specified as a dummy variable that equals 1 when a Democratic president is in office and 0 otherwise.[5]

Expectations may well depend on current and past economic conditions. The equation includes "information variables" that may be used by the public to form expectations. Unemployment change and inflation are chosen as the information variables because they are frequently reported in media and are matters of considerable public concern.[6] Lags 0–3 of the variables are entered in distributed lag form into the equation without imposing a restriction on the lag structure.[7]

The most important election variable equals 1 for an election quarter and 0 elsewhere. Following Grier (1989), the election-quarter dummy variable takes 15-quarter, second-order Almon polynomial distributed lag (PDL) to estimate a cycle.[8] The PDL technique imposes a smoothing restriction on the shape of a cycle; and the coefficient estimates for the distributed lags represent the values of the mean innovations in a dependent variable for each quarter of a presidential term. The PDL equations for consumer sentiment and personal financial expectations were estimated for the quarterly period between 1961.1 and 1988.4. The equation for unemployment expectations covered a somewhat shorter period between 1965.1 and 1988.4 due to many missing values in the SRC unemployment data in the early 1960s.

Table 1 presents the regression results. In the three equations, the regression fit is reasonable; and, importantly, all individual electoral coefficients are statistically significant by normal standards and appear to constitute a cycle. One relevant issue is whether the seemingly cyclical pattern is significantly different from a single, fixed intercept (or whether the 16 electoral coefficients are equal). To examine this point, the fit of the PDL model was compared with that of an ordinary least squares model that replaced the election-quarter dummy and the distributed lag structure with a single intercept. For the personal finance and the consumer sentiment models, the F-tests could reject the equality restrictions; but, for the unemployment model, the F-test failed to reject the equality restriction.[9] Thus, the interpretation of the unemployment results requires caution.

Figures 1–3 graph the shapes of the electoral dummy variable's lag distributions derived from the three PDL models. In each figure, a presidential term starts at the first quarter, and an election is held at the sixteenth quarter. Each figure is drawn to represent the mean value of a dependent variable for each electoral quarter computed from the estimates of a distributed lag coefficient and the adjustment coefficient for the lagged dependent variable.

The flexible estimates provide comprehensive pictures of shifts in expectations that are consistent with the PBC story. Financial expectations trough at 38.70% in the eighth quarter and peak at 42.81% in the sixteenth, or the election, quarter, whereas unemployment expectations peak at 23.80% in the sixth quarter and trough at 13.83% in the election quarter. Notice that the measurement unit of the two series is the percentages of the public expecting the improvement in personal financial conditions and declines in unemployment, respectively. These movements are electorally significant in creating crucial effects on contemporary presidential elections (e.g., the 1960, 1968, and 1976 elections) where the winners were determined by a few percentage-point differentials in popular votes. Consumer sentiment shows a similar electoral pattern (troughing at 96.27 in the seventh quarter and peaking at 103.89 in the election quarter). Because the consumer sentiment series is a composite index, it is difficult to interpret the precise electoral effect of the change.[10] Equally important, in the three equations, an anticipated economic upturn appears to last for at least six quarters before a presidential election. The persistence of an anticipated boom seems to suggest that citizens' expectations contain substantial inertial forces. This accords with the Nordhaus nonrational PBC model, rather than with the rational models predicting swift mean-reverting expectational behavior.

None of the coefficients for the partisan variable in Table 1 are statistically significant. As far as people's expectations are concerned, an election seems to have a larger effect than partisanship, contrary to the macroeconomy, where partisanship has been shown to exert a strong disturbance (Hibbs 1987). This result should not be too surprising. It seems logical that in an era when partisan loyalty has been on the wane (P. Beck 1984; Clarke and Suzuki 1991), voters might perceive that incumbency-based electoral politics plays a relatively important role in economic affairs. The scope of this argument, however, is limited only to public economic expectations. Suggesting the relative importance of electoral politics in people's subjective expectations does not reject the utility of the partisan theory in explaining political influences on macroeconomic conditions.[11]

As for economic variables, the linear combinations of lagged coefficient estimates are presented because our interest is in the total effects of the variables on expectations, rather than in the precision of each coefficient estimate. The estimates are largely sensible: unemployment change has a negative effect on consumer sentiment, while inflation has negative effects on financial expectations and consumer sentiment. Inflation also has a positive effect on unemployment expectations due to the stagflation of the 1970s, in which inflation and unemployment accelerated together. In the auxiliary analyses, (not shown), where all economic and partisan variables have been dropped from the PDL models in order simply to describe shifts in expectations, the shapes of expectational shifts have remained virtually unchanged. Expectational PBCs still appear, even controlling for economic conditions that may involve electoral cycles. These results imply that expectational PBCs may not necessarily be extrapolated from current economic conditions. Part of the expectational cycles

Table 1

Flexible Estimates of Electoral Effects on Citizens' Economic Expectations

VARIABLE	PERSONAL FINANCIAL EXPECTATIONS 1961.1–1988.4	UNEMPLOYMENT EXPECTATIONS 1965.1–1988.4	CONSUMER SENTIMENT 1961.1–1988.4
Lagged dep. var.	.17	.55**	.44**
	(.10)	(.09)	(.08)
Inflation[a]	−4.29**	4.24**	−6.31**
	(.64)	(1.29)	(1.02)
Unemp. change[a]	.01	.41*	−.39**
	(.08)	(.23)	(.12)
Partisan dummy[b]	−.56	−2.97	.87
	(.64)	(1.68)	(.82)
Lag distr. of elec.-quart. dummy (elect)			
Elect (t − 15)	34.54**	10.28**	56.13**
	(4.19)	(3.07)	(8.54)
Elect (t − 14)	33.88**	10.54**	55.48**
	(4.09)	(2.89)	(8.39)
Elect (t − 13)	33.33**	10.73**	54.94**
	(4.01)	(2.81)	(8.27)
Elect (t − 12)	32.88**	10.84**	54.51**
	(3.96)	(2.81)	(8.18)
Elect (t − 11)	32.54**	10.88**	54.20**
	(3.92)	(2.85)	(8.10)
Elect (t − 10)	32.29**	10.84**	54.00**
	(3.89)	(2.90)	(8.05)
Elect (t − 9)	32.16**	10.73**	53.91**
	(3.87)	(2.95)	(8.02)
Elect (t − 8)	32.12**	10.55**	53.93**
	(3.87)	(2.98)	(8.00)
Elect (t − 7)	32.19**	10.29**	54.07**
	(3.87)	(2.98)	(7.99)
Elect (t − 6)	32.35**	9.96**	54.32**
	(3.88)	(2.96)	(8.01)
Elect (t − 5)	32.63**	9.55**	54.68**
	(3.90)	(2.92)	(8.04)
Elect (t − 4)	33.00**	9.08**	55.16**
	(3.94)	(2.88)	(8.08)
Elect (t − 3)	33.48**	8.52**	55.74**
	(3.98)	(2.85)	(8.15)
Elect (t − 2)	34.06**	7.90**	56.44**
	(4.05)	(2.75)	(8.24)
Elect (t − 1)	34.74**	7.20**	57.25**
	(4.13)	(2.97)	(8.36)
Elect (t)	35.53**	6.42*	58.18**
	(4.24)	(3.17)	(8.50)
R^2	.68	.79	.90
Standard error	3.20	6.66	4.00
Durbin–Watson	2.07	1.97	2.25

Note: Standard errors are in parentheses.
[a]The estimate is the linear combination of the coefficients for lags 0–3.
[b]The dummy variable equals 1 when a Democratic administration is in power and 0 otherwise.
*$P \leq .05$, two-tailed test.
**$P \leq .01$, two-tailed test.

FIGURE 1

Cycle in Personal Financial Expectations

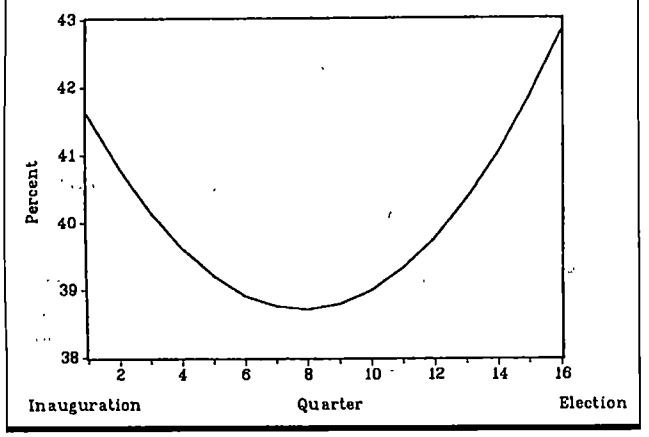
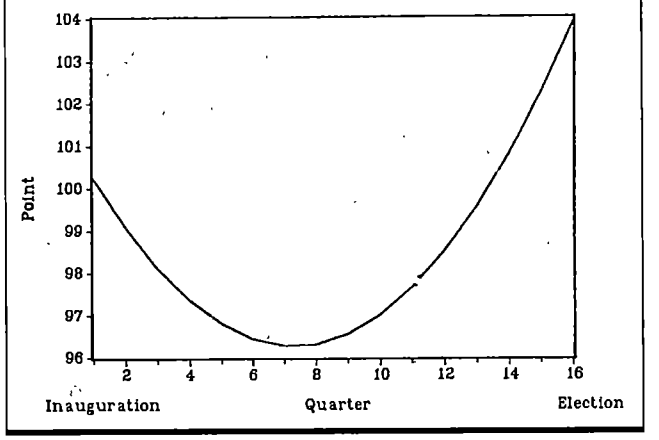


FIGURE 3

Cycle in Consumer Sentiment



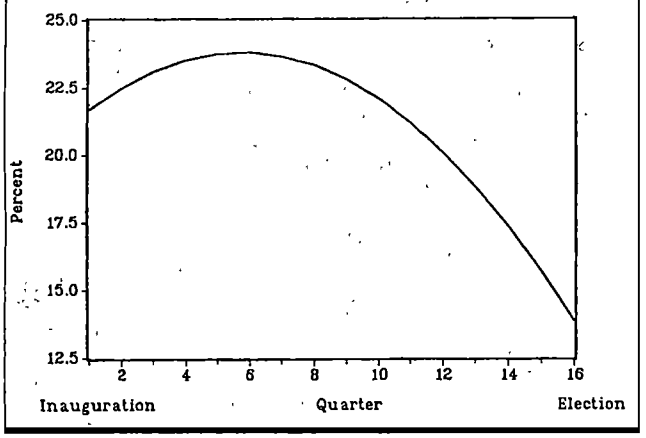
may be attributable to public cognition, independent of outside environments.

To deal with the possibility that these results are an artifact of the statistical techniques employed, I estimated expectational PBCs without distributed lags, using the conventional restrictive specification that models an election impulse, with the *election-year* dummy variable taking the value of 1 for an election year and 0 elsewhere. The ordinary least squares estimates reported in Table 2, columns 1, 3, and 5, support the presence of electoral impulses in the three expectational variables. In all equations, the coefficient for the election-year dummy variable is properly signed and statistically significant, indicating that the electoral patterns in citizens' expectations are insensitive to alternative model specifications.

Both PDL and ordinary least squares analyses involve one potential ambiguity, however. It is not clear whether the expectation of an economic boom is made based on the prospect that the incumbent president (or his party) will stay in power after an election. In contrast, voters may expect such a boom to occur under a new president from the opposition party. To clarify this ambiguity, I have modified the ordinary least squares model by replacing the election-year dummy variable with an interactive term between the election-year dummy and the incumbent party's win probability,[12] predicting a positive sign for this term in the personal financial and consumer sentiment equations and a negative sign in the unemployment equation. The ordinary least squares estimates reported in Table 2, columns 2, 4, and 6, indicate that the coefficient for the interactive term is properly signed as predicted and is statistically significant. The result upholds the hypothesis that the anticipation of an economic upturn can be linked to the incumbent party's win probability.

FIGURE 2

Cycle in Unemployment Expectations



CONCLUSIONS

Unlike previous political business cycle studies examining aggregate macroeconomic variables, my analysis has focused on people's subjective economic expectations that have been shown to influence voting behavior. The search for cycles has been made with SRC survey data using both restrictive and flexible specifications. The results show that financial and unemployment expectations, as well as consumer sentiment, contain four-year cycles adequately timed with a presidential election calendar. And the incumbent party is likely to be an electoral beneficiary of such cycles.

The question is thus, Which political business cycle model can explain these findings best? Because of the limited number of elections examined here, the analyses do not allow us to conclude confidently that the electoral patterns in citizens' economic expectations are either systematic or stochastic or accord either with the Nordhaus nonrational model or the rational models. Nonetheless, slow mean-reverting tendencies found in expectations seems to favor the Nordhaus model. If voters rely on retrospective economic judgments, cycles in their economic expectations may be extrapolated from observations of macroeconomic

Table 2

Restrictive Estimates of Electoral Effects on Citizens' Economic Expectations

VARIABLE	PERSONAL FINANCIAL EXPECTATIONS, 1961.1–1988.4		UNEMPLOYMENT EXPECTATIONS, 1965.1–1988.4		CONSUMER SENTIMENT, 1961.1–1988.4	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	31.51**	31.16**	10.37**	10.63**	49.89**	49.73**
	(3.83)	(3.82)	(2.72)	(2.76)	(7.49)	(7.56)
Lagged dep. var.	.20*	.21*	.55**	.55**	.49**	.49**
	(.09)	(.09)	(.09)	(.09)	(.08)	(.08)
Inflation[a]	−4.15**	−4.02**	4.23**	4.15**	−5.84**	−5.69**
	(.63)	(.62)	(1.25)	(1.24)	(.95)	(.95)
Unemp. change[a]	.01	.01	.39	.40	−.35**	−.36**
	(.08)	(.08)	(.23)	(.23)	(.12)	(.12)
Election-year dummy[b]	2.20**	—	−3.43*	—	2.88**	—
	(.75)		(1.67)		(.92)	
Partisan dummy[c]	−.58	−.51	−2.97	−3.17	.72	.78
	(.64)	(.64)	(1.64)	(−1.91)	(.81)	(.82)
Interactive term[d]	—	3.78**	—	−5.91	—	4.51**
		(1.30)		(3.06)		(1.61)
R^2	.68	.68	.79	.79	.90	.90
Standard error	3.20	3.20	6.63	6.64	4.00	4.00
Durbin–Watson	2.07	2.07	1.95	1.94	2.27	2.26

Note: Standard errors are in parentheses.
[a]The estimate is the linear combination of the coefficients for lags 0–3.
[b]The dummy variable equals 1 for the four quarters of the election year and 0 otherwise.
[c]The dummy variable equals 1 when a Democratic administration is in power and 0 otherwise.
[d]Election-year dummy times incumbent win probability.
*$P \leq .05$, two-tailed test.
**$P \leq .01$, two-tailed test.

PBCs that are shown to exist by Haynes and Stone (1989) and Grier (1989). The troubling point, however, is that expectational cycles exist independent of any cycle in economic outcomes.

This leads us to believe that expectational PBCs may be driven not entirely by macroeconomic PBCs but, in part, by sources other than policy manipulation. It can be suggested that part of the expectational cycles may be attributed to voters' preelection psychologies. Because of widespread preconceptions about reelection politics and the related media barrage, some voters may anticipate that the incumbent party will try to stimulate the economy before an election, even though they have not yet actually observed an economic upturn. Then, the incumbent party may, in turn, be forced to stimulate, since the failure to do so might result in a recession that is electorally detrimental.[13] Accordingly, such preelection anticipation may drive the electoral manipulation of macroeconomic policy, which, if there is sluggishness (e.g., price stickiness) in the economy, can create cycles in macroeconomic conditions.

I have suggested a new way of thinking about political economy. Political business cycles can, and should be analyzed in conjunction with voters' subjective perceptions that count in elections. I have shown that the politicoeconomic forces that influence voters' political decisions are vigorous and are here to stay.

Notes

An earlier version of this paper was presented at the annual meeting of the Midwest Political Science Association, Chicago, 1991. I thank Henry Chappell, Harold Clarke, John Londregan, and William Mishler for constructive comments.

1. Researchers made numerous efforts to search for PBCs in a variety of economic variables, including inflation, unemployment, real gross national product, and monetary and fiscal aggregates. Their analyses have produced mixed results regarding the presence of macroeconomic political business cycles in contemporary U.S. history. (Negative results are found in N. Beck 1982; Golden and Poterba 1980; Hibbs 1987, chap. 8; and McCallum 1978. Positive results are found in Grier 1987, 1989; Haynes and Stone 1988, 1989; Laney and Willett 1983; Maloney and Smirlock 1981; and Tufte 1978. See Alt and Chrystal 1983, chap. 5 and Nordhaus 1989 for reviews of empirical political business cycle studies.)

2. Econometric studies of votes in presidential elections initiated by Fair 1978 and subsequently replicated (Brody and Sigelman 1983; Hibbs 1987, chap. 6; Lewis-Beck and Rice 1982; Tufte 1978) found that voters reward the incumbent party for high economic growth and punish it for low growth. More recently, using improved model specifications Chappell (1990) and Erikson (1989) show that economic effects on votes are even stronger than Fair originally reported. These results

provide a rationale for the party in power to manipulate the business cycle for electoral advantage.

3. Using an equal probability sample design and telephone interviews, the SRC surveys over one thousand adults every month. Based on these data, the SRC produces the quarterly series employed in this study. The SRC notes that its survey involve sample errors of ±5%. Sample errors do not confound the time-series analyses presented in the text because errors remain relatively constant across samples.

4. The literature on voting behavior is not clear about how far ahead voters look in making electoral decisions. The one-year time horizon used in the SRC data seems reasonable because it is very difficult to predict future economic performance beyond this time horizon.

5. Alesina (1987); Alesina, Londregan, and Rosenthal (1990); Alesina and Sachs (1988); and Chappell and Keech (1988) claim that partisan effects may be transitory because markets are presumably able to return quickly to equilibria in a rational expectation environment. Rational voters may predict a partisan-driven economic boom based on the likelihood of the Democratic party's winning a presidential election. Thus, the rational partisan variable equals Democratic win probabilities that were computed from presidential approval. This computing procedure is similar to the one used to calculate incumbent win probabilities (see n. 12). The equation replacing the Hibbs partisan dummy variable with the rational variable also was estimated. The results showed that the rational partisan variable is not statistically significant.

6. Unemployment change is measured as percentage change in the seasonally adjusted quarterly unemployment rate (HURR). Inflation is measured as percentage change in the seasonally adjusted quarterly consumer price index. These economic data were obtained from the CITIBASE data tape. Other than the economic variables included in the model, I also tested macroeconomic policy measures such as M1 money supply growth and change in cyclically adjusted fiscal surplus/deficit. Neither policy measure was significantly related to the dependent variables. Thus, these variables were not included in the equations estimated in the text.

7. This particular lag length was chosen because people are shown to use approximately a one-year backward time horizon in forming their expectations (Feige and Pearce 1976; Figlewski and Wachtel 1981; Gramlich 1983).

8. Using a grid search over 1–4, the second-order polynomial distributed lag was chosen as the value that could maximize the adjusted R^2 of the regressions. The use of the second-order PDL model turned out to be consistent with Grier's (1989) estimated macroeconomic PBCs.

9. For the personal finance and the consumer sentiment equations, the test statistics were computed as $F_{2,99} = 4.70$ and $F_{2,99} = 4.34$, respectively (both are significant at the .05 level), rejecting the null hypothesis that the 16 electoral coefficients are equal. For the unemployment model, the test statistic was calculated as $F_{2,83} = 1.80$ and was not significant, failing to reject the null hypothesis at the .05 level.

10. In the working paper version of this article, I examined whether, on average, the public underestimates inflation that might rise after an election due to preelection policy stimulation. Using the SRC question about the 12-month-ahead expected inflation rate, inflation expectation errors for the average public were calculated as the deviations of the mean expected rates from the actual inflation rates. Then, a cycle in the errors was estimated via the PDL technique. The results indicated that the distributed lag coefficients are consistently positive and are significant during the first eight quarters of a presidential term. This implies that in a postelection period, actual inflation rises faster than the electorate expects but subsides and converges gradually to public expectations as an election approaches.

11. Nordhaus (1989) argues that electoral and partisan manipulations may well coexist in macroeconomic conditions.

12. The incumbent win probabilities were calculated using the procedure developed by Chappell and Keech (1988). I first regressed the actual shares of votes gained by the incumbent party on presidential approval shares in election quarters. From this regression I constructed a predicted vote share. I next calculated the forecast error for the prediction. Given the prediction and its forecast error, I calculated the incumbent's win probability, which was assumed to equal the probability that the incumbent vote share would exceed one-half. I proceeded in similar fashion to compute one-quarter-ahead win probabilities, using approval shares in various quarters before election quarters.

13. See Barro and Gordon (1983) for a theoretical treatment of the reputational game between the government and the public.

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COURTING CONSTITUENTS? AN ANALYSIS OF THE SENATE CONFIRMATION VOTE ON JUSTICE CLARENCE THOMAS

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The increasing public attention paid to Supreme Court nominations has elevated the salience of Senate confirmation battles, raising interesting questions about the impact of constituency preferences on senators' voting behavior. In this article, we explore this relationship using a logistical regression model to examine the impacts of African–American constituency size and the proximity of reelection on the roll call behavior of senators on the Clarence Thomas confirmation vote. Our analyses indicate that these factors were both statistically and substantively significant in the Thomas case. We conclude by discussing the theoretical and practical implications of such findings.

The factors that contribute to Senate confirmation votes on Supreme Court nominees have long fascinated scholars of both institutions. While partisan, ideological, and institutional variables are those most often cited, recent events suggest that some senators under certain circumstances are influenced by the preferences of their constituents. In this article, we take advantage of events surrounding the 1991 nomination of Judge Clarence Thomas to test this hypothesis. Our findings support the conclusion that constituency concerns played a significant part in Thomas's eventual confirmation and imply that further scholarly investigation of this constituency connection is warranted.

PREVIOUS RESEARCH

As a major player in the political process, the U.S. Supreme Court renders decisions that often have considerable impact on major areas of American life. While its decisions are periodically reversed, sometimes limited, and occasionally ignored by the other branches, the court wields significant power, particularly for an institution comprised of so few individuals.

Because the court's composition is a critical determinant of the policies it produces and because justices enjoy a life tenure on the bench, presidential nominations and Senate confirmations are important decisions whose repercussions echo for decades. Moreover, the appointment of Supreme Court justices takes on added importance in an era of divided party government, when representatives of the two parties struggle for control of the judicial branch. In such circumstances, the composition of the court is not only substantively important in terms of policy outputs, it is also a matter of considerable political interest.

Over the years, scholars have paid considerable attention to both the criteria upon which presidents base their nominations (e.g., Abraham 1985; Ackerman 1988; Daniels 1978; Fish 1988–89; Hulbary and Walker 1980; Perry 1991; Schmidhauser 1979; Scigliano 1971) and the "advise and consent" powers of the Senate (Grossman and Wasby 1972; Lively 1986; Nagel 1990; Rader 1988–89). Much related literature has addressed this branch interaction, seeking to illuminate the conditions under which presidents are likely to win Senate confirmation of their judicial nominees. Some examinations have focused on the overall political environment, especially factors related to the partisan balance of power. Halper (1972), for example, concluded that the Senate's opposition to a nominee is usually part of a larger pattern of opposition to the president, making the Senate more likely to reject a nomination put forth by an electorally weak or unpopular president. Others have expanded on this point, arguing that presidents who are late in their terms of office or who lack strong party backing in the Senate have more difficulty winning confirmation of their nominees (Palmer 1983; Scigliano 1971, 97–99; Segal 1987; Segal and Spaeth 1986). It has even been suggested that during periods when the court itself is unpopular, the Senate is more likely to reject nominees, particularly if such unpopularity coincides with presidential weakness (Halper 1972, 111–12; Schmidhauser and Berg 1972, 128).

In addition to examining the Senate's decisions on judicial confirmations, scholars have also scrutinized the votes of individual senators. Much of this research has substantiated the link between senators' ideologies and their voting behavior, especially on controversial nominations (Rohde and Spaeth 1976, 105–6; Songer 1979; and Sulfridge 1980). While partisanship and region have also been shown to have an impact on some confirmation voting, most recent work has concluded that ideology consistently has a larger effect on senators' voting behavior on controversial nominations (see Felice and Weisberg 1988–89).

Building on such work, Segal, Cover, and Cameron (1988–89) have suggested that senators' ideologies

interact with environmental factors to affect nomination outcomes. Specifically, they concluded that ideological distance between a senator and a nominee plays an important role in the senator's voting decision only when the nominee's qualifications have been questioned or when the political environment works against the president (i.e., when the opposition party controls the Senate or when he is in the fourth year of a term). They have also suggested that the behavior of electorally-minded senators is sensible and predictable: "When a strong president nominates a highly qualified, ideologically moderate candidate, the nominee passes the Senate in a lopsided, consensual vote"; on the other hand, when a weak president nominates a "less well qualified, ideologically extreme candidate," a closer, more conflictual Senate vote is likely (Cameron, Cover, and Segal 1990, 532; see also Massaro 1990).

Recently, some scholars have begun to suggest that there is also a "constituency perspective" to Senate voting on Court nominees (Felice and Weisberg 1988–89). This point has been articulated most forcefully by Caldeira, whose review of the literature in this area concluded that "more attention ought to be given to the influence of constituencies and mobilization by organized interests" (1988–89, 532).[1] Segal, Cameron, and Cover have recently addressed one aspect of the constituency connection; examining the relationship between constituents' ideological characteristics and the ideologies of Supreme Court nominees, they conclude that "confirmation voting is decisively affected by the ideological distance between senators' constituents and [Supreme Court] nominees" (1992, 109).

Such findings, of course, comport well with much general research on the Congress, where the received wisdom has long held that constituency preferences influence many aspects of congressional behavior (for authoritative statements see Fenno 1978; Fiorina 1977; and Mayhew 1974). Although some have questioned the impact of constituency on issues where members have strong ideological predispositions (Bernstein and Anthony 1974; McCormick and Black 1983; Moyer 1973), the weight of legislative research seems to agree that strong constituency preferences on salient issues will significantly affect congressional action either at the roll call stage or at some earlier, agendasetting stage (Arnold 1990; Overby 1991). In the area of judicial confirmations, attention to such a constituency dimension has been limited, Caldeira argues, due to difficulties in operationalizing measures of constituency effect and to the fact that court nominations have only recently been the subject of widespread public interest (1988–89, 537).

For several reasons, the 1991 confirmation battle over Clarence Thomas provides an excellent occasion to extend the insights of these researchers and explore in some depth the effects of constituency preferences on Senate confirmation votes. In addition to being a contested, nonconsensual nomination, the struggle over confirmation was of particular interest to an easily identifiable constituency, namely, African Americans. The fact that the size of this constituency varies considerably among the states (from .22% of the population in Vermont to 35.2% in Mississippi) permits testing its effect in a multivariate regression model. What is more, the fact that most organized African–American interest groups opposed the nomination (Biskupic 1991, 2692) while polls showed clear majorities of African–American respondents supportive of Thomas (Moore 1991) permits us to differentiate between the effects of organized interests and unorganized interests (constituencies).

Before turning our attention to the test of the constituency hypothesis, however, it is worth briefly reviewing the details of the Thomas nomination and confirmation.

THE THOMAS CONFIRMATION

On 1 July 1991, President George Bush nominated Clarence Thomas to fill the Supreme Court seat left vacant by the retirement of Associate Justice Thurgood Marshall. At the press conference announcing the nomination, Bush characterized Thomas as the "best person for this position" and denied that the nominee's race or his desire to fill a racial quota on the court had played any part in his choice ("Bush Announces" 1991).

A graduate of Yale Law School and a former chair of the Equal Employment Opportunity Commission (EEOC), Thomas was a judge on the U.S. Court of Appeals for the District of Columbia Circuit when Bush nominated him for the high court. Given his conservative political ideology and his rather turbulent tenure at the EEOC (where he had been criticized for allowing hundreds of age discrimination complaints to lapse without action), it was not surprising that there was significant opposition to his nomination from the beginning. In the Senate, some Republicans and many Democrats indicated they were unlikely to support a nominee who evaded questions or remained silent on the abortion issue (Biskupic 1991b, 1826). Outside the Senate, groups advocating abortion rights, women's rights, civil rights, and senior citizens rights urged the rejection of the Thomas nomination.

Thomas, however, was not without considerable advantages and support. His background, frequently characterized as a self-motivated ascent from childhood poverty in rural Georgia to the corridors of political power, evoked considerable national admiration. Likewise, his genial and composed personality mitigated the fears of some that he was ideological, extreme, and insensitive—another Robert Bork. Moreover, he enjoyed the strong support of Senator John Danforth (R-MO), a powerful figure in the chamber, for whom Thomas had once worked. Heading into the Judiciary Committee's hearings, Senator Dale Bumpers (D-AR) seemed to capture the Senate's emerging consensus when he noted, "Regardless of how I'm going to vote on [Thomas], I feel comfortable

he's going to be confirmed" (quoted in Biskupic 1991c, 2169).

Although he clearly did not please many liberal members of the Judiciary Committee, five days of hearings provided little reason to question the wisdom of Bumpers' assessment. The majority of questions concerned Thomas's views on abortion (he acknowledged a right to privacy but refused to commit himself on whether or not it extended to abortion), natural law (he indicated that his interest was political and did not affect his judicial philosophy), and civil rights (he testified that he preferred individual incentives to group remedies) (Biskupic 1991a, 2621–23).

On 27 September the committee split evenly (seven to seven) on its vote regarding the Thomas nomination. With one exception (Democrat Dennis DeConcini of Arizona), the vote cut along party lines. The Democrats who voted against him cited Thomas's reluctance to discuss *Roe* v. *Wade* and his equivocations on natural law as critical to their decisions. Although the split vote meant that Thomas's name would be forwarded to the full Senate without a recommendation of confirmation, most observers concluded that Thomas had gained momentum during the hearings and that the full Senate was likely to vote in favor of his appointment in early October.

Before the full Senate could act, however, the confirmation process took a very unusual turn. Reporters learned the identity of a woman who had accused Thomas of sexual misconduct during his service at the EEOC; the leaking of the story persuaded her to come forward and convinced the Senate to delay its vote. On 11 October, in front of the Judiciary Committee and before a nationwide television audience, Anita Hill, a professor of law at the University of Oklahoma and a former aide to Thomas at both the EEOC and the Department of Education, accused the nominee of sexual harassment. She alleged that Thomas had repeatedly made unwanted sexual advances to her and regularly included graphic sexual references in their workplace conversations. In his rebuttal, Thomas angrily denied these allegations and accused the committee of engaging in "a high-tech lynching for an uppity black who in any way deigns to think for himself" (quoted in Biskupic 1991d, 2948).

In the end, Hill's testimony failed to derail the Thomas nomination. After several days of extra hearings notable for their tension and a day full of angry floor speeches, on 15 October, by a 52 to 48 vote, the Senate confirmed Thomas to the Supreme Court. In the closest confirmation vote in over a hundred years, only 2 Republicans voted against Thomas, while 11 Democrats, mostly Southerners, voted in favor of his appointment.

MODELS AND ANALYSIS

With that background, we are now ready to examine the struggle over the Thomas nomination from the "constituency perspective," testing to determine whether the preferences of African–American constituencies in the states affected the confirmation voting behavior of senators.

Since the dependent variable we are interested in—the confirmation vote on Clarence Thomas—is dichotomous, we employ a logistical regression model to test the influence of constituency pressure. To control for the effects of senators' partisan and ideological predispositions, our independent variables include a dichotomous variable for PARTY (coded 1 for Democrats, 0 for Republicans) and each senator's Americans for Democratic Action (ADA) score from 1990.[2]

To test for constituency effects, we consider three variables. The first (RACE) is demographic—percentage of the state population that is African–American based on 1980 census data. Second, since previous research has shown that senators tend to pay greater attention to constituency matters as their reelection dates approach (Fenno 1982; Thomas 1985) and it is reasonable to assume that senators from states with large African–American populations might be especially sensitive to constituency opinion on the Thomas nomination, we include both an electoral proximity variable (REELECT, coded 1 for senators facing reelection in 1992, 0 otherwise) and an interactive variable (RACE AND REELECT) calculated by multiplying the state racial percentage variable by the REELECT variable. Together, these variables will allow us to test several components of possible constituency effects.[3]

Since we know from the literature that party affiliation and ideology should both have a large impact upon senators' voting decisions, we first model those two independent variables alone:

$$\text{Log (VOTE/1 - VOTE)} = a + b_1(\text{ADA}) + b_2(\text{PARTY}). \quad (1)$$

We regress the log odds of each senator's vote on ADA score and party affiliation, allowing for an intercept (a). The results of this test (reported in Table 1, col. 1) are predictable. The coefficients for ideology and partisanship are in the predicted direction (negative, indicating that Democrats and those with liberal ideologies tended to vote against confirmation); and when compared to their standard errors both are statistically significant at the .01 level.[4] In terms of model fit, these two variables permit the correct prediction of 88% (85/97) of the votes and reduce the error in predictions by 74.5% over a "naive" model that uses the modal vote as its prediction for all votes.[5]

To test for the direct effect of state racial composition on the confirmation vote, we add the racial demographic variable:

$$\text{Log(VOTE/1 - VOTE)} = a + b_1(\text{ADA}) + b_2(\text{PARTY}) + b_3(\text{RACE}). \quad (2)$$

While the results (Table 1, col. 2) are not dramatic, they are significant. The coefficient for race is in the

Table 1.

Logit Equation Predicting Senate Confirmation of Clarence Thomas

VARIABLE	EQUATION 1	EQUATION 2	EQUATION 3
ADA[a]	−.101**	−.091**	−.111**
	(.026)	(.027)	(.034)
PARTY[b]	−1.269**	−1.841**	−1.930**
	(.516)	(.621)	(.707)
RACE[c]	—	.103*	.022
		(.057)	(.067)
REELECT[d]	—	—	−2.195*
			(1.160)
RACE AND REELECT[e]	—	—	.277*
			(.139)
CONSTANT	6.233**	4.861**	4.872**
	(1.637)	(1.748)	(1.843)
% predicted correctly	87.6	89.6	93.8
Reduction of Error (%)	74.5	78.7	87.2

Notes: Figures in parentheses are standard errors; N = 97.
[a]Senator's Americans for Democratic Action voting score for 1990.
[b]Senator's party (1 = Democrat, 0 = Republican).
[c]Percentage of African Americans in state's total population (based on 1980 census figures).
[d]Senator's reelection status (1 = up for reelection in 1992, 0 = otherwise).
[e]Interactive variable calculated by multiplying RACE times REELECT.
*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.

Table 2.

Ordinary Least Squares Equations Predicting Senate Confirmation of Clarence Thomas

VARIABLE	EQUATION 1	EQUATION 2	EQUATION 3
ADA[a]	−.009**	−.008**	−.008**
	(.001)	(.001)	(.001)
PARTY[b]	−.288**	−.374**	−.399**
	(.093)	(.099)	(.095)
RACE[c]	—	.007*	.000
		(.003)	(.003)
REELECT[d]	—	—	−.222**
			(.082)
RACE AND REELECT[e]	—	—	.022**
			(.006)
CONSTANT	1.128**	1.053**	1.138**
	(.051)	(.061)	(.066)
Adj. R^2	.68	.70	.73

Notes: The first figures reported are unstandardized regression coefficients (*B*s); the figures in parentheses are their standard errors. N = 97.
[a]Senator's Americans for Democratic Action voting score for 1990.
[b]Senator's party (1 = Democrat, 0 = Republican)
[c]Percentage of African Americans in state's total population (based on 1980 census figures)
[d]Senator's reelection status (1 = up for reelection in 1992, 0 = otherwise)
[e]Interactive variable calculated by multiplying RACE times REELECT
*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.

predicted direction (positive, indicating that senators from states with large African–American populations tended to vote in favor of confirmation); and when compared to its standard error, it is statistically significant at the .05 level. More importantly, there is improvement in model fit, as the percentage of votes predicted correctly rises to 90% (87/97) and the error declines.

Finally, we construct our most sophisticated model of constituency influence in equation three by adding the reelection term and the interaction variable between race and electoral proximity, in effect testing whether or not state racial composition is most important to senators facing an impending reelection campaign:

$$\text{Log (VOTE/1 − VOTE)} = a + b_1(\text{ADA}) + b_2(\text{PARTY}) + b_3(\text{RACE}) + b_4(\text{REELECT}) + b_5(\text{RACE AND REELECT}). \quad (3)$$

As before, this more sophisticated model of constituency effect enhances the fit. Not only is the coefficient for the interactive term in the predicted direction (positive, indicating that senators facing a large African–American constituency in an impending campaign were more likely to vote in favor of confirmation), it is also significant at the .05 level. Interestingly, while the interactive coefficient is positive and significant, the coefficient for REELECT alone is negative and significant. This is perhaps an indication of the cross pressure many senators—especially those up for reelection—must have felt, caught uncomfortably between African–Americans, many of whom saw in Thomas a role model facing a "high-tech lynching", and others who were concerned about his suitability for the Court because of both his policy stances and Anita Hill's allegations of sexual harassment. Taken together, these additional variables improve the fit in comparison to the naive model, contributing to a better predictive capability of 94% (91/97) and a reduction of error value of over 87%.

Viewed together, the three equations summarized in Table 1 show that racial constituency characteristics consistently exercised a predictable, independent, and significant effect on senators' voting behavior and that there is a steady improvement in model fit as we treat the constituency effect in an increasingly sophisticated fashion. In comparison to equation 1, equation 3 cuts the number of incorrectly predicted votes in half (from twelve down to six) and reduces the error over the naive modal model an additional 12.7%.

Since the interpretation of logistic regression output can be difficult, we also modelled the equations using ordinary least squares. Although we technically violate this method's assumptions by using a constrained dependent variable, the closeness of the vote (52 to 48) mitigates the seriousness of this problem. The recalculations of each equation are presented in Table 2.

While we can have less confidence in the actual significance of the coefficients reported here, these findings generally reinforce those reported in Table 1. Race and the interactive race–reelection variables add to the explanatory power of the model in equations 2

Table 3.

Mean Probability of Vote to Confirm for Moderate Democrats* Under Certain Constituency Conditions

%AFRICAN-AMERICAN IN POPULATION	EQUATION 2 (RACE Alone)	EQUATION 3 (RACE AND REELECT) FACING REELECTION	EQUATION 3 (RACE AND REELECT) NOT FACING REELECTION
0	.19	.01	.07
10	.39	.15	.08
20	.64	.78	.11
30	.83	.99	.12

*For illustrative purposes, a "moderate Democrat" is defined here as one with an ADA score of 50.

and 3, respectively. Compared to equation 1, equation 3 increases the proportion of variance explained (adjusted R-squared) from .68 to .73.

The substantive impact of constituency concerns can better be understood by examining the behavior of specific senators. Compared to equation 1, equation 2 (which adds race) correctly predicts the *yes* votes of senators Ernest Hollings (D-SC) and Sam Nunn (D-GA), both moderately conservative Southern Democrats representing states with sizable African–American constituencies (30% and 29%, respectively). Likewise, compared to equation 2, equation 3 (which adds the reelection and the interactive race–reelection variables) correctly predicts the *no* votes of senators Wendell Ford (D-KY) and Robert Packwood (R-OR). Both of these senators are up for reelection in 1992, yet represent states with relatively small African–American constituencies (7% and 1.5%, respectively), suggesting that they responded to constituents mobilized against Thomas's confirmation. In addition, equation 3 correctly predicts the *yes* votes of senators Wyche Fowler (D-GA) and Mark Hatfield (R-OR). While Hatfield's case is difficult to understand, Fowler's is not. He is predicted correctly by this equation because, although he is a moderately liberal Southern Democrat (ADA = 72), he stands for reelection in 1992 *and* represents a constituency that is 29% African–American.[6] In short, adding constituency variables to the model not only aids in the prediction of votes, but does so in ways that are generally substantively interesting.

The substantive impact of constituency preferences is also highlighted by Table 3, which recalculates the logit regression coefficients generated in Table 1 for a hypothetical moderate Democrat under a variety of constituency circumstances and generates the probabilities of a *yes* vote under such conditions.[7] In the second column, the effect of race is considered alone; and the results are marked. A moderate Democrat from a state with an African–American population approaching zero had only a one-in-five probability of voting for confirmation. As the percentage of African Americans in the population rises to 10%, 20%, and 30%, the senator's likelihood of voting for Thomas increases to 39%, 64%, and 83%, respectively.

The third and fourth columns of Table 3 compare the hypothetical moderate Democrat's probability of voting to confirm, given the significant interactive effect between race and reelectoral proximity. For the senator facing reelection, the effect of race is striking. Such a senator's likelihood of voting for Thomas increases from 1%, to 15%, to 78%, as the percentage of African–Americans in the state constituency rises from 0%, to 10%, to 20%. At 30% African–American population, a moderate Democrat facing reelection is 99% certain to vote for Thomas.

CONCLUSIONS

While the centrality of constituency concerns has long been acknowledged in much literature on the Congress, only recently have scholars suggested that such concerns might affect one of the most important arenas of congressional decision making—Supreme Court confirmations. Moreover, until recently, the historical coincidence of reasonably noncontroversial votes and constituency connections that were difficult to operationalize conspired to make the testing of such a hypothesis nearly impossible. The nomination of Clarence Thomas, however, changed that. With the closely contested, highly controversial confirmation vote and the easily identifiable constituency groups interested, this nomination struggle provides an excellent opportunity to investigate the impact of constituencies on senators' voting behavior.

Our findings in this study extend previous research along an important new dimension that has both theoretical and practical consequences. By demonstrating how, and under what circumstances, constituency characteristics influenced the voting behavior of senators, we have substantiated Caldeira's claim and established that constituency considerations can significantly affect confirmation outcomes, at least in highly salient cases. This suggests that our general models of the process need to be amended to account for this factor. In the absence of such emendations, current models limited to institutional, ideological, and partisan variables will systematically fail to predict the behavior of certain senators—those who for electoral and demographic reasons have cause to be

most sensitive to constituency opinion. Furthermore, we extend the analysis of Segal, Cameron, and Cover (1992) by demonstrating that constituency preferences can influence confirmation voting behavior without working through the intervening factor of ideological distance from the nominee.

These findings carry at least two practical political implications. The first concerns the "representative" nature of the court. In an era of divided party government, Republican presidents and their judicial nominees are likely to face greater hostility and closer scrutiny from Democratic Senate majorities than ever before. Facing such a situation, strategic considerations counsel in favor of recommending nominees who have the support of a political constituency important to key, "swing" senators. Such was clearly the case with Thomas, a conservative African–American who had considerable appeal to several constituencies critical to the electoral fortunes of moderate, Southern Democrats. Whether other demographic groups could exercise the same pressure on the Senate is unclear, but certain groups (especially Hispanic Americans) at least have that potential. More generally, our findings raise the interesting possibility that in an age of televised confirmation hearings and glaring public scrutiny of nominees, "going public" to raise widespread popular interest and bring constituent pressure to bear on wavering senators might be one of the more productive strategies available to the White House in confirmation struggles with the Senate.

Second, in terms of the Thomas confirmation itself, knowledge of the constituency dynamic would have been—or, indeed, probably was—of considerable help to the White House in terms of predicting the outcome of the Senate vote. Operating with just the knowledge of individual senators' ADA scores and party, the White House could have predicted that Thomas would have been confirmed but only by the slimmest of margins—49 to 48, perhaps a close enough call to pull the matter from consideration, at least cause enough for extraordinary White House lobbying pressure or Republican parliamentary delays. With knowledge of the constituency effect, however, the Bush administration could rest easier, more confident of victory. Our full model of the confirmation vote, including ideology, party, race, electoral proximity, and the race–proximity interaction, predicts a 51-to-46 final vote, which is very close to the actual vote of 52 to 48.

In the final analysis, it is unlikely that constituency concerns will ever replace ideological predispositions and partisan loyalties as the principal determinants of Senate confirmation votes. In an age of divided party government, however, when such votes promise to be less consensual, constituency concerns may be of considerable significance in terms of the results of close contests. Given the outcome of the Thomas nomination, it seems likely that both strategic presidents and rational senators will pay close attention to constituency angles in future nomination battles.

Notes

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1. Caldeira differentiates between constituencies and "organized interests" by noting that the former are "unorganized interests, such as blacks, farmers, women, etc." (1988–89, 532n.).

2. In the case of Senators Brown (R–CO), Craig (R–ID), and Smith (R–NH) we employ 1990 ADA scores from the House of Representatives. Senators Seymour (R–CA) and Wofford (D–PA), both recently appointed, and Wellstone (D–MN), recently elected, without prior congressional service, have no ADA scores, and hence are generally excluded from the analysis, rendering the total number of cases 97 votes. We do not include a measure of presidential support in the model because it correlates highly with ADA scores (at approximately .8) and therefore produces multicollinearity problems in the model. In addition, the most readily available measures of presidential support are widely criticized in the literature (see Edwards 1989, chap. 2, for a summary).

3. We do not include a regional variable in our model, since such variables, especially in regard to southern senators, are often used merely as surrogates for ideology or constituency composition, factors we measure more directly with ADA scores and demographic characteristics.

4. As is usual in such models, determining statistical significance can be difficult and is open to some debate. We follow one standard practice, which is to interpret slope coefficients roughly as t-scores and evaluate them, in this case using a one-tailed test. The utility of such measures of significance is further complicated by the fact that the vote here is a population, not a sample, rendering the substantive interpretation more interesting than the statistical one.

5. In calculating the reduction-of-error (ROE) figure, which serves as a test of goodness of fit, we follow Lindsay 1991 and Hagle 1989:

$$\text{ROE}(\%) = 100 \times (\text{predicted } \% - \text{modal } \%)/(100 - \text{modal } \%).$$

6. In addition, not only is Clarence Thomas a native of Georgia (and, therefore, in a sense, a constituent), but Fowler depends heavily on the African–American community for reelection support. In his 1986 election, Fowler carried 75% of black votes, while winning only 39% of white votes (Petrocik 1987).

7. Moderate Democrats are, of course, the most interesting category in this regard. As "gettable" swing votes whose ideological predispositions had not already made their support virtually impossible, they seemed to be the focus of administration lobbying efforts in the days and weeks before the confirmation vote (Moore 1991).

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CONTROVERSY

GOING BEYOND THE STATE?

Timothy Mitchell's article "The Limits of the State" in the March 1991 issue of this Review *stimulated an unusual variety of interested comments. John Bendix, Bartholomew Sparrow, and Bertell Ollman offer critiques and suggestions from quite different points of view. In response, Mitchell clarifies further the distinctiveness of his own approach and its implications.*

COMMENT

The conceptualization of state and society remains troubling. We are dealing not with mutually exclusive categories but with a social and political relationship whose contours and definitions change over time and space. There is an unclear separation between the abstractions and the actual institutions said to be part of the state or society. We hold exaggerated, even romanticized, images of the total domination of state over society (absolutism, fascism) or the total domination of society over state (Marxism, anarchism). When we speak of the limits of the state we imply some point beyond which the state cannot go. Timothy Mitchell's "Limits of the State: Beyond Statist Approaches and Their Critics" (1991) has renewed this central debate for political science; and the neglect of the state that he attributes to the rise of behaviorism and systems approaches in the discipline echoes other critiques (Almond 1990; Caporaso 1989).

Mitchell argues that if we understood the state as "the powerful, metaphysical effect of practices" (p. 94) arising from internally erected distinctions in both state and society, rather than as an actual structure, we would come closer to the concept of the state. While this clever definitional solution owes something to Foucault, Mitchell's treatment of the "essentially idealist" approaches to the state taken by Nordlinger, Krasner, and Skocpol is disembodied, as though they, too, were not responding to previous concepts of the state. The historical development of state structures needs discussion if the "actual state structure" is not to become a straw man. Most importantly, Mitchell's contribution to the debate neglects how the state and society influence one another (Almond 1990).

The traffic across the border between state and society is more interesting than where the ultimately somewhat arbitrary demarcation line is drawn. Already in 1931, one observer claimed to have found 145 different definitions of the state (see Caporaso 1989, 28). States exist by virtue of, and together with, societies. The boundary is defined and contested; but *who* is delineating, negotiating, and crossing that boundary for what purpose is more important to investigate. The interactive elements complicate this pair of terms and make it interesting. To focus on the appearance of the boundaries imposes a definitional separation about which there is great disagreement. Recent analyses of interpenetration and mixed modes (corporatism, intermediate associations with both public and private functions, the interaction of bureaucracies with private interests) seems a more fruitful means to find the "limits of the state."

I suggest an expansion of Mitchell's argument by reviewing the state as concept, as law, and in its historical development. I suggest that we look at the state less in terms of its limits than in terms how it has been conceptualized and put into practice.

THE STATE AS CONCEPT

Max Weber's definition of the state as possessing the monopoly of the legitimate use of force is well known, as is his notion that the system of order of the state claimed binding authority over both citizens and territorial jurisdiction. Less well known is that Weber's state was based on the idea of a powerful corporate group whose membership either closed or limited outside access and whose functioning accorded with norms that corporate groups regarded as valid. While a person or persons were in authority to carry out the order that governed the corporate group, the state as an administrative and legal order was regulated and restricted by legislation (Weber 1947, 145, 156). Weber's definition was neither teleological nor idealistic; and he was aware that the combination of bureaucracy, legally based order, compulsory territorial jurisdiction and monopolization of the legitimate use of force rarely occurred (Bendix 1977).

Two major categories of objections to this definition of the state have been made. The first denies that the state is either unique (as legitimizer of associations, as claiming binding authority over its citizens, or as having a special life-long concern for its citizens) or omnipotent (Weldon 1960, 48–50). I am neither sanguine about the state nor convinced that the state is an association like other social groups. If one applies Hirshman's exit, voice, and loyalty typology to social groups, then state membership is of a rather different order than societal membership (Bendix, Bendix, and Furniss 1990; Skowronek 1982).

The second objection is that states act in class interests. Claus Offe's state sees the bureaucracy as the mediator of a class struggle in the capitalist accumulation process, Nicos Poulantzas' (and Antonio Gramsci's) state reflects the issues of the dominant class and becomes the "materialization and

condensation of class relations" (Carnoy 1984, 107, 130, 254; Chilcote 1981, 366). Yet there are "serious (empirical) questions about the plausibility of a theory of state autonomy for any advanced capitalist country" (Domhoff 1990, 28).

But pluralist, elite, and Marxist views of the state may not be incompatible. Each theory equally permits its adherents to assert a *cipher* image, where the state is passive and ultimately controlled by civil society; a *guardian* image, where the state is an autonomous force that can balance out the social pressures being put on it; or a *partisan* image, where the state advances the interests of its constituent parts, whether organizational or individual, though under constraints and through bargains struck with other social groups (Dunleavy and O'Leary 1987, 327–31).

One cannot understand states without making reference to society, and our confusion about their interaction must be laid at the feet of Hegel. Hegel's universalist state is an impossibility of enlightened absolutist guardianship bound to personal independence anchored in an internalized Kantian sense of duty. The state is the evolutionary high point of the development of the universal; civil society is private, filled with individual interests and activities, and by its very nature cannot achieve the necessary universal freedom (Chilcote 1981, 189). Society incorporates certain universal qualities but has to be guarded by the state; for the two are mutually dependent, interfused, and interacting, though society is clearly subordinate to the state (Barker 1961, 48–50). Hegel's argument permits both state and civil society to be *relatively* autonomous from one another while still exercising reciprocal influence.

Marx's reformulation is intended to undermine what he saw as a "separation of state from civil society" in Hegel (Marx 1977, 33). Marx believes that class domination would lead to a society subservient to the state, which is unacceptable (Chilcote 1981, 421; Giddens 1985, 20–21). Marx's *Critique of the Gotha Program* states that "freedom consists in converting the state from an organ superimposed upon society into one completely subordinate to it" (1977, 564). Hegel had not acknowledged that "developed civil society" remained hampered by the fetters of the privilege system, Marx felt; and civil society was "political society because the organic principle of civil society was the principle of the state" (quoted in Chilcote 1981, 190).

Interpreting the state as subservient to society—in part a legacy of Marx and in part a legacy of legitimizing popular sovereignty—has created an interesting problem. To many observers today the state has no other role to play *but* to serve the citizenry. "If the state has a proper realm," Harry Eckstein wrote, "then it surely is the redistributive" (1979, 16–17). We are left with the illusion that the "state is hard to distinguish from the citizens in whose name . . . its authority is being exercised" (Watkins 1968, 152–53).

The state should be responsive to society, but total societal dominance would make it impossible to meet citizens' needs. No one would guarantee distributive justice, protect the health of citizens, secure national defense, or even provide an alternative to a massive private market (Hague and Harrop 1987, 20–22). The inadequacies of a weak civil society, by contrast, have been very evident in Eastern Europe in the last few years.

Weber's conceptualization is firmly anchored in his ideas of society. The state stands at some remove from, but us ultimately under the control of, social forces; and it cannot avoid interacting with society. Weber reformulates Hegel's state-dominant, universalist interpretation, while maintaining a good portion of the interactive character of civil society with the state. Such a view also emphasizes the importance of popular sovereignty—a notion not easily reconciled with Hegel's emphasis on the guardianship role of the state. Marxist and neo-Marxist visions, on the other hand, have tried hard to eliminate the dynamic tension between state and society. Marx's attempt to undermine an artificial separation of state from civil society is commendable (if a misreading of Hegel); but his solution of favoring society over state to right past wrongs is not. The answer cannot lie in replacing state dominance with societal dominance. The neo-Marxist reinterpretation of the state distorts even Marx's reinterpretation of Hegel. The interaction between state and society is not made clearer by treating the state as a cipher and the power of society as emerging from a single class. Dunleavy and O'Leary's (1987) partisan image at least allows the possibility that both state and society may have their interests, bargains, and constraints.

THE STATE AS LAW

A different answer has been provided by those who see the state as law or popular sovereignty. Modern sovereignty implies both an older supreme power (the monarch's) and a newer legitimation through institutions (legislature, cabinet, executive, citizenry) tied to a constitutionalism that marks state institutions off from society. Social groups are inferior to the sovereign state, yet sovereignty based on popular legitimation makes the state ultimately subservient to society.

Modern sovereignty implies legally sanctioned institutions. The state today is defined as a legal association organized under, and by virtue of, a constitution operating under a rule of law that guarantees the rights, and establishes the duties, of citizens. The state is the "sovereign safeguard" that secures an "external framework for the inward movement of a good life" (Barker 1945; Barker 1961, 47; Benn and Peters 1968, 296).

Government in contemporary democracies is formally accomplished in accordance with written constitutions and administrative procedures that rest upon a system of laws. Yet legal systems differ. In

Continental European Roman law a set of transmitted written codes depended for its authority "on the powers inherent in the person or persons promulgating it" (Hall 1989, 10). Code law implies a subservient judiciary (to emperors, legislatures, legal experts) and thus a state above society, guiding and regulating it and distinct from it. Anglo-American common law, with its independent judiciary and constitution legitimated from below, places society above the state, making law and government the product of the customs and habits of its people. Such legal differences affect how state or government and its relation to society are analyzed.

THE STATE AS THE PRODUCT OF UNIQUE HISTORICAL CIRCUMSTANCES

The Europe of 1500 had 500 political units, the Europe of 1900 only 25 (Tilly 1975, 15). The fragmentation of the medieval era—with its array of economic institutions (guilds, merchant leagues), ecclesiastical power (churches, monastic orders), secular centers (cities, universities) and hereditary rule over bits of territory (aristocrats), all struggling for dominance and autonomy (Eckstein 1979, 6)—allowed for a consolidation by particular hereditary leaders through force, fraud, and alliance. Initially, there was little distinction between the monarch's state, family, property, and society: the French king governed with a family council; Frederick William I of Prussia in 1740 earned as much from his royal domains as he gathered in taxes; and in England the law of property encompassed the rules about public office (Barker 1945, 4–6). Yet increasing central control diluted the monarch's power—Frederick the Great claimed he was only "the first servant of the state" (Dyson 1980, 28)—because a larger territory needed to be defended. Defense meant armies and the development of an efficient, nationwide system of administration to raise the money for the army through taxes. "The business of administration," Ernest Barker notes, "becomes the business of providing and equipping an army" (1945, 39).

The alternatives to paid mercenaries—universal conscription and reliance on local nobility for troops—involved political challenges to the monarch. The first implied functional equality through equal obligation, at odds with the social hierarchy of absolutism; the second led to demands by local notables for favors or representation in return for providing men or money to the king. Yet the king's need nevertheless ushered in the functional equality of treatment through the formally uniform application of obligations across territory and subjects by a national administration.

Society had to become an entity free of the king and the estates that supported him, to move away from its connotation as "high society" and toward a politicized sense of groups organized for collective action. The French Revolution ushered in the new category of citizens and, with it, a politicized society that legitimated self-selected institutions. To the centralized, administrative state was added the new nation and a more inclusive society. Yet the newly legitimated state used the model of central penetration and standardization pioneered by absolutism (Tilly 1975, 66). The rise of market economies had undermined the privileges of estate, and the extension of suffrage made political participation increasingly inclusive and oriented toward collective action. Society came to mean the articulation of political and economic interests from below, even while the economic imperatives of nation-states necessitated continued involvement in the economy from above, particularly in the state regulation of property, status, and money (Fried 1968). In this history one begins to see a sense of the state as a commonwealth, rather than a paternalistic fiefdom, though the vestiges of a sense of the state imbued with idealistic "ennobling sense of purpose" remain as one of the more benign legacies of absolutism (Dyson 1980, 3, 6).

Absolutist rule bore within it the seeds of its own destruction, yet institutional structures established under the aegis of monarchical rule were not entirely abandoned. The role of the state now to guarantee the well-being of the citizens, combined with changes within the market, as well as the rise of nation-states, gave the new institutions of government a role to play in the regulation of property and rights, as well as continuing the older roles of defense of territory and collection of money. The participatory and arbiter sense of state arose with the civil society of the French Revolution, with its functional representations of interest and its competition among organized societal groups (Benn and Peters 1968, 348).

CONCLUSION

I agree with Mitchell that analyzing the state as a free-standing entity is not helpful, but I do not agree that the solution is to treat the distinction as a matter of appearance or the reproduction of a "line of difference." It too readily turns what should be a dynamic interaction into an issue of postmodern semantics.

Understanding the state-and-society boundary better is akin to asserting that the crucial problem in Eastonian systems theory is understanding where "the political system" ends and "the environment" begins. If, as Mitchell implies, the conceptualization of politics in terms of a "system" is flawed, what have we gained by better understanding the external and internal boundaries of that system? American political science has found it easier to study the outputs of institutions than to dwell on the public purposes that motivate state actions and easier to examine welfare policy than the concept of what the general welfare of the citizenry might be. Other intellectual and legal traditions indicate that this need not be the only way

to understand government, though it may be a distinctive feature of British pragmatists and American empiricists to analyze parts, rather than wholes (Dyson 1980, 17). However, organic or cybernetic models, with their use of the "political system" as a substitute for the state (with its inputs, throughputs, and outputs) may not be the most appropriate means to study how history, ideas, and law have interacted in the modern state.

Our conceptual problems come from replacing a static but interdependent Hegelian conception of state dominance and civil society subordination with a relatively dynamic Marxist idea of the development of societal dominance over a state compromised by class forces. The Weberian focus on the binding authority exercised by an exclusive corporate group gets lost in the shuffle. Neo-Marxist instrumentalist views only reinforce the dominance of society *over* state, rather than the interaction of society *with* state. The fusion of state and society provided by deconstructionist orientations does not aid our understanding of the relationship but merely comments on the relationship.

Boundaries do have meaning, whether in the empirical world of keeping undesirables out and citizens in, in the psychological and cultural sense proposed by Fredrik Barth, or in the abstract imperial claim to render unto Caesar that which is Caesar's. But boundaries are crossed, contested, opposed, and acquiesced in by those whom they affect. Boundaries are porous and changeable; and it is in the analysis of migrations across such boundaries that the state and society negotiate their relationship.

But we cannot escape the contradictory tradition of the European state. The modern state emerged from a history of absolutist guidance from above and overarching purpose, combined with a stratification that demarcated who had political and economic power and who did not. Its evolution into a benign guarantor of individual rights and policer of contracts makes us forget that it remains the authoritative definer of the public interest.

Jacob Burckhardt argues that it is an arrogance for the state to "attempt to fulfill moral purposes directly, for only society can and may do that"; the state is the guardian of the law and its business is to "prevent the various conceptions of civil life from coming to blows [so that the] various egoisms but also the various metaphysics shall carry on no blood feuds" (1943, 118–19). Burckhardt's state is custodial, protective, and based on law, rather than social contracts—a union based on conscious reciprocity (Dyson 1980, 11). This humane, functional characterization emphasizes what can be best about a state—not its limits or its definition but how it may interact with what is best in the society it is responsible for.

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COMMENT

Timothy Mitchell (1991) attributes to its elusive boundary the difficulty in defining the "state." One group of scholars, including Gabriel Almond and David Easton, finessed the problem of the state–society boundary by abandoning the state as a concept. But their "political systems" approach could not wholly avoid the issue of what constituted the "edges" of the political system—so the boundary question remained, as salient as ever. Other scholars, such as Eric Nordlinger, Stephen Krasner, and Theda Skocpol, resurrected the state as a subject of study but perpetuated this boundary ambiguity by (unhelpfully) restricting the state to a subjective arena of autonomous decision making.

Using the relationship between the U.S. government and the Arabian American Oil Company (Aramco) as an example, Mitchell argues that the state does not mark a "real exterior." Rather, the distinction between the state and society (i.e., among the institutions maintaining societal order) is *itself* a part of a "larger political order"—an order in which Aramco's "nonstateness" is also a resource. Mitchell finds the state–society boundary to be a "structural effect" that identifies which institutions constitute society and which, the state. Central in the creation of this structural effect is the production of *disciplines*, exemplified by the formation of the French Army as it became separate from society. Mitchell concludes that the production and reproduction of the definition between the state and society is "the essence of modern politics" (p. 95).

Mitchell's provocative thesis prompts a two-part response. The first part concerns the primacy of the boundary problem in the study of the state; the second part addresses Mitchell's alternative to a direct study of the state, the study of disciplines.

PROBLEMS WITH THE STATE

Since "a definition of the state always depends on distinguishing it from society," Mitchell holds that the "difficulty in drawing the boundaries of the state" is the fundamental issue for the state, explaining its controversial acceptance in American political science (p. 77). But to implicate the boundary problem as the cause of the "ill-defined" modern state ignores other, important causes for the state's imprecise definition. The presence of these additional reasons casts doubt on the centrality of the state's boundary-drawing problem.

One reason for an imprecisely defined state is the shadow that the distinct conditions of the United States cast on American and international scholarship on the state, granting the international impact of American political science. Whether for reasons of universal (white male) suffrage preceding industrialization, the lack of a feudal heritage, the self-selection of its inhabitants (with the important exceptions of

African Americans and Native Americans), the establishment of a federated and fractioned system of national government, or other factors, the United States has been less amenable to the state label compared to other states (especially relative to a Bismarckian Germany or an absolutist France). After all, Philippe Schmitter's corporatism, applied frequently to studies of European or Latin American governments, for instance, is scarcely used in research on the American government (Schmitter 1974, 1986). But an application of *the state* both to the United States and to other nations is explicit in the work of J. P. Nettl, Krasner, and Skocpol and implicit in the work of Easton, Nordlinger, and others. The different development of the American and non-American states ("American exceptionalism") has played a key role in the troubled concept of the state.[1]

A second reason for an ill-defined state comes from deficiencies in its specification, a problem that is more the consequence of method and less the product of the boundary issue. American political science, with its emphasis on positive research, has been not particularly receptive to the Marxian states of Fred Block (1977), Ralph Miliband (1969), Claus Offe (1974), or Nicos Poulantzas (1973), to name a few.[2] But the work of Krasner, Nordlinger, Skocpol, Stephen Skowronek, and others—work that succeeded the Marxian scholarship and that has been instrumental in bringing about the resurgence of interest in the American state—has conceptual and methodological ambiguities of its own. Three problems, in particular, attend this recent scholarship.

One is simply *defining* the state, as noted by Mitchell. Krasner defines the state as "the President and those bureaus relatively isolated from societal pressures" (1978, 62), a definition that allows for such an insulated state to take a variety of forms over time.[3] Nordlinger, on his part, defines the state as all public officials, federal, state, and local.[4] But Nordlinger's argument on the state's potential autonomy depends on state officials having subjective and independent preferences. Meanwhile, Skocpol's analysis of state autonomy includes state–society relations in its explanation, as Mitchell recognizes (pp. 87–88). Not only is there little agreement by those studying the state on what comprises the state, but those studying the state present problematic definitions of their own. The state has to be pinned down.

Second, there is the problem of determining the status of the state as an *explanatory variable* (see my earlier work, Sparrow 1991). Mitchell writes of the state exclusively as a dependent variable, for example, while Nordlinger considers the state solely as an independent variable.[5] Skocpol uses the state at once as an independent variable, an intervening variable, and a dependent variable.[6] But the status of the state as a variable is of obvious importance in assessing the validity of a state-centered explanation and in evaluating the salience of the state-society boundary. The explanatory role played by the state has to be made clear.

Then, there is a third problem—uncertain *explanation of change* in the state. Change in Krasner's state comes about through an indeterminate convergence of political forces (president, Congress and congressional committees, federal bureaucracies, pressure groups, and even public opinion) on particular policies (1978, 61–66).[7] Skocpol, on her part, does not present a consistent theory of state building (Burawoy 1989).[8] Neither does Mitchell provide the reader with the mechanisms of state change save for a discussion of disciplines (which I take up later). In short, the mechanisms of state change—taking the state as a dependent variable here—are often inexact.

These problems of specifying the mechanisms of change in the state, revealing the status of the state as a variable, and offering precise and consistent definitions of the state raise serious questions for scholarship of the state. These questions are separate, for analytic purposes, from those that address the state–boundary issue directly. The problems in conceptualizing the state warrant separate consideration. Likewise, the problematic application of the state to the American polity and to other (particularly non-Western) polities is an issue that merits serious attention in its own right.

INDETERMINATE DISCIPLINES

The other part of my response to Mitchell concerns his implications for studying the state and the state–society boundary. Mitchell's conclusions are that (1) the state should not be seen as distinct from society, (2) the state-society boundary should be the point of focus in the study of modern political order, (3) the study of the state as the locus of decision making and the policy process cannot suffice, (4) the state should be addressed as the effect of detailed processes that divide the state from society, and (5) the separate structures of the state should be recognized for their abstracted and constructed nature. The crux of Mitchell's project centers on the last two points, however—that the processes dividing the state from society should be seen as part and parcel of the use of power and that perceptions of such processes are themselves resources for political control. It is Mitchell's ambition to reveal that state and society are constructions and that existing institutions contain hidden imbalances of power. Yet this deconstructionist project amounts to an indeterminate and ultimately unfalsifiable political science.

Consider Mitchell's language: "The appearance that state and society are separate things is part of the way a given financial and economic order *is maintained*" and "The *arrangements that produce* the apparent separateness of the state create the abstract effect of agency, with concrete consequences. Yet such agency will always be contigent upon *the production of difference*—upon those practices that create the apparent boundary between state and society; his ultimate object of study, "disciplinary power," operates "not from the outside but from within, not at the level of an entire society but at the level of detail, and not by

constraining individuals and their actions but *by producing them*" (pp. 90–91, 93; emphases mine). The study of power-wielding, boundary-making institutions is typified by the absence of agents acting on behalf of any dominant interests and the omission of mechanisms connecting individual and organizational behavior to systemic outcomes.

The reader is left with little notion of the particulars of such discipline production, however. Take Mitchell's example of the creation of the French Army: "Of course, this apparatus [the French army] has no independent existence. It is an effect produced by the organized partitioning of space, the regular distribution of bodies, exact timing, the coordination of movement, the combining of elements, and endless repetition" (pp. 93–94). Space *is partitioned,* bodies *are distributed,* timing *is made exact,* movements *are coordinated,* elements *are combined* and effects *are repeated,* all to the perpetuation of existing power relationships. This is nothing more than systemic functionalism: "State *practices* define and help constitute a national entity . . . by establishing a territorial boundary and exercising absolute control over movement across it" (p. 94; emphasis mine). Mitchell nowhere provides the reader with the subjects who produce difference or maintain order.

The use of the term *disciplines* follows from Michel Foucault, of course. But Foucault's (1987) *Discipline and Punish* reveals the same agentless explanation of the penal system: "And, if one can speak of justice, it is not only because the law itself or the way of applying it serves the interest of a class, it is also because the differential administration of illegalities through the mediation of penalty forms part of those mechanisms of domination" (p. 272). Law, or its application, serves class interests; and the differential enforcement of the law is part of the same class dominance. Foucault continues, "Deliquency, solidified by a penal system centred upon the prison, thus represents a diversion of illegality for the illicit circuits of profit and power of the dominant class" (p. 280). As Foucault summarizes the penal system, "A whole series of illegalities *was inscribed* in struggles in which those struggling knew that they were confronting both the law and the class that had imposed it" (p. 274; emphasis mine). The outcome (maintained class division) explains individual and organizational behaviors (delinquency and the particular characteristics of the penal system). Again, this is the logic of systemic functionalism.

Raymond Boudon observes that Foucault ends up positing that the ruling class has absolute power, can conceal this power, and can at once exploit the penal system to its own advantage while criticizing imprisonment. As Boudon puts it, "Prison is useful to the police, and the police are useful to the ruling class" (1987, 136–37). Mitchell appears to take this same position: "The appearance that the state and society are separate things is part of the way a given financial and economic order is maintained" (p. 90). The creation of disciplines allows the state to act, directly and indirectly, in the interest of existing power relationships. Only we are not told how to study the detailed processes that actually produce the state–society boundary.

Yet there exists a body scholarship that could serve as a point of departure for a detailed study of institutional behavior and social processes—the work on organizations and institutions coming out of the Carnegie school (e.g., Simon 1948, 1957; March and Simon 1958; Simon 1962; and Cyert and March 1963—and, more recently, March and Olson 1984 and Moe 1984).[9] "Neoinstitutionalists" share a focus on individuals precisely *not* for establishing an independent "set of intentions or preferences, just as persons are thought of as units of autonomous consciousness and desire distinct from their material or social world," as Mitchell has it (p. 88) but, rather, for establishing the cognitive bases of organizational and institutional action.[10] This new institutionalism also pays heavy attention to an organization's environment, acknowledges the loose coupling among organizational components, and emphasizes the reflexive aspect of individual behavior (Powell and DiMaggio 1991, 11–19). By examining the embedded and uncertain qualities of individual action, such an approach is able to provide the microfoundations of organizational and institutional behavior.

Not only is organizational and institutional behavior the product of individual action, but the reverse holds, as well: standard operating procedures, organizational routines, classifications, and framing may determine individual actions and, hence, aggregate outcomes. Once habits, roles and labels become established and taken for granted (even if themselves human creations), they may reciprocally affect individual actions. The preferences of corporate, associational, and governmental actors are not developed autonomously or in distinction to material or social context but are *learned.*

BEYOND THE STATE?

The state may be defined inexactly for reasons apart from its boundary problem. One reason is that the distinct qualities of American political institutions, less amenable to the state label for reasons indigenous to the United States, has hindered the specification of the state in American political and social science. Another reason for the troubled concept of the state has been the methodological imprecision of scholarship on the state, imprecision that has taken at least three forms: (1) those researching the state offer problematic definitions of it and thereby obscure its conceptualization; (2) scholarship on the state has often been inconsistent or ambiguous on the status of the state as a variable; and (3) an inattention to the mechanisms of state change has made the state difficult to study. It is by no means clear that the state–society boundary is the primary difficulty in defining the state or that the troubled boundary

between the state and society in fact defines the modern political order.

We might be cautious before going "beyond statist approaches." A focus on disciplines provides little assurance that research on the state will be other than nonfalsifiable or externally invalid. Neither is Mitchell's attention to microprocedures (the processes that create the institutions constituitive of the state and society) novel. And the recourse to functionalist explanation may be a step backward for political inquiry. When Mitchell says that the state "should be addressed as an effect of detailed processes of spatial organization, temporal arrangement, functional specification, and supervision and surveillance, which create the ap[p]earance of a world fundamentally divided into state and society" (p. 95), we may wonder (1) *to whom* the state and society appear so fundamentally divided, (2) what is *new* about turning scholarly attention to organizational configurations and microprocesses, and (3) *who or what* are effecting such divisions between the state and society through the creation and nurturing of these discipline processes.

In consideration of these remarks and in view of Mitchell's own telling arguments—arguments that debunk the attributed coherence of the state and that explain that the boundary of the state does not actually separate between the mechanisms facilitating social and political order—what may we conclude about the study of the state?

CONCLUSION

It may be hard to do better than recommend a return to the work of Max Weber. Weber defines the state as "an administrative and legal order" with "binding authority" over "the area of its jurisdiction." For Weber, the state "is thus a compulsory association with a territorial basis" (1978, 56). A Weberian definition of the state would identify the state with legal–coercive bureaucracy. But Weber also remarks upon the *zweckrational* behavior of modern business and public administration. The study of the codification, standardization, centralization, and specification of authority in governmental administration—an interest shared by Mitchell—is also included in Weber's analysis (1946, 196–244). Statist research would examine these instruments of administration and policymaking.

But to study the bureaucracy and the means of administration by themselves would still be to restrict the study of the state; for if we are to study the state as a political association claiming a monopoly of legitimate violence (Weber 1978, 54), we need to consider the institutions exercising a monopoly of authority, whether autonomously or through the joint operation of governmental and societal actors. Recall that Weber places primary emphasis on the explicit or implicit coercion of the state (pp. 903, 909). In the terms of Nettl's four ways of defining "stateness," it may be possible for the state to be an institutionalized collectivity (dimension 1), and international unit (dimension 2), and a sociocultural phenomenon (dimension 4) but not have sectoral autonomy (dimension 3). Nettl (1968) omits a consideration of coercion (or nonvoluntarism) in discussing the state, even though he cites Weber in multiple instances. Similarly, Mitchell mentions coercion once (when discussing power and the institutions creating and reproducing power, p. 82) but does not touch on the *monopoly* of power.

The monopoly of power is crucial for any definition of the state, however; for the presence of such a monopoly may implicate nominally private actors, such as professional associations, business corporations, labor unions, and even individuals, in the state. Consider the government–societal ties implicit in extensive transactions in the receipt of retirement benefits and government pensions, the sale of government securities, the payment of income taxes, and military spending. Indeed, Mitchell's reference to investigating "the relationship between state and private institutions in the financial sector, in schooling and scientific research, or in health care and medical practice" suggests precisely this focus (p. 90). This focus is explicit in the work of Laumann and Knoke (1987), who define the state as the networks of interaction between governmental actors (congressional committees, federal bureaus and agencies, and the White House office) and societal actors (trade associations, business corporations, labor unions, and other pressure groups). It is implicit in the work of Skocpol (1979, 1981, 1985) and Ikenberry (1988), who look at state–society relations as part of a statist analysis but do not include such relations as part of the state. The state–society boundary *is* problematic; but it may not be so much a "boundary" as a sector of coordinated, and possibly conflictual, government–society interaction that exercises degrees of jurisdictional monopoly.

A *fourth* response to the problem of the state may be added to Mitchell's three. Besides eliminating the state from study (Almond, Easton); confining it to an autonomous existence (Nordlinger, Krasner, Skocpol); or studying it as a structural effect (Mitchell), we could expand its definition to include the societal actors that—either in direct conjuction with, or tacit agreement with, governmental actors—are able to exercise effective monopolies of authority. Indeed, the literature on "policy networks" and "iron triangles" addresses this very overlay of the "public" and the "private." To study the state defined along this line demands a respecification of the state, however, rather than its relegation to the realm of a "metaphysical effect."

It may be true as Mitchell says, that Weber's definition of the state "does not tell us how the actual contours of this amorphous [state] are to be drawn" (p. 82); but it is also true that Weber never finished his analysis of the state (Bendix 1977). Studies of the state, just like those of the frontier, the law, or army building (subjects suggested by Mitchell) no doubt *can be* specified. Such a program for examining bu-

reaucracies, the means of policy administration, and the relations between the government and society calls for innovative research able to provide indicators of the multiple dimensions of national power—the composite, fractioned, and aggregate qualities of a state—and to test propositions on these constructions of national power. It is incumbent on students of national political authority to do so—to attempt to update Weber's work on the state.

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COMMENT

To his credit, Timothy Mitchell (1991) does indeed go "beyond" the boundaries that have been drawn in most debates among political scientists on this subject, but he leaves us with the question whether he has gone far enough. After effectively criticizing various writers' attempts to establish a clear division between state and society, Mitchell offers a version of their union that emphasizes people's numerous everyday activities that constitute the forms and hierarchies of order and control (our political institutions), while simultaneously making these institutions appear separate from, and independent of, the larger social context. In this way, the same practices that give rise to the state are held to be primarily responsible for the widespread misunderstanding of its nature—in particular, its limits.

But why do we want to understand what the state is in the first place? Primarily, I would have thought, because we want to understand better what the state does. This suggests that most people already have a rough idea what the state does: it makes laws, administers and enforces them, adjudicates disputes on the basis of such laws, and represents people who live under the rule of one state in their dealings with other states. Until greater clarity is attained regarding who and/or what does all this, however, it is difficult to distinguish the state from such kindred phenomena as the government and the nation or to understand why setting the boundaries of the state should be such a problem. Worse, it is impossible to explain the patterns observed in the state's conduct of its work and the effects resulting from there, namely, that some groups in the country consistently play a greater role in the higher reaches of the state and benefit to an inordinate degree from most of what the state does. Well, who are they, and why should this be so? And does it have anything to do with the nature of the state as such? These would seem to be reasonable, and even obvious, questions.

While all the writers that Mitchell criticizes are indeed guilty of setting up the state as an independent entity, they do at least provide us with answers to these questions if only to deny the accumulated evidence—though it is usually more than that, like attributing the observed bias to the decisions of politicians. Mitchell, on the other hand, offers no answers whatsoever. The Foucault-inspired micro practises of "organization and articulation" (p. 92) that constitute the state in his account are disembodied, depoliticized, declassed, and, despite a couple of vague references to history, ahistorical. Emptied of all social content, they can tell us nothing about why the state acts as it does.

Yet there is much in Mitchell's essay that suggests he really knows what the answers are. He says, for example, that the spillover of society into what is ordinarily taken to be the state, their internal relation, has to do with how a "social and political order is maintained" (p. 90). But we never learn who benefits most from maintaining the present social order or how, exactly, the state helps them. The social order itself is never named. Capitalism is nowhere mentioned in his article.

Further, Mitchell is aware that a major effect of the attempt to remove the state from society is that it reinforces the view that the state and/or officials acting on its behalf are free and independent agents, only occasionally influenced—and then only to a minor degree—by larger social forces. From this, it is but a short step to recognize (as I believe Mitchell must, although he nowhere makes it explicit) that there are few beliefs more important to the health of the status quo than the one that treats state institutions as neutral in the political and economic clashes between different groups, implying that each of them has a more or less equal chance of winning the support of these institutions. To believe that the state has special ties to one group, ensuring that its major interests always come on top, would cost the state a great deal of its legitimacy and make it far more difficult for it to do whatever it does on behalf of the favored group.

Finally, Mitchell's own examples suggest to us that he knows more than he says. Respecting the division between state and society, he says, "The essence of politics is not politics formed on one side of this division being applied to or shaped by the other, but the production and reproduction of this line of difference" (p. 95). Do the Aramco Oil Company and American banks, two of Mitchell's main examples (pp. 89–90), engage in politics to establish a clear separation between the state and society, or to promote their own money-making interests? The question should answer itself. If, in the process of promoting their interests, they make use of means that disguise their integration into the state—chiefly as a way of avoiding democratic accountability—this certainly deserves to be mentioned; but it should not require that we exchange one disguise for another. While Mitchell isolates a major ideological distortion that emerges from the particular means by which a favored group helps constitute the state in the very process of using it, he does so in a manner that further mystifies who, exactly, is acting and why they are acting in just these ways.

Why such hesitancy? After criticizing various thinkers for setting the state out from society, Mitchell seems unwilling to investigate the systematic bias that he clearly suspects is present in state practices because he fears falling into the same trap as those he criticizes. To present the state as consistently acting as it does due to pressures (whatever their form) located in society would be to reestablish, he seems to believe, the very separation between state and society that he took such pains to demolish. In short, Mitchell's explanatory framework and commitments keep him from recognizing what, at some other level, he knows to be there.

Is there another way of treating this matter that would avoid this dilemma? Mitchell's own approach suggests that there is. The key to his approach involves extending the notion of the state back in time to include the wide variety of practices by which the institutions of the state came into being. Having stretched the boundaries of the state so far, there can be no objection, in principle, to stretching them a little further in order to include who and/or what are primarily responsible for these practices. Mitchell's conception of the state, in other words, could—and, in my view, should—include the group or groups whose special ties to the state give its forms and practices their distinctive character. This would enable us to explain the patterns we observe in the effects of its forms and practices without introducing a split between state and society. If the everyday activities of citizens helps explain how the state is constituted, it is only the connection between these activities and the requirements of the favored group or groups that can account for why it is constituted in just these ways at just this time. An adequate conception of the state must contain answers to the questions *why* and *when*, as well as to the question *how*.

There is no single answer to where the state ends and where society begins, because these are not neighborhoods separated by broad avenues. Rather, the one performs a range of essential functions within the other. The ways, and even the forms, through which these functions are performed, however, vary considerably depending on both the nature and the stage of development of the society in question. They also vary depending on which group has enough power based on its structural position in society to ensure that its interests generally emerge on top and that the requirements rooted in these special interests get taken as society's requirements. Such a group is more accurately referred to as the "ruling class." With changes in society, in its ruling class, and in what this class requires in order best to serve its interests, the functions of the state will also vary and, along with them, the boundaries that establish state limits, as well as the very meaning of the concept "state" in our discourse.

Functions imply a system, however; and our quest is complicated by the fact that the functions performed by the state belong to three different, though overlapping, systems. These systems are class society, capitalism, and modern American democratic capitalism. They differ from each other in the degree of generality that mark all their qualities—which is to say, our society is a class-divided society and, as such, possesses a host of interacting qualities that are similar to those found in other class-divided societies during the whole period that they have existed on earth (roughly 5,000 to 10,000 years). Ours is also a capitalist society and has many systemic qualities distinguishing it as such, which it shares with all the capitalist societies in the five to six hundred years that this social formation has existed. Finally, our society is a stage of capitalism that has come to the fore under the special conditions that have developed in America in recent years (say, the last twenty to fifty years). What we call "our society," then, is a layered construction composed of sets of internally related qualities that fall on these three different levels of generality, indicative of three different time frameworks.

While the qualities that belong to these three systems are often treated together, we can also distinguish between them; and for certain purposes, it is essential that we do so. As regards the state, it is clear that what is meant by the ruling class, as well as by its requirements, varies somewhat as we move from viewing the United States as a class society, to viewing it as a capitalist society, to viewing it as modern American democratic capitalism.

At the level of class society, the dominant economic class requires political help in reproducing the conditions that underlie its position of dominance. Such help involves various means of socializing the population (especially its most oppressed sections) into an acceptance of the status quo and a repressive apparatus (chiefly, police, soldiers, and courts) that can be called upon when the former do not wholly work. Socialization is aimed primarily at gaining popular acceptance for the institutions of private property, for the basis (whether by divine will, birth, or money) of the prevailing division of labor, and for the idea that the special interests of the dominant class are the same as the interests of the nation. The mystification involved in the latter leads Marx to refer to the state during the entire period of class history as an "illusory community" (1942, 74). It is a "community" because it tries to reproduce the conditions underlying the mutual dependence of all people living in a given geographical area; it is "illusory" because it privileges the interests of one class under the guise of serving the interests of everyone. So much is true of the state in all class societies.

But (as we said) ours is also a capitalist class society; and, as such, it requires political help tailored to the peculiar character and interests of the ruling capitalist class. Perhaps the most distinctive feature of the capitalists as a ruling class is their contant drive to accumulate, to treat wealth primarily as a means to produce still more wealth. While socialization and repression remain essential state functions for capitalism, the nature of accumulation in this period requires certain modifications in the forms through which these functions are carried out. To cite just one

example, workers perform their roles in accumulation more effectively if they feel they have had some freedom in choosing jobs and at least the possibility of influencing state policy in so far as it affects their jobs. All of this gives to the production of capitalist ideology—and to the institutions devoted to this activity—a much more important place in capitalism (and therefore in the capitalist state) than it has in other class societies and their states.

The centrality of accumulation in capitalism also forces the state to undertake a function that does not exist in other class societies and that is helping the capitalists realize the value of their products. Given how capitalism works, capitalists simply require more direct political help in their efforts to enrich themselves than did feudal aristocrats or the slave-owners of antiquity. They require what amounts to state planning on their behalf—and they get it. While rendered mostly invisible by their ideology, such planning covers everything from investing, to borrowing, to keeping production costs down, to buying, to selling the finished products. Most of our tax, trade, monetary, and labor policies (including both laws and their administration) come under the rubric of state planning in the realization of value. The political institutions and practices that serve capitalist interests in these ways are essential parts of the capitalist state.

While Mitchell's examples show corporations and banks only interested in making money (an interest aided by "appearing" private, outside the state), he never examines the system in which this occurs. Consequently, there is no mention of capital accumulation and the kind of help this requires from the state. Had he placed his analysis squarely within a capitalist framework, he would have noticed that appearing separate from society is but one of the ways (though perhaps, ideologically, the most important) in which the state serves the interests of the capitalist ruling class.

Just as ours is a class society and a capitalist society, it is also (as I have said) the modern American democratic capitalist society, and some of what our state is and does pertains to the special interests of the American capitalist class, given its place and role in world capitalism at this stage of its development. If the first set of qualities we examined are shared with all societies within the period of class history and the second with other capitalist societies for the entire period that they have existed, these qualities apply only to the American democratic capitalist state of recent times. Among the new developments that have had a major impact on the character and practices of the state on this level of generality, three seem to stand out. First, the enormous increase in the gross national product, together with the intensification of competition between capitalist societies in the last few decades, has led to the state's taking on a larger and more direct role in the realization of value while, for good ideological reasons, insisting ever more loudly that nothing of the kind is happening. Second, the Cold War has given us the national security state, including the "military–industrial complex," which has greatly facilitated the capitalists' effort to repress internal opposition and bolster their profits by draping both with the mantle of national security. Finally, the development of communications technology has made it possible for the American ruling class to extend the political work of socialization in capitalist ideas and values to the entire globe, transforming the media into an indispensable arm of the state—which is to say, into an aspect of the state itself (or what Louis Althusser dubbed an "ideological state apparatus" [1976, 67–126]).

As our American capitalist class becomes increasingly international, with more foreign capitalists investing in the United States and more U.S. capitalists investing abroad, the political help required to realize value has led to the development of world institutions such as the International Monetary Fund, World Bank, and the General Agreement on Tariffs and Trade, whose economic labels disguise their important political functions. They are the beginnings of a world capitalist state, whose origins and development (as with the rise of the national states that occurred earlier) are best understood as political responses to the changing requirements of the ruling capitalist class on the level of generality of modern capitalist society. The national state in the United States is also losing some of its power to regional political forms, such as the New York Port Authority and the Appalachian Regional Commission, and for similar reasons: the interests of the capitalist class in this period are better served by having certain political functions performed in this way and on this scale. With no democratic accountability to worry about, the technical intelligentsia operating in these arenas can play the role of farseeing statesmen for the capitalist class as a whole.

Barring a socialist transformation, what we also have here is a preview of the next stage of the "American" capitalist state, with the national forms of the state being reduced to performing mainly repressive and socializing functions, while most of the help rendered to capitalists in their drive to accumulate and maximize profits is provided at the world and regional state levels. If we were describing a European country, we would have to add still another level, the multinational level of the European Community, which does not yet exist in the case of the United States but may soon, as the new economic treaties with Canada and Mexico begin to take on a life of their own. How all this will affect the national state's ability to perform its repressive and socializing functions remains to be seen.

Viewing political institutions and practices as internally related to the ruling economic class, as equal and necessary aspects of a more broadly conceived notion of the state, and as falling on three different levels of generality does not keep us from recognizing occasions when a set of unusual circumstances allows some government officials, political parties, and even whole institutions to operate in relative autonomy from ruling-class imperatives. The point is that this is

the exception (and a relative exception at that, meaning short-term and for limited purposes) and not the rule, as is the case in most non-Marxist interpretations of the state. Such exceptions are also largely explainable in terms of the special circumstances that apply. These exceptions also become increasingly rare as we move from the more specific level of modern American capitalism, to the more general one of capitalism, to the still more general one of class society—which is to say that it was easier to have a New Deal (the part of it that favored the interests of workers) than it is to interfere with the structures that underly capitalist—and, more generally, ruling-class —domination.

Nor do I mean wholly to dismiss the struggle by subaltern classes, taking advantage of the problems and contradictions that come with the territory, to use some part of the state to promote their interests. While the cards are carefully stacked against such efforts, it is sometimes possible to win a little or temporarily or to keep a capitalist initiative from succeeding as much as it otherwise would. And by reordering power relations even slightly, every victory resets the odds for the major confrontations up ahead, when the problems inherent in the normal functioning of the system have reached the explosion point. The larger goal of these confrontations can only be to socialize the means of production and in this way deprive the capitalist ruling class of the basis of its power. Should the effort succeed, a new ruling class, representing now the great majority, would organize the state in a way that would best serve its special interests, once again altering the boundaries of the state in the process. The internal relations between state and society (a very changed society, hence a very different state) would remain the same.

If many political scientists treat the state as a kind of "ghost in the machine" (David Easton's not altogether friendly observation, quoted by Mitchell, p. 91), it is only because the machine acts in ways that belie all attempts at explanation that are restricted to the decisions of the people who sit at the controls. Something more and different has to be introduced to help account for the persistent patterns found in how the machine works. Until this is done, all attempts to kick the state out the door (e.g., in the manner of the political systems theorists) will only result in its climbing back in through the window. Recognizing this, Mitchell seeks to highlight the numerous everyday practices that constitute the state in its present form and enable it to function as it does. In so far as these practices are themselves left unaccounted for, however, he has just substituted one ghost for another. It is only by extending the connections that Mitchell *started* to trace back further, in order to take account of the political requirements of our ruling class—grasped as a ruling class in general, as a capitalist ruling class, and as the modern American capitalist ruling class—that the otherwise inexplicable movements of Easton's ghost (and Mitchell's) become comprehensible.[11]

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RESPONSE

In "The Limits of the State" I argued that the state should be approached not as an object, site, or agent, but as a set of powerful yet elusive methods of ordering and representing social practice. These methods create the effect of an agent or structure—the state—that stands outside the social. Most theories of the state respond to this phenomenon in one of two ways. One response is to mistake the effect for something real and reify the state as a self-contained entity. Without grasping the discursive strategies that generate the state effect, such responses merely add to existing representations and become a part of the effect. The other response is to dismiss the effect as illusory and turn attention to the real political forces that stand behind. This overlooks the power and complexity of a discursive order and tries to substitute a simpler, less ambiguous, nondiscursive reality. These alternative responses are illustrated by the two different kinds of reaction to my article published here, Bendix and Sparrow on one side and Ollman on the other.

I first argued that political science had always failed to reach an adequate definition of the state, one that can distinguish it from the social world it seems to order. This is because the boundary between the state and society is elusive, porous, and mobile. I suggested that this elusiveness should not be eliminated by sharper definitions but explored as a clue to the nature of the state. Rather than assuming the existence of a distinct object, we need to bring to light the everyday techniques of difference that create its apparent distinctness.

Sparrow responds with the traditional view that the elusiveness of the state is more a question of "conceptual and methodological ambiguities." He imagines these problems of method to be a separate issue from the discursive ambiguity of the state and offers three solutions:

First, Sparrow says, we must find a definition to get the state "pinned down." This misses the point. A set of practices whose effectiveness lies in making what is elusive, incoherent, and open-ended appear as an object that is ordered, bounded, and univocal cannot be pinned with a singular definition. A definition that is "precise and consistent," such as Sparrow demands, will not capture political relationships whose very nature is to be imprecise and inconsistent.

Second, is the state in relation to society an independent, dependent, or intervening variable? Sparrow claims that I present the state exclusively as a

dependent variable. I do no such thing. The question presumes what my argument denies, namely, that state and society are separable units or spaces. It assumes that each unit has an essence independent of the other and that their relationship can be imagined in the language of nineteenth-century mechanics, in which cause-and-effect is a unidirectional movement between self-contained, externally related objects.

This problem is also found in slightly different terms in Bendix's critique, which faults my article for neglecting "how the state and society influence one another" and complains that I am advocating "the fusion of state and society." He argues, instead, for a study of the dynamic interaction through which "the state and society negotiate their relationship." As with Sparrow, Bendix's argument conceives these constructions of language, cultural representation, and organizing strategy not as aspects of discursive practice but as independently existing objects that enter into relationship with one another. To criticize, as I do, the image of state and society as separate objects is not to advocate their "fusion" into a single object. Rather, it is to propose a less mechanistic and more discursive understanding of how social reality occurs—that is, an understanding that takes the constructed nature of the social seriously, rather than relegating such construction to some secondary realm labeled "culture" or "belief," and that acknowledges that social constructions are not bounded entities with singular identities but strategies and relations that often exceed their limits, become displaced, reverse themselves, or otherwise elude the descriptive realism that sees them simply as objects.

Third, Sparrow imagines the mechanical relationship of state and society to form a structure fixed in place, rather than a process that remains fluid, emergent, and incomplete. He says that we must decide how change affects the state, assuming change to be an exterior force visted upon an otherwise stationary object. All three of Sparrow's solutions to the problem of ambiguity start from the wrong place and end up reproducing the views that my article critiqued.

The second part of Sparrow's response, still picturing the state as a machine, complains that I have not named the operators that run it. He wants me to identify "the subjects who produce difference or maintain order." Why assume that every explanation must lead back to agents who stand outside the machinery and produce or maintain it? Why must our theories recreate the impression that subjects produce difference, never difference, subjects? Sparrow later on contradicts his own criticism by admitting that "procedures, organizational routines, classifications, and framing may determine individual actions." Yet for Sparrow, this relationship must be imagined as reciprocal (the machines running the people who run them), because he works within the dualism that takes the world to be divided into institutions versus individuals, structures versus actions, system versus behavior, and state versus society. He never asks how or why this dualism has come to be so taken for granted. My article attempted to bring these assumptions into view by suggesting that if we are to understand the state, we must see it as an aspect of the history of this dualism. The neoinstitutionalists Sparrow urges us to read cannot help with this.

Nor can Weber. His definition of the state as an organization that successfully claims to monopolize the legitimate use of coercion will not do. As I have argued elsewhere, it is problematic to approach the question of political order in terms of the Weberian distinction between coercive forms of power and the noncoercive, or between force and consent (Mitchell 1990). Too many aspects of the political order cannot be made to fit the terms of the distinction—such as forms of disciplinary, spatial, or institutional power that are physical yet not based on force or forms of violence that create frames of meaning for the language of consent. Weber himself seems to have recognized the difficulties in applying his definition to specific cases. His own writing on the state tends to leave the definition aside and focus, instead, on the professional organization to which it refers. Bendix rightly draws attention to Weber's extensive and important writings on bureaucracy. But this work represents not so much a part of the broader conceptualization of state power that Bendix describes as a narrower, institutionally focused substitute for a definition that Weber himself was unable to make work.

The other kind of response to the elusiveness of the state is to see the state as a fiction, whose apparent power and autonomy disguise the real power of the ruling class that controls it. Ollman proposes that we eliminate the fictional distinction between state and ruling class by stretching the loose boundaries of the state to include the ruling class within it. This would improve upon the prevailing view of the state as an autonomous entity (as my examples of the relationship between state and capital illustrate). But in rejecting the view of the state and the dominant social class as objects or agents independent of one another, one does not have to amalgamate them into a single entity. Ollman seems to acknowledge this by consistently distinguishing between the ruling class as the sovereign agent that rules and the state as simultaneously the instrument and the disguise for the agent's rule.

Yet Ollman's response leaves open some large questions. Nothing he says explains how a group so small as the ruling class can "use" an apparatus so large and amorphous as the modern state to serve so successfully its requirements. Nor does he suggest an adequate account of how this success is so consistently disguised. If the state is the "illusory community," then we need a theory of how such an artificial totality is produced. How do the partial, particular, localized practices of state power come to appear something universal, general and permanent? I argued that the state characteristically appears as a structure, in the specific sense of an abstract, enduring, architectonic frame. How can one explain this

effect of structure? Marx, for all his insight into the process of abstraction that underlies the fetishism of the commodity and the very phenomenon of capital, does not elaborate a theory of structure. He makes frequent use of the analogies of architecture and the machine; but to capture the modern illusion of a structural totality, he tends to fall back on the classical, premodern distinction between essence and phenomenon (cf. Althusser 1970, 184–93).

Offering no analysis of the methods of this illusion, Ollman runs the risk of overstating its effectiveness. His discription of the way the state is used to serve the processes of capital accumulation sounds like a hymn to capitalist efficiency. He admits that there may be exceptions to this ordered functioning of the power of capital but only under "unusual circumstances." The setbacks capital might suffer are never more than small or temporary. Capitalism is thus imagined in its own metaphors, as a complete and unified system whose energies are rationed and whose logic operates without flaw. Fissures are secondary, division is external and short-lived—until some future confrontation that destroys the entire machine. Why not imagine capitalism differently? Why not bring its incoherences and fissures to the center and see the state not as the tool of its efficiency but as a temporary and uncertain coherence somehow engineered?

By subordinating the state to the logic of capital accumulation, Ollman also implies that this accumulation process possesses a necessary and singular calculus. Yet it is far from clear that the variety of forms taken by state practice at different moments and in different places can all be deduced from the logic of accumulation. The work of the regulation school (see Boyer 1990), for example, has been exploring the recent history of different regimes of accumulation and opening up the complexity and variability of the relationship between state practice and methods of accumulation.

Ollman seems to be moving in an opposite direction, reducing five or ten thousand years of global history to just three levels of analysis. Yet even to use a single concept, the state, to cover such a range of historical experience seems liable to mystify as much as it illuminates. Is it appropriate to refer to a five-thousand-year-old requirement of "socializing the population" when even the concept of "the population" has a history of only two or three hundred years and relates to practices of a similar age? Is it appropriate to make the same historical reference to a "repressive apparatus" of police, soldiers, and courts when almost everything we mean by these institutions is equally recent in appearance? Ollman's second level of generalization, "capitalist" societies of the last five to six hundred years, also fails to correspond to the rise of these modern institutions. The innovation that Ollman associates with this time frame, state planning, is even more recent. Finally, Ollman's third level of analysis, American capitalist society of the last 20 to 50 years, corresponds neither to the origins nor to the decline of arguably the most distinctive feature of twentieth-century American capitalism, the Fordist regime of accumulation.

Although I agree with Ollman that it can be helpful to distinguish different levels of generalization and analysis, I find little correspondence between the logical levels he has chosen and the aspects of the state that he wishes to derive from each logic. Even if his three levels were differently or more accurately defined, moreover, it is not clear to what extent they represent separate "systems." At what moment in the last five or six hundred years, for example, and at which sites, does capitalism distinguish itself as a "system" from other systems around it? And why are there only three levels? Could we not discern additional, intermediate levels of analysis and numerous other systems and logics? Even then, might we not find that at each level and for each time frame state practice did not follow a unique pattern but developed a variety of forms and strategies—strategies that on occasion reverse themselves, fail, produce accidental side effects, or otherwise escape the kind of singular and mechanical logic that Ollman envisions?

These uncertainties do not imply that questions of capital accumulation and class power are to be ignored, as they are in so much of the political science literature on the state. Rather, they suggest the need to explain, rather than assume, the relation between larger logics of accumulation and the local mechanisms of state power.

It was in this context that my article emphasized the need to examine the micro, everyday practices that generate the effect of the state, drawing in part upon the work of Michel Foucault. Ollman and Bendix see this emphasis as an attempt to present an alternative to the macro analysis of the state; in fact, it was offered only as a place "to begin." The potential of Foucault's micro analysis of disciplines for developing a macro theory of the state is recognized by Nicos Poulantzas in his later writing, which is strongly influenced by works like *Discipline and Punish* (Foucault 1977). (American scholars tend to ignore the later work of Poulantzas; in fact, the English translation of *State, Power, Socialism* [Poulantzas 1978] has never been published in the United States.) But as Jessop (1990) points out in one of the best recent contributions to state theory, Poulantzas leaves unresolved the question of how the variety and incoherence one finds in disciplinary strategies at the micro level relates to the general pattern of power in the capitalist state. Foucault himself advocates what he calls an "ascending analysis of power," starting from its "infinitesimal mechanisms" and then examing how these come to be utilized, displaced, transformed, or maintained by "global mechanisms and the entire state system" (1980, 99–101). But he never actually conducts this examination, never explains how a global social order can be built out of such a diversity of infinitesimal strategies.

It was, in effect, to this gap in Foucault's work that the latter part of my paper was addressed. I proposed that the distinctiveness of those modern techniques of organization I termed "enframing" lay in the

power to create an effect of order, abstraction, and totality out of mundane methods of distribution and control, an argument I have developed at greater length elsewhere (Mitchell 1988). Rather than deriving these forms of political power from the logic of capital accumulation, as Ollman proposes, it would be more appropriate to consider how capital itself is a phenomenon that is generated from these same methods of abstraction—methods of creating a seemingly abstract, nonpersonal frame of order, meaning, or value out of a diversity of infinitesimal processes of arrangement, representation, and control. One could then explore the relationship between capital and the modern state not by reducing one to the possessor of political power and the other to its instrument but by treating both as aspects of a more general discursive process of abstraction. These are questions that the kind of mainstream conceptualization of the state to which Bendix and Sparrow wish us to return would never enable us to pose.

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Notes

Sparrow thanks Mike Dennis, Laura Jackson, Bernard Silberman, and the members of the University of Chicago's Organizations and State-Building workshop and the University of Texas's American Politics workshop for their remarks.

1. This is not an argument for any particular variety of "American exceptionalism." For an overview of American exceptionalism, see Katznelson 1980, 3–17. See also the essays in Shafer 1991.

2. Miliband does not show why a state manager's individual actions need to be consistent with the collective dependence of the state on capitalist interests. Nor can Offe say why individual state actors should necessarily act in the collective interest of the state. Poulantzas, on his part, provides no microfoundations for the state's behavior, leaving the reader with a black-box state. Block, while providing for individual state actors, has difficulty in identifying "working-class pressures" and in dealing with the possibility that a state manager may default from acting in the general interest of capital. See Carnoy 1983 and Alford and Friedland 1984 for overviews of Marxian scholarship on the state.

3. Mitchell omits mention of Krasner's (1984) analysis of state-centered work. Krasner observes, "From the late 1950s until the mid-1970s the term state virtually disappeared from the professional academic lexicon" (p. 223). Krasner goes on to explore "the extent of state autonomy and the degree of congruity between the state and its environment" (p. 224). He proceeds to frame the statist approach in the context of the behavioralist revolution. Krasner thus not only touches on the boundary question but is also very much interested in the intellectual history of, and different perspectives on, the state.

4. Skowronek, on his part, defines the state to be at once composed by a coercive power settling class conflict at the national level; an "integrated organization of institutions, procedures and human talents" at the meso level; and individuals maintaining order at the micro level (1987, 11–12).

5. Nordlinger's (1981) *On the Autonomy of the Democratic State* is a thought experiment on state autonomy and intentionally omits empirical evidence.

6. Skocpol uses *the state* to refer to the societally autonomous cause of particular political outcomes—the state as an independent variable (Ikenberry and Skocpol 1987; Skocpol and Ikenberry 1983; Weir and Skocpol 1985). But *the state* may also refer to institutions mediating crisis or societal changes in order, again, to cause specific policies—the state as an intervening variable (Amenta and Skocpol 1988). Finally, the state may consist of the structures of government themselves, brought into being through preceding policies, particular alignments of political mobilization, and certain systems of representation—the state as a dependent variable (Skocpol 1979, 1981).

7. For Skowronek, "state building" consists of a convergence of tensions provoked by crisis, class conflict, and complexity—a convergence mediated by existing institutions and electoral politics.

8. See Burawoy 1989 for a criticism of Skocpol's (1979) methodology in *States and Social Revolutions*. Burawoy argues that Skocpol's inductivist method obscures the actual theory, that the method is unfalsifiable, and that it is ungeneralizable, that is, a summary of facts that subsequent, uncovered facts could disprove.

9. I say "point of departure" because research on organizations and institutions has gone in a number of directions: the transactions costs approach, with the economic transaction as the basic unit of analysis (e.g., Oliver Williamson, Douglass North); the study of law and economics, with a focus on the contract (Ronald Coase, Richard Posner); the new economics of organization, looking at principal–agent relations (Kenneth Shepsle, Barry Weingast, Terry Moe); and work in international relations (Robert Keohane, Stephen Krasner).

10. This "new," or "neo-,"institutionalism may be distinguished from the earlier institutionalism on the grounds of its response to the behavioralists' comparative neglect of social context and the durability of social institutions, an explicit concern with theory and methods, and a response to empirical anomalies—to wit, the lack of convergence between the study of organizations and politics and rational actor or functionalist accounts of these phenomena (Powell and DiMaggio 1991, 2–3).

11. For a more detailed exposition of this analysis see the chapters "Putting Dialectics to Work: The Process of Abstraction in Marx's Method" and "Theses on the Capitalist State" in my book, *Dialectical Investigations* (Ollman n.d.).

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FORTHCOMING IN MARCH, 1993

The following articles, research notes and controversies have been tentatively scheduled for publication in the March 1993 issue:

James Q. Wilson. "1992 Presidential Address: The Moral Sense."

Alberto Alesina, John Londregan, Howard Rosenthal. "A Model of the Political Economy of the United States."

David P. Baron. "Government Formation and Endogenous Parties."

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POLITICAL REGIME CHANGE: STRUCTURE AND PROCESS-DRIVEN EXPLANATIONS?

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The Faces of Fraternalism: Nazi Germany, Fascist Italy, and Imperial Japan. By Paul Brooker. New York: Oxford University Press, 1991. 397p. $69.00.

To Craft Democracies: An Essay on Democratic Transitions. By Guiseppe Di Palma. Berkeley: University of California Press, 1990. 248p. $35.00 cloth. $11.95 paper.

The Third Wave: Democratization in the Late Twentieth Century. By Samuel P. Huntington. Norman: University of Oklahoma Press, 1991. 366p. $24.95.

Liberalism, Fascism, or Social Democracy: Social Classes and the Political Origins of Regimes in Interwar Europe. By Gregory M. Luebbert. New York: Oxford University Press, 1991. 416p. $49.95 cloth, $19.95 paper.

Capitalist Development and Democracy. By Dietrich Rueschemeyer, Evelyne Huber Stephens, and John D. Stephens. Chicago: University of Chicago Press, 1992. 387p. $45.00 cloth, $19.95 paper.

Political regimes may be defined as the rules and basic political resource allocations according to which actors exercise authority by imposing and enforcing collective decisions on a bounded constituency. Over the past decades, profound political regime transformations in southern Europe, Latin American, East Asia, Eastern Europe, and perhaps now even Africa have revived political scientists' concern with the breakdown and replacement of political governance structures. The main theoretical division within this field is drawn between those who seek more "structural" and "configurational" explanations, on the one side, and those who focus on the process of change itself—the sequence of events and the strategic moves of the actors.

The differences between the two camps encompass theory, methodology, and research design. Structure-oriented scholars typically assume that historical actors face extremely narrow choice sets *or* that their rational choices are clearly constrained by the distribution of resource endowments and their exogenous interest to maximize income and/or power, in light of which they calculate optimal strategies. Process-oriented scholars, in contrast, are primarily interested in the actors' manipulation of their own and their adversaries' cognitive and normative frames. Such change may eventually bring about political regime changes that were neither anticipated nor desired by any of the participants at the beginning of the process.

In other words, the main division in the field of studies on regime transformation is *not* one between historical and political institutionalists, on the one hand, and economic institutionalists with rational choice models, on the other. These two currents represent only strands within the structuralist camp. At most, the two differ on the nature of actors' interests (self- or other-regarding) and the relationship between interests and structural constraints (exogenous or endogenous). What is at stake between structure- and process-oriented studies of political regime change is a more fundamental division concerning the *concept of choice in political action itself*. For structuralists, choices represent calculations in light of given preferences and institutional constraints.[1] For process-oriented scholars, choices are caught up in a continuous redefinition of actors' perceptions of preferences and constraints.[2]

I shall focus the debate within the structuralist camp and between structuralists and process-oriented approaches on the question whether Barrington Moore's seminal hypotheses about regime trajectories in *The Social Origins of Dictatorship and Democracy* (1966) can still be considered valid. In a nutshell, Moore argues that the deconcentration of political power and economic power before the advent of industrialization (precipitated by a commercialization of land, a rising bourgeoisie, and a fragmentation of state power) was beneficial to democratization. At least four of the five structuralist and process-oriented approaches I shall examine address these hypotheses directly or indirectly.

Structure- and process-oriented approaches also differ on methodological and design-related questions. Structuralists prefer systematic macroquantitative or conceptually disciplined qualitative comparison, with countries as units of analysis. As the books by Huntington, Luebbert, and Rueschemeyer, Stephens and Stephens testify, followers of the comparative case study methodology have also ventured to increase the number of cases they compare and thus to lend more credibility to their main theoretical contentions.[3] In contrast to structural approaches, the process-oriented literature revolves around descriptive diachronic reconstructions of individual cases of regime transition with very little systematic comparison across a wider universe of countries. Whatever comparison is brought in remains on the illustrative and impressionistic level. I know of no single process-oriented study that supplies a systematic comparative analysis of regime transitions for a large number of instances.[4]

I wish to argue that structuralist and process-oriented analyses do not directly compete with each other but focus on different objects of explanation with different research methods and comparative designs. Taken as absolutes, structuralist approaches tend to explain "too much," whereas process approaches explain "too little." Structuralist approaches are good at accounting for the general causes of

regime breakdown and the consolidation of new regimes. Process approaches may explain the timing of breakdown and transition as well as the specific trial-and-error process of searching for a new viable regime. Structuralists have little to say about timing and transitions; nor have process-oriented studies succeeded in predicting regime consolidation. As far as the probability of democratic consolidations is concerned, a modified version of Moore's arguments still appears to have considerable plausibility.

STRUCTURALIST APPROACHES

Gregory Luebbert concludes his study of regime change in the interwar period with an unabashed structuralist proclamation: "One of the cardinal lessons of the story I have told is that leadership and meaningful choice played no role in the outcomes" (p. 306). Drawing upon a wide range of cases essentially covering the entirety of Europe, including the Balkan states, Luebbert claims that it is class cleavages and class alliances articulated on the level of group and party mobilization that determined whether regimes became fascist, liberal democratic, or social democratic in the interwar period. In a nutshell, the strength of liberal democratic free-market-oriented parties before and after World War I is key. Such parties were furthered before World War I by the absence of religious, ethnic, or regional cross-class cleavages that could have divided nonsocialist voters. Dominant liberal parties were able to enter alliances with the emerging labor parties against entrenched conservative and antidemocratic elites and thus weakened and coopted the labor movement in a democratized capitalist order (Britain, France, Switzerland). Whereas strong liberalism with weak labor movements persevered during the interwar decades of economic crisis, divided bourgeois parties and strong labor mobilization before and after World War I led to fascist regimes where the peasantry threw in its lot with the reaction (esp. Germany, Italy, Austria, and, to a lesser extent, Spain) and to social democracy where the peasantry entered red–green coalitions (Scandinavia). In eastern and southern Europe, mass political mobilization, particularly that of the working class, had not proceeded far enough to yield anything beyond an authoritarian dictatorship, regardless of whether fascism, liberalism, or social democracy are considered (pp. 258–66).

Whereas Barrington Moore saw large landholders and labor-repressive agriculture as a key force promoting fascism in the twentieth century, Luebbert maintains that by the 1920s, the German Junkers no longer played a critical economic role and that the landed elite did not control the votes of rural laborers anyway (p. 309). Luebbert's argument, however, fails to recognize that an application of Moore's thesis to twentieth-century regimes must necessarily emphasize the imprints past class dominance had left on the political organization of the state and on the mobilization of the peasantry. In fact, where feudal and absolutist regimes based on large landowners had disappeared early, fascism had no basis for success because either the state organization (military, bureaucracy, judiciary) was inimical to a new authoritarianism or a class of family farmers had an independent political voice that usually favored democratization, or both.

The critical cases undermining Luebbert's arguments are Belgium and the Netherlands, included among the countries he classifies as without hegemonic liberalism before World War I; yet contrary to his theoretical expectations, they did not produce fascism in the interwar period. Both countries are, however, characterized by the early demise of feudalism, the dominance of family farms, and a large market-based middle class.

Luebbert's argument that late industrialization has nothing to do with the failure of liberalism (pp. 59–62) also rests on weak foundations. Timing may not be as important as the character of late industrialization in critical countries in order to account for regime outcomes. Late industrialization is often associated with an initially state-led process of industrialization yielding a highly "organized" capitalism with a bourgeoisie closely intertwined with an antidemocratic state apparatus. If there were no offsetting conditions, such as an independent liberal–democratic peasantry (Sweden), this pattern of industrialization favored authoritarianism (Germany, Japan). Again, this interpretation explains why Belgium and the Netherlands escaped authoritarianism in spite of cross cleavages dividing liberal parties. Both countries experienced a gradual and relatively spontaneous, decentralized process of industrialization.

Although Luebbert is a structuralist, he is forced to admit the contingency of political choice in at least one critical instance. He argues that whether or not farmers allied with the Left or with fascism essentially depended on the socialists' choice to mobilize farm workers. If socialists engaged in that strategy, they tended to unite midsized farmers and large landholders against the Left and thus prepared the way for a fascist coalition, whereas in other instances red–green alliances were feasible (pp. 277–95).[5] In the spirit of Luebbert's structuralism, however, it must be noted that countries where peasants coalesced with fascism had a different legacy of agricultural property rights than countries where they did not. Conversely, if historical patterns of landholding and their political consequences did not matter, political choice on the part of socialist party leaders is clearly critical in accounting for interwar regime outcomes. Luebbert cannot have it both ways, that is, both deny the political consequences of rural political economy and provide a structuralist account of interwar regime outcomes.

Overall, Luebbert's account convincingly demonstrates that structures matter, but he may push this point too far. For example, the *timing* of the antidemocratic relapse in Germany, Italy, or Spain could hardly be accounted for by his model. Moreover, structuralism yields no deterministic, but a probabi-

listic, prediction. Even if all the structural constraints were right, would there have been a rise of the German Nazi party to power in 1933 had it not been for the Depression and the unique form of elite politics ushered in by the provisions of the Weimar constitution?

Luebbert's historical account yields rather pessimistic conclusions for the future of democratization in the late twentieth century. Liberalism and democracy were compatible only before the working class acquired the capacity to mobilize and disrupt the operation of free markets early in this century. Liberal–Labor alliances, modeled on pre–World War I Britain or France, between the urban middle class and the working class are thus extremely unlikely (p. 315). At the same time, a rural middle class was critical in the past and does not appear to be present in most countries still facing the possibility of democratization. Luebbert also does not hold out much hope for a "tutelary," restrained democratization under elite control because it will only compound the problems of democratic stabilization (p. 313). Under these circumstances, he envisions instability as the only "stable" outcome, "a cycle of establishment of democracy and its collapse, military intervention, and withdrawal" (p. 315).

Luebbert's book is an impressive achievement in its comparative scope and its unrelenting, tenacious defense of a clear analytical thesis; yet it ultimately leaves too many empirical anomalies unaddressed, maybe because Luebbert did not take Barrington Moore seriously enough. In contrast, Rueschemeyer, Stephens and Stephens's *Capitalist Development and Democracy*, though analytically more complex and less tightly knit, incorporates more aspects of Moore's thinking and arrives at more plausible and valid explanations of an even wider range of cases than Luebbert considered. The authors' objective is to synthesize two hitherto antagonistic traditions in research on regime change and apply the synthesis to regime change in today's advanced industrial countries (Western Europe and British settler democracies), Latin America, and the Caribbean. Macroquantitative studies have consistently found that economic development furthers democracy. Comparative historical analyses in the political economy tradition from Weber to Moore, however, have emphasized class relations and political institutions. Rueschemeyer and his coauthors argue that comparative historical political economy provides the *mechanisms* that explain why, when, and how economic development translates into democracy. Moreover, political economy can explain the cases that remain "outliers" in macroquantiative analysis.

Greater explanatory specificity in accounts of regime transformation requires examination of three sets of variables. First, class interests and class alliances matter, but in ways that modify Barrington Moore's master thesis. Consistent with Moore, Rueschemeyer, Stephens and Stephens show that labor-intensive and labor-repressive agriculture under the direction of large landowners is inimical to democracy. They also demonstrate that in the late nineteenth and early twentieth century the political effects of long-term political hegemony of landowners on state structures (military, bureaucracy, judiciary) may be as important for political regime forms as the direct impact of agricultural property rights.

Contrary to Moore's thesis, Rueschemeyer, Stephens and Stephens argue that the bourgeoisie is generally inimical to democracy. The true source of persistent democratic drives is working-class mobilization, which, in combination with middle-class support, can bring about a political constellation favorable to democracy. The authors qualify their emphasis on the working class in several respects. When turning to Latin America, they note that the middle class is critical in bringing about full democratization (pp. 167, 181, 216–7). Even for advanced industrial societies, however, the subordinate role of the middle classes may be questioned. If the working class is mostly for democracy, it is really the middle class—a concept the authors define as "urban professionals, state employees and employees in the private sector, artisans and craftsmen, and small entrepreneurs, sometimes joined by small and medium farmers" (p. 185)—whose variable position determines regime outcomes. A good part of Rueschemeyer, Stephens and Stephens's middle class may be covered by Moore's concept of bourgeoisie, thus reducing the gap between the two accounts. Going beyond simple structural accounts of stratification, Rueschemeyer, Stephens and Stephens emphasize that class formation is an organizational process, rather than a political–economic configuration of property rights. But they might have further developed this insight in systematic comparisons of working-class, "middle"-class, and bourgeois-class formation to highlight their position vis-à-vis Moore.

The other two sets of variables explicitly theorized in Rueschemeyer, Stephens and Stephens's explanatory framework are state structures and transnational power structures. In general, the more resources state elites control independently of socioeconomic classes and the more they represent a hierarchically integrated and ideologically united state apparatus, the more likely it is that authoritarian regimes will take hold. The precise nature of state influence depends on the countervailing force of independent voluntary associations ("civil society") and the nature of the dominant class from which the state is autonomous. Also, transnational power structures have highly contingent effects. In general, dependency in the periphery of the world system involves mechanisms unfavorable to democracy (foreign direct investment, capital-intensive and sectorally constrained industrialization, or labor-repressive and labor-intensive agriculture). But the authors also show that British colonialism and its legacies in state structures also had indirect effects that kept the "British" Caribbean, with few exceptions, democratic in the post–World War II era, whereas the "hispanic" Caribbean under U.S. influence experienced some of the most repressive and exploitative dictatorships of

that era. In addition, other international factors, such as war, the Cold War, and alliance systems, have significant but varied effects on regime change.

Whereas these three sets of variables (particularly class structures and alliances) tend to account for democratization in advanced industrial countries very well and also cover the four cases of authoritarian breakdown (Italy, Germany, Austria, and Spain [pp. 99–121]), the discussion of Latin America and of the Caribbean requires a further set of variables, namely, the institutionalization of political parties. The authors discover that only where parties are institutionalized as lasting competitive organizational alternatives (whether based on clientelistic linkages or programmatic differences) do democracies survive for any length of time. It is somewhat unfortunate that the systemic role played by parties is not emphasized also for Western democratization. Characteristically, in Germany, Italy, and Spain the nonsocialist parties did not solidify around firm party organizations in the interwar period and were easily swept away by fascism. Austria is the exception, but here a full-fledged fascism was imposed only through Germany's annexation of the country. The importance of parties in twentieth-century political regime change would have allowed the authors to marry Moore's and Luebbert's central insights in a more encompassing framework.

The authors touch upon the role of ideology in regime change briefly but postulate that it is "linked to structural and organizational realities" (p. 50). In particular, this applies to religion. While the authors maintain that religious organization is more important than church doctrines (p. 275), the two elements are usually so highly intertwined that it is not clear in which direction the causal arrow runs. Moreover, lacking a close analysis of countries beyond the reach of Christian doctrines and church organizations, it is hard to determine how important either aspect of religion is for political regimes.[6]

Given the complexity of the variables Rueschemeyer, Stephens and Stephens take into account, what is the analytical bite of their study? What events and trajectories of regime change does it rule out? The discussion of the Latin American cases (preceded by a 12-page summary of the main argument), in particular, is so complex that it is not entirely clear in the end what has been achieved in terms of explanatory parsimony. Rueschemeyer, Stephens and Stephens do wish to argue against a facile modernization theory associating industrialization with democracy; yet at the same time, they embrace it as a partial explanation. They emphasize the role of the working class but do not get around the role of the middle class. The discussion of state structures and transnational relations leaves so many options and contingencies open that just about anything appears to be possible. One wishes the authors would have explored some less complex structural explanations, such as Ronald Rogowski's elegant and provocative account of political cleavages and regime alternatives in *Commerce and Coalitions* (1989) to show that a complex approach still combines the virtues of greater empirical verisimilitude and of discrimination between predicted, possible, and impossible pathways of regime transition.

At times, one has the impression that Rueschemeyer, Stephens and Stephens try to convert every contingency into a structural historical determinacy in order to exorcize the role of a process-oriented account of regime change. Particularly in Latin America, where it is often hard to say whether regimes ever "consolidated," this standard may be driving the structuralist approach too far. Rueschemeyer, Stephens and Stephens argue against the "presentism" of process-oriented explanations of democratic regime transitions (p. 35); but they do not confront these explanations with their own findings. The complexity of their account is rooted in the ambition to explain regime trajectories exhaustively and represent complex configurations and sequences of causation. In light of the result documented in their book, however, more than a few readers may conclude that the isolation of individual mechanisms affecting political regimes and the selective consideration of contingencies to account for outliers promises to provide a more manageable structuralist research strategy than explanatory holism. Nevertheless, Rueschemeyer, Stephens and Stephens have produced a splendid book that will definitely further theorizing on political regime change.

In the spirit of Moore, there is one very important hard-and-fast message that consistently comes across in Rueschemeyer, Stephens and Stephens's study: countries with labor-intensive and labor-repressive agricultures will not become stable democracies. This message also influences their concluding speculations about the future of democracy. In Taiwan and South Korea, the absence of a landlord class and rapid industrialization combined with the strength of autonomous states yields moderately favorable prospects for democracy. The decline of agriculture has also brightened hopes in Latin America; but the weakness of working-class organization, state and military autonomy, dependence on foreign capital, and the lack of strong parties cast dark shadows over the future of democracy on that continent. In Eastern Europe, finally, the *nomenklatura* may constitute the functional equivalent of a landlord class and the large autonomous state apparatuses within weak civil societies also do not bode well for democracy. Even in the early 1990s, structuralists like Luebbert or Rueschemeyer, Stephens and Stephens arrive at muted-to-pessimistic conclusions about the future spread of democratization.

PROCESS-ORIENTED APPROACHES

Whereas structuralists hold actors' preferences constant and focus on changing institutional constraints, process-oriented studies of regime transformation emphasize the contingency of all parameters of choice. In this regard, Paul Brooker's *The Faces of*

Fraternalism represents an analysis at the crossroads between the two perspectives. It is emphatically rooted in Durkheimian sociology in its emphasis on the changing role of a comprehensive collective conscience/consciousness for societal integration and regime stability. Yet Brooker is careful to explain in the first three chapters of his study that efforts to revive a communitarian spirit by "fraternal" political regimes in Germany, Italy, and Japan in the first half of the twentieth century cannot be explained in social-structural terms. Fraternal appeals did not initially inspire a mass following; nor did it necessarily come to the fore before, or even soon after, national socialism, fascism, and its Japanese military variant came to power. Thus, contrary to Durkheim, fraternalism does not grow out of a segmental division of labor with mechanical solidarity but somehow emerges in advanced, functionally differentiated societies with organic solidarity, something that would appear to be an anachronism to Durkheim. For this reason, Brooker offers a quasi-rational explanation of fraternalism in twentieth-century politics. In order to consolidate domestic regime entrenchment and to mobilize psychological resources for their external imperialist plans, totalitarian regimes decided to engage in fraternalist appeals and mass organizations (p. 74). Over more than two hundred pages, Brooker then provides a detailed account of the organizations and mechanisms each of the three regimes employed to instill fraternalism in the population. The concluding assessment argues that fraternalism was carried furthest in Japan yet remained more or less unsuccessful in Germany and Italy. In the most successful case, however, structural conditions for fraternalism were most conducive.

Brooker warns his readers on the first page of the preface that "most political scientists and sociologists will view the book as conceptually lightweight" (v); and this, indeed, is the book's major problem. Its conceptual apparatus and explanatory thrust could have been strengthened by engaging in a variety of related or competing theoretical approaches to fascism and to the rise of segmental or "pillarized" mass parties and reactionary social movements in order to explore why these, but few other, countries were vulnerable to the lures of a totalitarian order appealing to a new spirit of particularist (racist, nationalist) community. In terms of comparative design, Brooker samples on the dependent variable. Fraternalist regimes would appear in a much starker light if they were contrasted to regimes that failed to become fraternalist when faced with similar decision problems and political crises. Why is it that fraternalist appeals struck a resonant cord within elites primarily in Germany, Italy, and Japan but not in most other industrial countries? Brooker does not answer this question. In the end, Brooker provides an instructive description of the internal institutions of totalitarian rule in the three countries but little more. The book's greatest weakness, in fact, may be that it does not discuss alternative structuralist comparative accounts of regime change, such as the strand of analysis to which Barrington Moore had contributed.

Guiseppe Di Palma's *To Craft Democracies* has rather different ambitions and is firmly rooted in the "proceduralist" view of democratic regime change. Not structural conditions but human actions achieve democratic consolidation. At the center of Di Palma's concerns is the process of "democratic crafting" involving "negotiated agreements" between challengers of the old order and incumbent elites that move common perceptions of self-interest toward accepting democracy as the best possible regime form under given conditions. The establishment of democracy, unlike that of other systems, does not legislate a particular distribution of resources among social groups, but institutes uncertainties over outcomes as Adam Przeworski (1991) argued in *Democracy and the Market*. For this reason, Di Palma believes that the game between authoritarian elites and challengers can be more easily converted into a positive sum game, provided that the right steps in the process of regime transition are undertaken. Authoritarian elites must be assured of "coexistence" and fair play. For this reason, the transition should be engineered through negotiated agreements (pacts) that focus on rules of decision making, rather than outcomes. Demonstration effects and the diffusion of successful examples from abroad may be important in precipitating such steps. Careful crafting can exploit the elites' internal divisions on strategy and their sense of uncertainty about their own interests. Di Palma demonstrates these strategies of *garantismo* with an analytical reconstruction of the Italian and Spanish experience and contrasts it to the Portuguese case, where democratization almost failed.

Di Palma then discusses a number of tactical devices that facilitate the transition process. In addition to pacts, a speedy timetable of electoral reform and balanced concessions among all participating forces pave the road to democracy. Over time, the rules that are instituted generate their own support, once actors devise their strategies around the new opportunity structures. For this reason, "legitimacy," as a normative belief in the correctness of the system of rules, is certainly not a precondition of democracy. Institutional habituation of actors' strategies to democratic procedures is the overriding goal of democratic crafting.

While not denying that tactical moves on the part of democratic challengers and skilled techniques of converting games into positive-sum payoff structures facilitate a transition to democracy, we must ask how far such processes carry us in explaining democratic consolidation? Much of his attack on structuralism targets a vague modernization-theoretic version that stresses democratic values and elite attitudes yet not the more hard-nosed comparative studies in the tradition of Moore and others that focus on property rights, class relations, and state institutions. After Di Palma raises some rather strong claims for procedural analysis in the opening chapters of his book, he takes much of them back in chapter 8 where he begins to

compare, in a rather impressionistic and unsystematic manner, the structural conduciveness of different regions and countries to democratic crafting. Such crafting, taken by itself, is insufficient to explain democratic prospects in particular cases: "Even when we favored explanations that focus on the transition and its strategies, these in turn have begged for their own explanations—often of a deeper historical and structural nature" (p. 156). The final chapter of the book also raises the importance of international system configurations and concedes that hegemonic powers play a critical role in regime change.

In the end, Di Palma's book provides several interesting insights into transition techniques that may make a difference in countries characterized by roughly similar structural conduciveness to democratization. These observations, however, could be analytically sharpened. For examples, Di Palma's study puts a great deal of emphasis on pacts and agreements between incumbent and challenging elites yet never engages in a specific analysis of the different scope, bindingness, timing, and longevity of pacts in the transition process. More importantly, the book does not deliver a specification of the structural circumstances that may give significance to different techniques of crafting for the ultimate outcome. Di Palma delivers a timely reminder that economic, political, and cultural determinants are not everything when we try to understand regime change but does not present a sufficiently crisp and precise analysis, corroborated through systematic comparison, to advance the emerging field of "transitology" in significant respects. Whereas structural approaches explain too much, Di Palma's "transitology" explains too little. For this reason, Di Palma's optimism requires a more robust theoretical base.

COMBINING STRUCTURAL AND PROCEDURAL ACCOUNTS OF DEMOCRATIZATION

Samuel Huntington, in *The Third Wave*, lays out a broad, eclectic canvass that encompasses structural and procedural arguments. He also considers the most comprehensive universe of cases and treats them in a more rigorous comparative analysis than does Di Palma; but he is less systematic than Luebbert or Rueschemeyer, Stephens and Stephens in tracing the impact of different sets of variables on regime patterns. Huntington resists efforts to pigeonhole his account as "nomothetic" or "ideographic" from the very beginning (P. xiii). Democracies are complex phenomena, and they are caused by many forces' contributing to historical "waves" of democratization. In contrast to the first two waves following on the heels of the two world wars in the twentieth century, the third wave of democratization beginning with the 1974 Portuguese revolution appears not to be triggered by international political events but by a variety of domestic forces. It is important to note that that 23 of the 39 countries democratizing since 1974 had at least one previous democratic experience, whereas most countries that had never been democratic stayed authoritarian until 1990 (p. 44). This association reestablishes a linkage to externally induced democratization in previous waves.

Overall, Huntington sees five causes of the third wave—at least three of them structural but one unambiguously procedural. First, authoritarian regimes are unable to dissociate the regime's economic effectiveness from the legitimacy of rulers or the legitimacy of rulers from legitimacy of rules of governance. Hence, the oil shocks and the economic dislocations and structural transformation of the world economy in the 1970s and 1980s precipitated the downfall of many dictatorships. With the decline of communism, "outside of Africa and a few countries elsewhere, democracy had come to be seen as the only legitimate and viable alternative to authoritarian regimes of any type" (p. 58). Second, Huntington stresses the importance of economic development but sees it as neither a necessary nor a sufficient condition. Going beyond older modernization approaches, he argues that the source and distribution of wealth are important. Hence, the oil-rich, state-centered regimes did not become democratic; but countries that generate a broad-based urban middle class through a wide diversification of a growing industrial economy did. Huntington's analysis is here quite close to Rueschemeyer, Stephens and Stephens's main hypothesis and certainly consistent with the overall gist of Moore's earlier analysis. Third, however, Huntington emphasizes the independent effect of religious doctrine and religious change. Reversing Weber's causal chain, he notes the rise of Christianity—particularly an individualist and socially activist Christianity—in democratizing countries that has forced Catholicism to withdraw support from the political status quo. Fourth, beyond these structural changes, new transnational politics effected by the United States, the Soviet Union, the European Community, and the Vatican improved the atmosphere for democratic change in the 1980s. Finally, the democratic wave began to feed on itself. Demonstration effects and snowballing reinforced opposition efforts to bring about democracy in countries where structural conditions were even mildly conducive to regime change.

Huntington shows that the mode of transition interacts with the nature of incumbent political regimes (p. 133); thus, he goes significantly beyond the structuralism/proceduralism divide. Elite-initiated or elite-negotiated transitions are most typical in bureaucratic one-party or military regimes, whereas personal "sultanistic" dictatorships hold out less promise for change and often require the wholesale replacement of the ruling elite. For each of the patterns of transition, Huntington provides detailed descriptions of actors' moves and tops off each analysis with a set of lessons actors might draw. For example, reformers should always participate in elections, because even rigged elections, monitored by

foreign observers, weaken an authoritarian regime. In any case, rulers usually miscalculate their chances in open elections, except in cases where there is only a small urban middle class (p. 179).

Huntington finally discusses various aspects of democratic stabilization, such as how to decide on the prosecution of authoritarian elites and how to deal with the military. The most interesting segment deals with the prospects of democratization of countries that have made initial steps in the third wave of democratization (pp. 270–79). Based on their previous democratic experiences, economic development, location in the international system, and duration of democratization effort in the third wave, Huntington is least optimistic about Mongolia, Sudan, Pakistan, Nicaragua, Romania, Bulgaria, Nigeria, and El Salvador.[7] He remains especially pessimistic about democratic prospects in regions of the world that have not entered democratization, especially homegrown Marxist–Leninist regimes linked to nationalist appeals, sub-Saharan Africa, Islamic countries, and certain areas of East Asia. Huntington makes a special point to highlight the antidemocratic implications of Confucian and Islamic religious doctrines, a factor not considered by any of the other studies.[8]

Huntington's analysis is impressive in scope; but at times he is compelled to trade sophistication in conceptualizing critical variables for breadth of comparison. Rueschemeyer, Stephens, and Stephens's account of economic development, class structure, and democratization tends to go in the opposite direction. Clearly, Huntington's book does not provide as detailed an analysis of political institutions and regime structures preceding democratization (pp. 110–21). Huntington's global reach, however, has its own virtues; it encompasses structure and process and, due to its global reach, is able to say more on such contentious issues as the role of religion for political regime transformation.

Overall, the two studies from which most can be learned for the further analysis of regime change are Huntington's and Rueschemeyer, Stephens, and Stephens's. Luebbert's study is a close runner-up. All of these analyses in one way or another feature the importance of Barrington Moore's insights and of the tradition on which Moore built. All of them also refrain from extreme optimism or pessimism about the future avenue of global democratization. Huntington and Rueschemeyer, Stephens, and Stephens lead us to hypothesize that in the Middle East or sub-Saharan Africa, but also in some of the postcommunist successor regimes, chances for democratic consolidation are poor. Conversely, there are a number of countries and regions of the world that give reason for hope to those who welcome democracy.

Against the backdrop of these studies, what future avenues may be open for analyzing political regime change? Luebbert and Rueschemeyer, Stephens, and Stephens have probably pushed the case-oriented systematic historical–comparative approach as far as it can productively go. Conversely, Huntington offers a broad, variable-oriented comparison, which still remains bivariate in its consideration of causal elements. In the future, multivariate analysis or Boolean algebra, as proposed by Charles Ragin (*The Comparative Method* 1987) may help us to develop a more formal representation of casual complexity in pathways of regime transition. At the same time, it is desirable to spell out more clearly the precise mechanisms that make each causal force relevant if we are to see sharper analysis of regime transitions.

Notes

1. For examples, Ronald Rogowski's *Commerce and Coalitions* (1989) is an avowedly rational-choice-based account; yet it considers itself to provide a generalization of Gerschenkron's and Moore's comparative-historical class-based structuralism. Conversely, Theda Skocpol's *States and Revolutions* (1979) is explicitly based on structuralist premises yet implicitly assumes rational actors who maximize wealth or power and are not driven by more illusive ideas.

2. Sometimes contributors to studies of regime transformation misunderstand their own accomplishments. In this sense, Adam Przeworski in *Democracy and the Market* (1991) wishes to offer a rational-choice-based explanation of regime change yet really falls back onto a historical institutionalism *plus* a process-oriented reconstruction of actors' changing cognitive frames.

3. Rogowski's book (see n. 1) is the other major examples of this more comprehensive comparative approach.

4. Probably closest to the systematic and encompassing comparison of transition processes I am looking for are Terry Karl's articles, especially "Dilemmas of Democratization in Latin America" (*Comparative Politics* [1990]) and (with Philippe Schmitter) "Modes of Transition in Latin America, Southern and Eastern Europe" (*International Social Science Journal*, May 1991). In these and other examples, however, the conceptualization of the main independent variable, modes of regime transition, is not sufficiently refined.

5. I am setting aside here the question whether it is still adequate in light of recent historical research to refer to the rise of Scandinavian social democracy as a "red–green" coalition of workers and farmers against the bourgeoisie. As Swenson's research makes clear, there emerged, if anything, a sectoral coalition in which parts of Labor and business cooperated with agriculture against Labor and business in other sectors. See Peter Swenson's "Bringing Capital Back In" (*World Politics* 43[1991]).

6. To explore the influence of religion, see especially the broad comparative neo-Weberian account in John Hall's *Powers and Liberties* (1985).

7. I have added up rankings of the countries on each of Huntington's four criteria and singled out countries with the four lowest rankings. At the top of democratic promise are Spain, Greece, Portugal, Turkey, Uruguay, Chile, Ecuador, Czechoslovakia, Hungary, Bolivia, Brazil, Peru, Philippines, and South Korea.

8. According to Huntington, even formal democracies in predominantly Confucian countries are characterized by the absence of government turnover (p. 304).

POLITICAL THEORY

Nietzsche Contra Rousseau: A Study of Nietzsche's Moral and Political Thought. By Keith Ansell-Pearson. New York: Cambridge University Press, 1991. 284p. $49.95.

A considerable number of specialized scholarly studies of Nietzsche's political thought have appeared in the last few years, a growth of interest in the subject that clearly reflects the recent revival of Nietzsche's reputation among contemporary scholars. *Nietzsche Contra Rousseau* stands out among these studies for the clarity and soundness of its interpretations, as well as for its greater familiarity with the best of German language scholarship on Nietzsche.

Ansell-Pearson uses Nietzsche's critique of Rousseau as a way of identifying and exploring the central issues in Nietzsche's moral and political thought. This dialogic approach to interpretation has many advantages, especially since Ansell-Pearson gives Rousseau a chance to respond to Nietzsche's criticisms and thereby help us identify Nietzsche's own limitations. It is particularly helpful in identifying the most important features of Nietzsche's understanding of the relationship between resentment and democratic politics. As Ansell-Pearson shows, Rousseau's obsessive and self-dramatizing moral indignation represents for Nietzsche the hidden core of democratic moralism: resentment against the fundamental conditions of human existence.

Ansell-Pearson develops this point in his excellent chapter on Nietzsche's *The Genealogy of Morals*. He presents in this chapter an especially interesting and original account of Nietzsche's understanding of justice and community. This account effectively demolishes the widespread image of Nietzsche as an ethical and political individualist.

Bringing Nietzsche into dialogue with Rousseau also helps Ansell-Pearson identify the sources of Nietzsche's own resentment against the world. Nietzsche, of course, preached the overcoming of resentment and the birth of a "second conscious innocence" toward the cruelties of human existence. But as Ansell-Pearson effectively shows, Nietzsche's own bitterness toward modern individuals and society led him to delude himself in many ways. In particular, Nietzsche's hatred of the modern world led him to believe that modern degeneration and corruption might itself provide the basis for the greatest and most life-enhancing transformation of human culture. Rousseau, by contrast, was far more realistic. He recognized that if society had completely corrupted human character, then there was little to hope for from revolutionary upheavals but grand displays of vice and cruelty.

This Rousseauian response to Nietzsche can be pushed even further. Rousseau had the great advantage over Nietzsche in being quite self-conscious about his own resentments. The contrast between Rousseau's *Reveries* and Nietzsche's *Ecce Homo* as autobiographical testaments is quite instructive and none too flattering to Nietzsche.

Ansell-Pearson's dialogic approach to interpretation does not, however, serve him as well when he begins to assess the value of the arguments explored in the book. Building on the Rousseauian critique of Nietzsche just mentioned, he points us toward a synthesis of Rousseauian and Nietzschean politics, "a Dionysian politics that is both agonistic and democratic" (p. 18). But in order to consider this vision of politics seriously one would have to step further outside of the Nietzsche–Rousseau dialogue than Ansell-Pearson seems willing to do.

Other political thinkers have explored the possibility of a "politics that is both agonistic and democratic." Hannah Arendt, in particular, springs to mind. Her efforts, as well as the internal tensions within such a view of political life, suggest a number of critical questions about the possibility of realizing such a goal.

How, for example, can one maintain an "agonistic" view of politics without developing aristocratic contempt for the mass of the people who are unable or unwilling to participate in political struggles for recognition? Many would argue that Arendt's attempt to synthesize Rousseau and Nietzsche fails precisely because she ends up expressing such contempt. Alternatively, how can one value all members of a democratic community without developing moralistic resentment toward the most creative and powerful individuals, the kind of resentment that Nietzsche argues poisons the minds of Rousseau and all other democrats? Is there a nonmoralistic way of defending the view that every individual deserves a certain basic level of respect and dignity? *Nietzsche Contra Rousseau* would have been a much more interesting book had Ansell-Pearson stepped back a little from his dialogue between Nietzsche and Rousseau to consider these important questions more fully.

Nevertheless, Ansell-Pearson's reconstruction of that dialogue makes for interesting reading. It provides a very stimulating and useful way of identifying some of the most important questions raised by Nietzsche's political thought.

University of Wisconsin, Madison BERNARD YACK

Justice, Law, and Violence. Edited by James B. Brady and Newton Garver. Philadelphia: Temple University Press, 1991. 368p. $44.95.

Violence, Terrorism, and Justice. Edited by R. G. Frey and Christopher W. Morris. New York: Cambridge University Press, 1991. 319p. $54.50 cloth, $17.95 paper.

Almost everyone condemns violence, but there agreement ends. There is a broad consensus among writers on the subject that unlike "force," which may be used for good or ill, "violence" is necessarily objectionable, at least prima facie. Violence has a case to answer. Beyond that narrow strip of common ground, disagreements multiply. Whether violence can ever be justified—and if, so how; whether some forms of violence are worse than others; whether violence is, on the whole, diminished or exacerbated by governments and legal systems; and even what, exactly, "violence" is—these are questions that excite vigorous and sometimes partisan dispute.

This diversity of opinion is well (although no doubt not comprehensively) reflected by these two volumes.

Of the two, Brady and Garver's *Justice, Law, and Violence* casts the wider net, grouping its contributors under three major headings. "Violence" deals with the nature of the beast and attempts to define it; "Law and Legitimation" examines putative justifications of violence, especially as used in the service of law; and "Controversial Instances of Allegedly Legitimate Violence" raises issues concerning punishment, terrorism, and domestic violence. Individual contributions are consistently thoughtful and interesting, although they vary in length and clarity. The presence of writers in the Continental tradition, as well as the Anglo–American, is, predictably, a mixed blessing, providing the usual combination of challenging extremes and murky prose.

Two such writers give some idea of the range of views to be found here. Sergio Cotta argues that violence is "the absence of measure," whose content is given by natural law. Although objective moral standards may sanction the use of force, "violence" consists in force that goes beyond what is permitted by such standards. The perceived increase in violence in the contemporary world (Is there really such an increase?) is due, most importantly, to the abandonment of objective norms in favor of a cult of subjectivity that takes the individual self as the sole author and judge of value. For Bernard Waldenfels, on the other hand, violence is *not* fundamentally opposed to legal and moral norms. On the contrary, violence is inextricably intertwined with law and, indeed, with justification itself. In exactly what sense this is so is not entirely clear. Does he mean that since justifications are always incomplete, remaining open to objection, laws will always require enforcement? But Waldenfels seems to want to go further, to the idea that justifications can be violent in themselves. How this might be is quite mysterious, especially since Waldenfels sensibly rejects Lyotard's absurd conflation of coercion and reasoned argument.

While few writers would join Cotta in his appeal to natural law, most would share his general goal of fixing on a distinction between justifiable force and objectionable violence, in preference to Waldenfels' sanguine thesis that violence is absolutely everywhere. The general view is that violence is often difficult to separate from legitimate force but that some such distinction is surely needed. To this end, attempts are often made to argue that certain kinds of force or violence are intrinsically worse than others. In Brady and Garver's collection, for example, John Ladd distinguishes "collective" from "individual" violence as especially offensive. But the species most often singled out for special attention in this way is terrorism, the focus throughout of Frey and Morris's *Violence, Terrorism, and Justice*. One of their contributors, Loren Lomasky, articulates what is probably the mainstream of public opinion in the West when he argues that the mark of terrorist violence is the rejection of civilized constraints. Terrorism is unjustifiable, it seems, by definition.

The trouble with this sort of approach, as Virginia Held points out (her excellent paper appears in both collections), is that it rules out by definitional fiat a question that still seems to make sense, namely, whether terrorism might be justified in some circumstances. As Alan Ryan shows (in Frey and Morris's collection), a broadly utilitarian perspective implies that terrorism may indeed be justified if the anticipated benefits are great enough in relation to the costs. From such a perspective, there is nothing *intrinsically* evil about terrorism; terrorism is merely an extreme and especially frightening application of force. To resolve whether terrorism might be justified, we need to look beyond the concept itself to its consequences.

Here Ryan makes a methodological point that suggests an important criticism of many contributions to the field: "It is not enough merely to stare at such terms as 'force', 'violence', and 'terror', as if we might divine their essences." Too many writers in both of these collections give the impression that conceptual analysis is sufficient—that once we have isolated the salient features of "terrorism," for example, we will automatically know what judgments to make about it. In fact, identifying the features of terrorism is no more than a start; it remains to be explained why those features should be judged in one way rather than another.

When it comes to judging violence and terrorism, we should recognize that the evaluative perspectives open to us now (given the complexity of our moral culture) have the appearance, at the very least, of plurality. Thus, Frey and Morris note in their introduction that ethical consequentialists are more likely to justify violence than are Kantians and that the latter will be less inclined than contractarians to argue that acts of violence place the perpetrators beyond the moral pale. In the Brady and Garver collection, the essays most sensitive to value pluralism are those by Elizabeth Wolgast and Garver himself, the latter drawing on the work of Isaiah Berlin. (On the relevance to this field of historical and political perspectives, as well as moral–theoretical, see Martin Warner and Roger Crisp's edited collection *Terrorism, Protest, and Power* (1990).) These contributions apart, the question of perspective is little discussed in either of these collections. That is a pity, since it is arguably more fundamental than any other. Still, the two books contain much that is stimulating and original; and both make important contributions to the literature.

University of California, Berkeley GEORGE CROWDER

Tragedy and Denial: The Politics of Difference in Western Political Thought. By Michael Brint. Boulder: Westview, 1991. 181p. $38.95 cloth, $12.95 paper.

Michael Brint's first book is at once highly ambitious and curiously modest. It is ambitious in that it proposes nothing less than a new way of reading the Western tradition of political thought, as a succession of *antitragic narratives*, and discusses a wide range of theorists: Plato, Rousseau, Constant, Hegel, Sartre, Derrida, Arendt, and Rorty. This is a daunting list for a mere 168 pages. The modesty of Brint's book lies not in its length, however, but in its pedagogical purpose: it is clearly designed to offer relative newcomers to the field of political theory a narrative "hook" to facilitate their grasp of a large and unwieldy canon. If, as Brint puts it, our tradition of political thought offers "some of the greatest stories ever told" (p. 1), his own work is similarly narrative in character. Like Voegelin, Arendt, or Strauss, he wants to provide a story about Western political theory that uncovers its driving force. Brint's story, in keeping with the book's pedagogical task, is a good deal simpler than these more familiar narrativizations of the tradition. Despite the simplicity of his story,

however, Brint clearly intends his work as a contribution to current debates about political theory and the "politics of difference." In my view, *Tragedy and Denial* is more effective as pedagogy—as an "edifying story"—than as an argumentative intervention.

Brint's central thesis is straightforward. He argues that human existence is beset by irreconcilable ("tragic") conflicts between the goods and values of competing life spheres. A "tragic" conflict is, he points out, one between not good and evil but good and good (e.g., the private good of the family represented by Antigone vs. the public good of the state represented by Creon). Such conflicts, Brint argues, constitute an "essential fact" of life, one that political theory has had a difficult time acknowledging. Indeed, Brint suggests that we view the Western tradition of political thought as a series of attempts aimed at "banishing tragedy itself from the human domain" (p. 2). The "denial" referred to in the title is Western political theory's denial of the "essential fact" of tragedy, a denial that ignores "the significance of relevant moral differences" (p. 2). Its "desire to eliminate human tragedy" leads Western political theory to efface the reality of difference, and tragic conflict, by a series of antitragic narratives. In Brint's account, political theory is a succession of stories about political community, stories that hold out the possibility of overcoming or sublating the ethical differences that produce tragic conflict. Brint's paradigmatic examples of such antitragic narratives are, unsurprisingly, the political theories of Plato and Rousseau (two theorists who had little use for difference).

The theoretical point of this perspective on the tradition is also clear. In picturing Western political thought as fearful of tragedy and difference, as seeking an escape from the irreducible plurality of values and spheres of life, Brint echoes a pervasive theme of poststructuralist criticism. Like the poststructuralists, he views the tradition as animated by the desire to efface difference in its most fundamental and intractable forms. Also like them, he makes a plea for preserving and respecting difference as a constitutive fact of our humanity and moral–political life (pp. 3, 11, 164). Brint's vision of the "politics of difference," however, is rooted in an "ethical pluralism" that clearly has a lot more in common with liberalism than with poststructuralism or identity politics (p. 6). Indeed, Brint can be seen as representative of that small but growing species, the liberal who has read Derrida (a species that originates with Rorty).

Within this broad framework, Brint offers a set of readings that constitute the heart of his text. These readings are divided between "pure" exemplars of the antitragic impulse (Plato and Rousseau) and an eclectic group of modern theorists who, according to Brint, attempt to resolve the conflict between individual liberty and democratic freedom through three narrative strategies: comedy (Hegel), satire (Sartre and Derrida), and romance (Arendt and Rorty).

The readings, while intelligent and well informed, are uneven. Brint makes a strong and subtle case for the antitragic impulse behind the *Republic* and Rousseau's political theory; but few readers will be surprised to learn that Plato (or, as Brint would have it, Socrates) and Rousseau were hostile to ethical pluralism. Brint raises a number of intriguing issues (e.g., the narrative structure of the *Republic*, the Socratic inversion of Homeric morality, how irony makes Plato's text a self-deconstructing artifact, the question of Rousseau's subjectivism and theoretical narcissism); but these have all been treated in greater depth by Voegelin, Arendt, and Starobinski. While the treatments here are not exactly original, they are nevertheless concise and thought-provoking; and they are fairly free of plotted "plot summary." The same cannot be said, however, of the readings in part 2, "The Politics of Tragedy." Here, one theorist after another steps on stage just long enough to be typecast (as comedic, satiric, or romantic) and sent packing. Brint's point is to reveal the more insidious forms taken by the antitragic impulse as the threshold of modernity and postmodernity is crossed. This threshold is marked, as Lyotard has claimed, by "incredulity toward metanarratives." While Brint does not mention Lyotard, his point is similar: Hegel's "comedy" of reconciliation was political theory's last "big" story, a story succeeded by Sartre's and Derrida's parodies of metaphysics and, finally, by the reemergence of political theory as "romance,"—as a storytelling that knows itself to be such (Arendt, Rorty). The problem is that the readings in this section, designed to support Brint's narrative typology, are necessarily in broad strokes and not very illuminating. But the reader can hardly expect an original interpretation of a major theorist in 6–10 pages.

Brint does have a point in rushing so many theorists on and off his stage. He wants to show how political theory, even when it takes up difference as its explicit theme and becomes self-consciously narrative in mode, nevertheless continues to evade the full force of tragic conflict. As narrative genres, comedy, satire, and romance evince the same "failure of political nerve" (p. 2), the same antitragic impulse, found in Plato and Rousseau. Brint's solution is to uphold tragedy as a normative model for political theory and to suggest (as the title of his last chapter phrases it) that we "put the 'e' back into *differance*." By this he means that we should break with dialectical or deconstructive treatments of otherness, treatments that in his view thematize difference only to sublate it or reify it into an "ineffable other." In other words, we should avoid the Hegelian temptation of reading difference back into sameness while at the same time resisting the Derridean temptation of simply inverting the "metaphysical" valorization of identity and difference.

I do not find Brint's alternative particularly persuasive. The moral–political thrust of his anti-Hegelian, antideconstructive stance is a plea for a renewed appreciation of the inner articulation of our pluralist polity, an articulation born of different "life spheres," of different "vocabularies, traditions, activities, and practices" (p. 165). Like Rorty and Walzer, Brint wants us to respect these differences and to resist seeking some standard of the just or the good that pretends to render the incommensurable commensurable. This is fine, as far as it goes; but it is (Brint's protests notwithstanding) simply good liberalism. There is no dishonor here: one of the virtues of Brint's book is that it clarifies the pluralist ethical vision of liberalism in a compelling way. He does this, as did Weber, by stressing the pathos, the tragic quality, of conflicts between rival values and goods. Despite the poststructuralist trappings of Brint's book, the result is not so much a *critique* of liberalism as a *restatement*.

Amherst College DANA VILLA

Action into Nature: An Essay on the Meaning of Technology. By Barry Cooper. Notre Dame: University of Notre Dame Press, 1991. 291p. $34.95.

Critical Theory of Technology. By Andrew Feenberg. New York: Oxford University Press, 1991. 235p. $35.00 cloth, $14.95 paper.

Barry Cooper and Andrew Feenberg are both dismayed by technology's assault on nature and human nature. Feenberg allows for minuscule openings in the deadly machinery of modernity while Cooper leaves us with a dispassionate so-it-goes ghetto journal, recording in depth that death is in season, implying (as Andy Warhol did) that one's talent can no longer be employed for anything but embroidering and inflating the products and fetishes of our era.

The world, as Cooper knows it, is in shambles. Its population has been dehumanized by Galilean mathematization, social science positivism, the triumph of light over the mysteries of darkness, and music that is rocking and rolling history to its end. Communists, liberals, fascists are all technicists using means which, in the purposeless whirl of modernity, are themselves ends. "All things," he echoes Marx, "are in movement . . . all is process and change" (p. 102). Global decomposition which Feenberg traces to the reification of ends, Cooper (*pace* Norman Mailer) attributes to the cancer of technicism. And yet Cooper's erudite trouble shooting and whistle blowing—his unrelieved complaints—are not a prophet's jeremiad—for however frequently he recalls Strauss and Voegelin, Cooper never interrupts himself to project a transhistorical order that might give credence to an indictment. He relates; he doesn't blame.

His narrative is a losely concatenated flow of declarations and pronouncements: "Today reality is sufficiently grotesque to make satire impossible: after all, how could it be exaggerated?" (p. 112). "Modern technological societies are not threatened by outbursts of revolutionary compassion" (p. 122). "Now that prosperity has been attained by nearly the whole population, any serious notion of the purpose of leisure has evaporated" (p. 250). And so on.

Cooper makes no claim that alterations are feasible. In the spirit of Heidegger, he envisages no basis for restoring a pre-modernist consciousness. As modest as one of Beckett's characters, he nonetheless goes on—goes on writing and quoting and footnoting while he knows that the ground for going on has eroded. Tenacious, deadserious, and gravely scholarly, it is a strangely reassuring performance: a case against suicide.

Feenberg's case, on the other hand, is explicitly one for healing the body politic, for recovering the marginalized abiguities of Marx so as to fulfill the promise of socialism. Implictly it is one for bypassing postmodernists whose gay science leads them to embellish industrial products, celebrate fast food, universalize irony, and deconstruct metanarratives all because they assume that it's far too late to reverse an increasingly autonomous technicism.

Feenberg does not feel, as Heidegger finally did, that "only a god can save us now," or that only a state apparatus can impose a new order, or that a simplified lifestyle will heal us, or that a consciousness-raising catastrophy will inspire an immortal phoenix to rise out of the ashes. It remains possible to weave repressed human needs into the stainless steel fabric of the dominant technology. Designed by variable cultures which harbor various desires, technology is neither autonomous nor value-neutral.

Only a metaphysical pathos (in Alvin Gouldner's phrase) of the elites of contemporary society stands in the way of transforming hierarchies of power, one-dimensional work by de-skilled workers, and meaningless consumption. The degradation of labor, liberal education, and the environment inheres not in technology but in the belief system that currently defines technological development.

Yet change can be generated by social movements guided not by Marx's pathetic determinism but by his open-ended vision of a process which can itself arouse and incorporate the latent "irrational" and "inefficient" impulses within individuals and nature. Thus the locus of technical control can be shifted—as in Feenberg's view it might have been in the 1960s—from a control-oriented technicism to a multitude of local social institutions and workplaces.

When Feenberg proceeds to provide examples of the impact of countercultural movements on the capitalist-military-corporate-media complex, he is poignantly evasive. It is disheartening to realize that he keeps himself from sharing his knowledge of the capacity of the technological imperium either to repair (or to embroider) the cracks in the hegemony of capitalism. Sadly, he decides to remain silent about how effectively *Rambo* incorporated the traits of the counterculture in the ideology and policies of the Reagan Era. Feenberg has reason to shy from discussing the limited effects of well-intentioned movements to stop environmental degradation, to free the world (and one's neighbors) from the threat of weapons, or, as in the 1960s, to transform society, its politics, its educational practices, its racism, its compulsions, its very epistemology. Contrary to Cooper, a sad necessity restrains Feenberg.

The reader who spends a month in Feenberg's and Cooper's company may finally come to realize that their books, and perhaps all artifacts patiently lived with, finally complement one another.

University of Hawaii HENRY S. KARIEL

David Hume and the Problem of Reason: Recovering the Human Sciences. By John W. Danford. New Haven: Yale University Press, 1990. 228p. $25.00.

With the possible exception of Kant and Mill, no modern political philosopher has had as much affect on all facets of current political theory as David Hume. Before I read this book, I was sure that Hume's story in relation to political science had been told; but now I am no longer certain.

The purpose of the book is to establish a role for reason, truth and the use of universal principle in the interpretation and reading of Hume's political thought. Danford asserts that we are now enmeshed in a "crisis of Western rationalism" but that Hume's political philosophy can be utilized to reestablish "the status of reason" (pp. 7–8) in political life. It is obvious to anyone who reads the literature in our field that there is a general skepticism about the role of reason and truth in our studies. Rawls, for example, doubts that metaphysics or truth have any place in "political" concepts of justice, while in other disciplines deconstructionism and radical relativism have built upon the ruins of positivism and

reinforced the idea that our task as theorists is indeed a limited one.

This book contends that a reading of Hume that moves away from dependence on the *Treatise* and toward reliance on *The History of England* and the *Dialogues Concerning Natural Religion* will produce a definition of reason and truth that is connected to experience and "common life" (p. 8). The author claims that "common life" acts as a basis of a political theory combining history, religion, and ethics into a bridge that connects the search for reason and truth with an "understanding of what is good for mankind" (p. 193). In this way, the application of reason to the search for truth departs from its dependence on the methods of natural science and moves to the specific framework of political life that defines its proper context. Here, Hume's approach is separated from those who established the use of scientific method in political theory (e.g., Hobbes) and becomes a counterargument to those who have since maintained that "relativism" and "interpretation" must replace "truth" in the theoretical analysis of politics (p. 19).

I had assumed that Hume's political theory rested in the ideas of passion motivating individual action, limited self-interest, convention, and the relative utility of justice and government. This interpretation would make Hume a champion of those who proclaim the death of reason and universal truth in political thought. If Danford is correct, however, the truth of Hume's political theory lies in reason, general principles, common life experience, religion and the historical process of social evolution. Seeking the core of Hume's politics outside the *Treatise*, Danford integrates "common life" with the evolution of individual liberty and the "general principles" of "usefulness and agreeableness" that together provide a framework within which reason and truth in political analysis are reestablished within a stable environment for human social interaction (p. 99).

The argument is elegant and concise (perhaps too concise). However, I am not sure it covers all the bases. Although I am willing to believe that the *Treatise* can be replaced as the core text of Hume's politics, I would maintain that it contains too many supporting beams of what is traditionally thought to underlie Hume's "relativism" to be dismissed without proper argument. I would propose that it is critical for the author to provide a complete transitional argument between old and new interpretations that would overcome the contention that Hume's politics is all about convention and cultural relativism. Danford needs to recast the central role of convention as a competitor for contract, property as a basis of justice/government and the connection between utility and preference that has come to play such a major role in current Hume scholarship (e.g., in both collective action theory and "preference" utilitarianism). Instead, I remain confused by such statements as that Hume rejects convention-based moral theory but maintains an "unblinking acceptance of the actual desires and preferences of mankind" (pp. 157, 160).

This book is one of the most interesting and unique renderings of Hume's epistemology and political and moral thought that I have ever come across. It is a must read for anyone interested in Hume's philosophy and its relation to political science or to anyone who is troubled by the relativism and positivism that provides the framework for most of what passes as social science these days. It is a perfect text for a seminar on Hume; and although I believe that more argument should have been provided as to how this "new" reading of Hume's politics can replace the "traditional approach," the uniqueness and creativity of this book, as well as its pertinence to current theoretical debate, is enough for me to recommend that anyone interested in all the dimensions of Hume's political thought ought to have this volume in their library.

Trinity College, Hartford JOHN MARTIN GILLROY

The Liberal Self: John Stuart Mill's Moral and Political Philosophy. By Wendy Donner. Ithaca: Cornell University Press, 1992. 229p. $35.00 cloth, $12.95 paper.

The Liberal Self proposes to "resolve some traditional puzzles" raised in the secondary literature on John Stuart Mill's thought (p. 2). Most of the book is devoted to a discussion of Mill's moral philosophy. However, since Wendy Donner maintains that "Mill's utilitarian philosophy unifies his ethics and his liberal political philosophy" (p. 1), *The Liberal Self* should be of considerable interest to those concerned with Mill's political thought. This interest may be tempered by Donner's interpretation of Mill's view of political theory.

According to Donner, Mill's system of ethics is grounded upon the preferences of knowledgeable agents, individuals who have developed their intellectual and moral capacities (p. 5). At the same time, she argues, "Mill's most fundamental commitment . . . is the promotion of human self-development and the happiness involved in the development and exercise of our higher human faculties" (p. 3). On the face of it, there appears to be an irreconcilable tension between the absoluteness of a universal right to self-development and the preferences of a privileged minority who have developed their capacities as an axis from which to evaluate competing moral claims. Nevertheless, Donner believes that Mill's "sophisticated form of hedonism," in which qualitative judgments supply the definition and measurement of value, can be defended as an objective and logically consistent expression of utilitarian moral theory (pp. 4–5, 9, 55).

Few readers are likely to be convinced by Donner's defense of qualitative hedonism. In this and in other instances throughout *The Liberal Self*, the problems she draws from the secondary literature on Mill are more profound than her responses to them. Still, she does have some valuable things to say regarding the social basis of rights in Mill's thought; and she quite rightly stresses the importance of self-development to Mill's moral and political theory. But to affirm truly that Mill is equally concerned with sociality as he is with individuality or that he defends a positive, as well as a negative, conception of liberty (pp. 148, 162) hardly constitutes a resolution to the puzzles various critics have discovered in Mill's thought. Indeed, to admit that Mill does not tell us how to employ the principle of utility to resolve a conflict of vital interests (as Donner does, p. 187), leaves us inhabiting a world characterized by social conflict where appeals to such vacuous propositions are not likely to provide any guidelines for practical action.

However, whatever the moral dilemmas confronted by individuals qua individuals, Mill does offer a specific political example of what would count as a successful application of utilitarian philosophy. "The test of Mill's

commitment" to the goal of human self-development, Donner notes, "is his stance on opportunities of development of the working class of nineteenth-century Britain" (p. 198). To appreciate the extent of Mill's "commitment" one should include (Donner surprisingly does not) the self-development of women. Still, if we consider this test as applied to the working class, both the tensions internal to Mill's thought and the difficulties with respect to Donner's interpretation of it will become apparent.

Assuming that Mill's political theory is simply the logical extension and practical application of his moral philosophy, Donner maintains that Mill's political thought expresses the preferences of knowledgeable agents (pp. 131, 202). To grasp the implausibility of such a view, one need only consider Mill's commitment to democracy and universal suffrage in the context of nineteenth-century British politics. Such a perspective was not espoused by Mill's intellectual contemporaries, the vast majority of whom sided with Macaulay in opposing democracy. In other words, it was the working class, not a developed or knowledgeable elite, who advocated political and economic democracy—the core of Mill's political thought. Moreover, Mill did not believe that political theory could be identified with the preferences of knowledgeable agents. Rather, following Alexis de Tocqueville, he maintained that observable empirical sociological tendencies at work in society provided the foundation for any political theory. The political theorist's task, Mill argued, was to ascertain the good and bad effects of these tendencies and to assist the extension of the former and inhibit the development of the latter. Not only was it possible for any perceptive observer, including members of the working class, to grasp the significance of the empirical tendencies favoring democracy or cooperative socialism, for example, but it was also possible for highly educated intellectuals to pay insufficient attention to such sociological evidence and thus to hold a seriously deficient political theory.

Donner maintains that Mill's elitism makes him "overly protective of the interests of the middle class at the expense of working class advancement." Thus, she asserts, "Mill assumes that the already competent members of the middle class who are to help others become competent are able to put aside their class interests and make social choices grounded on the interests of all" (p. 202). This is patently absurd, as any reader of *Considerations on Representative Government* knows. In that work, Mill argues for equal representation in Parliament of middle- and working-class interests precisely, he insists, because neither class is capable of "putting aside" its class interests. A tiny number of individuals from *both* classes, Mill suggests, will, with respect to a particular legislative proposal, be able to see beyond their class interests, thereby securing its passage (the majority of members on both sides voting in accordance with their class interests).

According to Donner, Mill is concerned only with the dangers posed by the working class's coming to power but not with the abuse of political power by the middle class (p. 203). But based on Mill's correspondence and his published writings, not only is it Mill's view that class-interested abuse of power is a danger in all forms of government (not merely a defect peculiar to democracy or to the empowerment of the working class), he also has some especially harsh criticisms to make of the British middle class, which, he argues, has already demonstrated its unfitness to rule. Far from being concerned with protecting a particular class's interest, the problem for Mill was to find a way to put an end to class conflict.

It is ironic that Donner accuses Mill of having little sociological knowledge of the workings of his society (p. 207). On the contrary, it is Donner who presumes that social, political, and historical context are useful merely as background information. The fact is that the philosophical problems as they appeared to Mill cannot be formulated correctly in the absence of such contextual knowledge.

University of California, Los Angeles RICHARD ASHCRAFT

Regarding Politics: Essays on Political Theory, Stability, and Change. By Harry Eckstein. Berkeley: University of California Press, 1992. 404p. $45.00 cloth, $17.00 paper.

This collection of Harry Eckstein's monographs and articles, reprinted from journals and books which have appeared over the last quarter century, is almost, but not quite, representative of the rich and varied body of his work. It includes three monographic essays which ensure Eckstein's membership in the small group of scholars who have made creative contributions to the definition and methodology of political science. Had he not reprinted them here, the obsolescence of the symposia and handbook in which they originally appeared, might have imposed prohibitive "search" costs for a contemporary scholarly generation indifferent to disciplinary history. His essay "Political Science and Public Policy" is still one of the best interpretations of Max Weber on these themes. At a time when the termination of the Cold War enables us to reconsider the politicization of our scholarship, Harry Eckstein's 1966 paper analysing Weber's writings on these topics and political activities during the late Wilhelminian Empire and the early Weimar Republic belongs at the top of the reading lists of our syllabi for introductions to political science. His "Historical Perspective on Comparative Politics," introducing the anthology that he and David Apter published in 1963, still has no rival in scope and erudition. His "Case Study and Theory in Political Science" is one of the most original methodological contributions of our time, bringing case studies into the world of scientific rigor and theory and turning the ideographic/nomothetic dichotomy into a continuum with five distinguishable degrees of theoretic–scientific sophistication. Drawing on work of Sidney Verba but pressing the argument to its full implications, Eckstein spells out the properties of the "ideographic–configurative" case study, which makes no claims to theory, at one extreme and the "crucial-theory-infirming," or "theory-confirming," extreme at the other. The three types in between—"the disciplined–configurative," the "heuristic," and the "plausibility probe"—take increasingly firm steps in the direction of self-conscious hypothesis testing and theory building.

Eckstein is a strong advocate of the crucial case study as the way to theory. While he does not denigrate statistically based theory, he makes a convincing case for the special value of clinical depth in avoiding the loss of important information. It has been an only partially fulfilled aspiration of Eckstein's to conduct a crucial case study of his "congruence theory" of political stability (his study of Norway); and he has not as yet done the

study that might settle the rivalry between the political culture and rational choice approaches to explanation. Eckstein takes political science very seriously indeed. His model for crucial case studies in political science is the series of natural observations that have confirmed (or failed to falsify) Einsteinian relativity. Michel's theory of political oligarchy based on the study of the organization of the Social Democratic party of Germany is Eckstein's strongest example of a crucial case study in political science. While a program of accumulating a large collection of crucial case studies in political science may turn out to be visionary, Eckstein's exhortation to carry out case studies with theoretical insight and discipline and his systematic elaboration of the various ways this can be done is wholly constructive and admirable.

Creative scholars are risk-takers; they have the courage to propose new approaches and theories even though they may fail. In the early 1970s Eckstein made the bold proposal that political scientists redefine the focus of their discipline from the state and government to authority relations in general (family, church, school, economic enterprises, associations, etc., as well as government and the state). This initiative had its origin in Eckstein's congruence theory of political stability, that is, the hypothesis that a political system with organizational characteristics similar to the main nongovernmental structures would be stable, whatever that organizational structure might be. His theory of stable democracy—now more than three decades old and republished here as part 3, "Political Stability"—added to the organizational congruence requirement for generic stability the "balanced disparity" requirement for governmental–political institutions. In other words, democratic stability would require not only organizational congruence but limits on democracy. Such are accomodativeness to authority; deferential attitudes, as well as egalitarian and achievement values; and consensualism, as well as conflict. Having formulated this theory in the late 1950s on the basis of an informal comparison of England, France, and Germany, he carried out what approximates a "crucial case study" of Norway in the mid-1960s. His essay on case studies came after his theory of democracy had been formulated and tested in the case of Norway—a country about which Eckstein at the outset knew little save that it was a good democratic performer on all counts and stable in all aspects. His book, *Division and Cohesion in Democracy* showed that that country had both organizational congruence and balanced disparities.

Eckstein's theory of political stability was the high point of a research program that had begun most auspiciously in his early work on the British Medical Association, on interest groups, and on empirical pluralist theory. A member of the scholarly generation that lived through (in Eckstein's case most personally) the traumatic years of the fall of the Weimar Republic, the crisis and fall of the Third Republic, the rise of fascism and nazism, and World War II, he shared the preoccupation with the conditions and characteristics of stable democracy. Of all of that generation, his work on this theme was the most original and the most seriously theoretical.

Having gained such leverage on so important a problem as democratic stability by emphasizing general social organizational and cultural characteristics, Eckstein took what seemed to him the logical next step: he proposed that political science as a discipline change its focus from government and the state to authority relations, broadly speaking. If nongovernmental structure and culture were of such importance, then why not broaden the field and thereby the body of explanatory knowledge? Drawing the consequences of this argument, he, along with his colleague Ted Robert Gurr, embarked on a research program that culminated in a book entitled *Patterns of Authority*, which invited the political science profession to adopt this inclusive paradigm in the place of those then dominating the field.

In his introduction to *Regarding Politics*, Eckstein refers to his effort to broaden the subject matter of political science as the "greatest failure of his career." Let me console him with the thought that our colleagues are much more indulgent of us than this passage in his introduction suggests. The response to his appeal, judging by the productivity of Eckstein's students in nongovernmental authority studies, was quite forthcoming and was enriching of the literature on political socialization and political culture. There are, indeed, few restrictions on the kinds of authority patterns that political scientists are encouraged to investigate. If there was any fault in Eckstein's appeal to broaden the focus of political science, it was that the walls were already largely down. It may be that the packaging of his book on authority diverted attention from its rich and suggestive content and that future generations of scholars will draw on it for research designs and for hypotheses.

Parts 4 and 5 include articles and papers on political development, political culture, political violence, and, most intriguing as indicative of future Eckstein work, "Civic Inclusion and Its Discontents." Here he is pursuing themes first developed in Tocqueville's *Democracy in America*, particularly the second volume forecasting the spread of equality and all the promise and problematics that this trend implied. As one would expect from his prior work, Eckstein's notion of the scope of civic inclusion means much more than the spread of the suffrage and formal access to officeholding. He is concerned with family, peer group, neighborhood, school, and workplace participation and acculturation, since this creates the culture of the poor and shapes and limits the process of democratization. This essay promises another important contribution from the Eckstein atelier.

Stanford University GABRIEL A. ALMOND

Interpersonal Comparisons of Well-Being. Edited by Jon Elster and John E. Roemer. New York: Cambridge University Press, 1991. 400p. $49.50.

In view of the contemporary opinion regarding the intractability of the problem of interpersonal comparisons of well-being, it may seem curious that the classical utilitarians (Bentham, the younger Mill, and Sidgwick) were scarce troubled by this issue. To be sure, they were aware of the considerable practical problems of measurement; of the propensity of individuals to make mistakes in hedonic judgments even with regard to themselves (let alone others); of the want of reliable scientific understanding of the causes of pleasures and pains, especially in regard to quantity; of the diversity of individual satisfactions and displeasures; and of the great uncertainty about effects, under which decisions about what would be conducive to the greatest happiness would have to be taken. Nonetheless, all three were in agreement that the thing to be measured was an objective

concept amenable to scientific enquiry (Mill's idea about higher and lower pleasures notwithstanding). In Bentham's and Sidgwick's accounts of this, it almost seems as if measuring pleasures and pains, and thus well-being, was akin to measuring blood pressure or body temperature. (Sidgwick talks of "psychophysics," i.e., physiological psychology, in this connection; see *Methods of Ethics* 2.6.) Mill's account of the test for quality of pleasure in chapter 2 of *Utilitarianism* is all too brief; but it seems clear from what he does say about the matter that the "verdict of the only competent judges" was meant to be an objective one, good for all. If so, then it is understandable why no one among them might have seen interpersonal comparisons of such a measure of well-being as being in the least problematic.

On the other hand, once we follow the path of the economists from the classical hedonic concept or concepts to the modern concept of utility as subjective preference, to be revealed by the choices individuals are disposed to make, then the problem of interpersonal comparisons does, indeed, loom large. In particular, part of the skepticism about John Harsanyi's reconstruction of utilitarianism derives from doubts on this score.

Of the 11 papers in this volume, 6 are of a technical nature, from which the reader is not likely to get much without at least some acquaintence with the mathematics used in social choice theory or welfare economics. The other five, however, should be more generally accessible.

To begin with the skepticism about Harsanyi and one of the technical essays, Amartya Sen some years ago raised the question of whether the theorems at the core of Harsanyi's defense of his preference utilitarianism were substantive proof of his position or were (as Sen argued) merely theorems about the representability of social preference that are consistent with a nonutilitarian interpretation. In short, what has Harsanyi really proved? John Weymark, in his contribution to the volume, gives a meticulous analysis of this question, a model of the clear exposition of a technical matter. The verdict is that Sen was right.

Both Tim Scanlon and James Griffin, in their chapters, argue that we must move in the direction of a more "objective" measure of well-being if we are to cope with the interpersonal comparison (though for somewhat different reasons). Neither advocates a return to Bentham or Sidgwick, however. Scanlon argues for a more indirect measure along the lines of Rawlsian social primary goods, and Griffin would have us move in the direction of Mill. Both argue that the proper choice of concept of well-being is a substanive issue of ethical theory, an insistence found in several other contributions, as well (e.g., Weymark's and Hammond's). Scanlon's analysis provides a critique of Harsanyi's assumptions about the interpretation of individual preference that is complementary to the discussion given by Weymark.

Daniel Kahneman and Carol Varey, in their essay, present interesting evidence from the psychological literature to the effect that individual preferences and hedonic experience (their term) may diverge significantly, especially where the former involves anticipation of future experience or the latter, adaptation or context effects of various sorts. The preference model, in practice, would diverge significantly from one along the lines of Bentham and Sidgwick; thus, this is not an idle distinction. On the other hand, the evidence about adaptation or context effects suggests that the Benthamite project is rather more complicated than the opening remarks about Bentham or Sidgwick would suggest. It is unfortunate, however, that the evidence they present does not appear to give us much of a handle on the issues raised by Griffin and Scanlon.

But supposing that we follow Griffin, in the direction of Mill, is this approach technically viable? Suppose our "only competent judges" are only "local" experts (but that any two agree on matters of mutual experience). Ortunõ-Ortin and Roemer show that we can piece together these local judgments to get a global interpersonal ordering. This is the good news. The bad news is that if we allow everyone to be a "global expert," do not require agreement, and impose analogues of the Arrow conditions, then we get an analogue of the Arrow result in the form of a "judgment dictatorship"—as Aanund Hylland shows in his chapter. Weaken the universal domain condition to require "antipaternalism," and the conditions become consistent with a "positional dictatorship" (e.g., the maximin rule). But these results ought to be expected, I think, given that Hylland restricts attention to ordinal preferences and ordinal interpersonal comparisons.

Peter Hammond's chapter gives a broad survey and appraisal (mostly negative) of the ideas for interpersonal comparisons that have been tried in welfare economics and includes an extensive (perhaps exhaustive) bibliography. His suggestion at the end that a successful model of interpersonal comparisons will involve judgments about what sorts of people are desirable seems alarming at first blush but may be somewhat less troubling (though still provocative) once one realizes that we already are comfortable with some such judgments (the undesirability of sociopaths, to take an extreme case). The technicalities are at a minimum here, and Hammond's chapter should be accessible to a wider audience than just those skilled in mathematical welfare economics. The same, alas, cannot be said of the chapter by Blackorby and Donaldson, "Adult Equivalence Scales."

The other chapter that I found instructive is that by Jon Elster about the ingredients that seem to go into small-scale (*local* is Elster's term) policy decisions involving interpersonal comparisons of well-being. Elster has space for detail about only a small subset of the kinds of policy decision he initially presents (from allocation of scare medical resources like kidney transplants to allocation of tasks within households), but there is enough to show that "it is a messy business"; and even when comparisons of well-being are the primary feature of a policy decision, significantly different aggregation procedures may implicitly be used, depending on the nature of the decision. Elster has useful things to offer by way of bringing some order into this picture, and his chapter is a welcome addition to a collection primarily of a more theoretical character.

In summary, the primary audience for this book will be those doing normative political theory, political philosophy, political economy, or the like, with some technical background in social choice theory or welfare economics and a formal bent. They will, I think, find much that is of interest here. Some of the material should be accessible, and of interest, to philosophers, social scientists, and policy analysts for whose work the issue of interpersonal comparisons of well-being is pertinent. Finally, a number of interesting problems for

further research both formal and empirical are to be found herein.

University of Texas, Austin PAUL LYON

Democracy and Deliberation: New Directions for Democratic Reform. By James S. Fishkin. New Haven: Yale University Press, 1992. 133p. $17.95.

This small book ranges widely over topics of interest to students of democratic political systems—from the appropriateness of democracy for the governing of large nation–states to problems confronting peoples seeking to move from authoritarian to democratic rule. Its centerpieces, however, are a critique of recent democratic reform movements and a proposal for remedying some shortcomings of the present procedures for nominating presidential candidates in the United States.

Fishkin argues that a fully realized democratic system (to qualify as such) must weigh its citizens' preferences equally in political decisions, insulate those preferences from influences that threaten their integrity, give interests with significant followings effective opportunities to be heard, reject policies that destroy anyone's essential human interests when policies without that result could be chosen, and permit citizens equal and sufficient opportunity to discover and test their preferences on issues to be decided. The actual institutions and procedures of democratic states satisfy these conditions (which Fishkin subsumes under the headings of political equality, nontyranny, and deliberation) very imperfectly; and arrangements that further the realization of one may hinder realization of another. The reforms in the late 1960s of the U.S. system for nominating presidential candidates are a striking illustration of this kind of trade-off. They gave many more people a direct say in these important choices but made the process of deliberation about them a largely empty one.

Fishkin proposes that we choose a sizable body of citizens of voting age by random sampling methods; bring these citizens together for several days and before any states have held caucuses or primaries; and assign Democrats and Republicans to separate "national caucuses," letting independents choose which of these they will join. Then, Fishkin suggests, we should bring presidential candidates before the appropriate caucus, singly and in debates, and let its members question them about themselves and their positions on issues. After the members of each caucus have debated the issues with candidates and each other, we should poll them to learn their choices of candidates and opinions on the issues and (ideally) let them choose a limited number of delegates-at-large. Broadcast some part of these proceedings on national television.

This ingenious proposal has already drawn much attention; indeed, a version of it came near to realization this year. Its intent is to offer "a means of launching candidacies and issues in a competition that is more representative of the entire country than either Iowa or New Hampshire and more deliberative than our conventional campaign process" (p. 4). On some reasonable assumptions, the proposal is a very appealing one. These assumptions are that (1) the major parties will continue to choose most of the delegates to their national conventions in sequential primaries; (2) the sample of voters drawn will in fact be representative of eligible voters nationally; (3) members of the two national caucuses will be able to inform themselves at least as well as those voters who participate in the Iowa caucuses and the New Hampshire primaries now do; and (4) news agencies, contributors, volunteers, and primary voters will see the national caucuses as important tests of the viability of candidacies. Grant these assumptions, and you have gone far toward agreeing that Fishkin is proposing—if not a better way to run the railroad—a better way, at least, to start the train down the track.

Fishkin's agenda for thought and action goes considerably beyond this proposal for national caucuses. These are, in fact, only an example of what he calls a "deliberative opinion poll," that is, a poll that "models what the electorate *would* think if, hypothetically, it could be immersed in intensive deliberative processes" (p. 81). Such polls, though their high costs would necessarily limit possible applications, might play a useful role even in a system for nominating presidential candidates like that abandoned after 1968 or in the consideration of issues submitted in referenda. In any case, deliberative opinion polls are in turn but one example of what Fishkin truly has in his sights, namely, reforms of the institutional processes of nation–states that will realize both political equality and deliberation.

The strength of this book is its capacity for provoking thought about the institutions of representative democracy and ways to make them serve basic democratic values. It also has some weaknesses. It is sketchy in its presentation of arguments, and some claims are exaggerated or otherwise questionable. For example, even critics of the present system for nominating presidential candidates are likely to balk at the flat characterization of it as "momentum without learning; choice without deliberation" (p. 6). And the claim that the U.S. Congress has moved toward majoritarian control is dubious, at least, however much its members now cultivate the electoral connection. Theorists will find little new theory of a fundamental sort, students of institutions, no new evidence about them. To make much of this last observation, however, would be to miss the point of the book. The author intends not to produce new evidence or to build new theory but to invite us to think about how to bring theory and knowledge to bear on the design of better political institutions. The invitation is both an engaging and challenging one, and he sets a good example.

Princeton University STANLEY KELLEY, JR.

Liberal Purposes: Goods, Virtues, and Diversity in the Liberal State. By William A. Galston. New York: Cambridge University Press, 1991. 343p. $49.50 cloth, $16.95 paper.

Is it possible to do political philosophy without any conception of what is good or valuable in human life? Some have tried: John Rawls and others have explored the possibility of grounding principles of justice and right on procedural ideas like equality, reciprocity, and deliberation. Their critics delight in showing that such an enterprise is bound to fail. Reciprocity and equal respect, they say, remain empty as ideals absent some account of what we value in each others' agency; and deliberative norms cannot yield principles for social life

without some account of the substantive point of our conversations.

Those who retain any shred of faith in the proceduralist project should read William Galston's new book, for it is both a clear and up-to-date attack on the proceduralist experiment and a new and sympathetic outline of the value commitments that are actually presupposed by the core positions of liberal political morality.

The critical part of the book is mainly an attack on liberal neutrality, that is, on the principle that both political decisions and normative theories of political decision making should be, in Ronald Dworkin's words, "independent of any particular conception of the good life, or of what gives value to life." Galston considers four justifications for neutrality: skepticism about value, free individual choice, peaceful accommodation among a diversity of groups in society, and equal respect in political argument. The first is easily dismissed: a general skepticism about value can hardly justify a *principle* of neutrality. The fourth, equal respect, elaborated in recent discussions by Charles Larmore and Thomas Nagel, is too constraining for Galston's taste. If we are prohibited from raising anything that offends anyone's cherished beliefs, then most of what we understand as public argument will be ruled out. The middle two are less easy to dispose of but also less obviously proceduralist: each appeals to a conception of the good—either to values concerning autonomy or to the values involved in giving priority to life and peace. So neither can be used as a reason for excluding considerations of the good from politics.

There is nothing strikingly new about this discussion. Galston's account has both the advantage and the disadvantage of being very much up to date. Because it directly addresses the latest articles in the debate about liberalism, it has the character of an informed engagement in the immediate fray, rather than of a more detached discussion of the philosophical issues involved. Neutrality actually raises some very deep questions about why we organize ourselves into states; yet most of the modern discussion takes the existence of the modern state for granted. Thus, Galston does not discuss the "How dare they!" defense of neutrality—the argument that calls in question the very idea that it is for coercive organizations to take stands on matters of ethics, value, and religion.

The second half of the book, in which the author sketches out the value premises of an acceptable theory, is likely to be more important to readers. The crucial chapters are entitled "Liberal Goods" and "Liberal Virtues." We are told that the liberal conception of the good comprises the following values: life, the development of basic capacities, the fulfillment of interests and purposes, freedom, rationality, community, and subjective satisfaction. The virtues liberals should be inculcating are things like courage, loyalty, independence, tolerance, self-restraint, and an ability to listen to another's point of view.

I wish Galston had devoted more space to the detailed discussion of all this. It's not that the lists are wrongheaded. On the contrary, his account is plausible and largely incontestable. However, the philosopher's job is not to pander to our intuitions but to take what sounds plausible and make it seem contestable for sake of bringing out its deeper significance.

Most of the discussion in the book is programmatic. It is about the very idea of liberal goods and virtues, rather than the particular conception itself. It is as though Galston were engaged in the debate about neutrality throughout the book and not just in the first half—as though the goods and virtues were listed in the later chapters purely for the sake of illustrating the more abstract antiproceduralist argument. Surely, if liberalism involves commitment to substantive goods, then liberal political philosophy should consist in the deep and systematic exploration of those goods. In *Liberal Purposes* there are precisely 16 lines devoted to a discussion of the good of life. But whether life (and the peace that secures it) is the preeminent good is precisely the issue on which the whole tradition of liberal toleration turns. There is less than half a page on the satisfaction of desire. But whether satisfaction is worth pursuing for its own sake (even as one good among others) is a crucial issue between liberals and their opponents. Again, there are just 9 lines on the virtue of loyalty. Should one be devoted primarily to the liberal institutions of one's own country, or should one be devoted to their realization everywhere? Are liberal virtues too universalistic to sit easily as civic virtues? These questions are not answered.

We await a discussion that explicitly shows what it is to pursue intimations about the good life in a substantive way if that is what we should be doing. Meanwhile, at the end of this book the most prominent difference between "proceduralist" liberals and their opponents remains that the former, like John Rawls, have actually *done* political philosophy in the style they advocate (whether convincingly or not is another issue), whereas the latter have so far spent all their energy just advocating a change in style.

University of California, Berkeley JEREMY WALDRON

Legal Hermeneutics: History, Theory, and Practice. Edited by Gregory Leyh. Berkeley: University of California Press, 1992. 325p. $15.95.

The word "hermeneutics" tends to divide people into two camps, those who won't go near any book that has the word in the title and those who will buy such a book precisely because it does. For those of you who are in the first camp (but have gotten this far into a book review on the subject), *Legal Hermeneutics*, edited by Gregory Leyh, is a very good place to catch up on what you have been missing, whether you are specifically interested in law or not. For those of you who are already fans of hermeneutics, this book contains a valuable and unusually lucid set of contributions to the development and application of hermeneutic ideas in the law.

What is hermeneutics? Many of the authors, particularly Fred Dallmyer and Gerald Bruns in their excellent essays on the rule of law and the situatedness of texts, respectively, turn to Hans-Georg Gadamer for inspiration. Gadamer believed that interpretation (the activity hermeneutics attempts to understand) is always practical, done for specific purposes in specific situations. Interpreters invariably draw on their broader historical and cultural understandings to work out what various texts mean in the concrete situation at hand—but not because they choose to. Instead, Gadamer argued, they can do otherwise.

These observations have been devastating for many who believe in the rule of law and in the ability of legal

texts like the Constitution to govern through time. Many arguments about the dependence of the meaning of legal texts on the historical and cultural contexts of readers have slid directly into claims of radical uncertainty, instability, and illegitimacy of the law. As most of the authors in this anthology make plain, however, this is not where a deeper hermeneutical understanding should lead. Instead, hermeneutics illuminates why it is that readers in common cultural circumstances generally *do* agree on what a text means. It is precisely because these readers share a common way of life (practices that underwrite interpretations that underwrite practices, etc.) that the meaning of cultural products like legal texts is generally widely shared. And, as Ken Kress notes, *some* indeterminancy in legal interpretation does not completely undermine the legitimacy of law anyway.

Legal Hermeneutics brings together some of the most impressive authors writing about this set of issues. Though a few of the essays (those by James Farr, Kress, and Michael Perry) have been published before, the other essays (by Dallmyer, Bruns, Peter Goodrich, Jerry Stone, Terence Ball, Druscilla Cornell, David Cousins Hoy, Steve Knapp and Walter Benn Michaels, Lief Carter, Gregory Leyh, and Stanley Fish) are published for the first time here. Most of these writers have their primary backgrounds in political or literary theory, rather than in law alone. As a result, this is not the usual set of inquiries about how the Constitution ought to be interpreted in specific doctrinal areas currently under contention but is, instead, a much more enduring contribution to how political and literary theory drives us to reconsider the interpretive practices of the political community in which we live. In general, the quality of historical understanding and theoretical sophistication is much higher in this collection than one generally finds in the vast literature on interpretation and the law.

Hermeneutics is seen in this volume as a discipline with a history, not just a set of slogans to be lobbed at current events in the law. Farr writing about Francis Lieber; Goodrich writing about Petrus Ramus and his followers; and Stone writing about Rudolf Bultmann, Karl Barth, and Paul Ricoeur show how hermeneutical inquiry is not a recent invention for the postmodern age of fragmentation (though Cornell writing about Jacques Derrida, Maurice Blanchot, and Emmanuel Levinas does give the problem a decidedly postmodern spin). The subjects of these essays are people who have thought long and hard about how texts change their meanings over time and yet (for all but the latter set) believe that the interpretation of texts remains within a tradition that is never completely lost.

The largest disagreement in this volume is over the relevance of the historical construction of intention in understanding the meaning of a text. While Knapp and Michaels, as well as Fish, have long thought that intention was crucial to interpretation, Ball and Hoy mount forceful arguments against that view. There are not clear winners and losers among these essays, but one does wonder how much it matters that those who defend a concentration on intentionality have literary works as their main examples and those who reject intentionality concentrate primarily on law. As Gadamer would be the first to point out, the conventions of practice within particular communities of interpretation make all the difference.

In the concluding section of the book, Carter, Perry, and Leyh write about the implications of these debates for legal practice. Perry focuses on how judges' choices of theories of interpretation do affect the outcomes of cases. Leyh concentrates on the conventions of legal interpretation taught in law schools. But perhaps the most telling essay in the whole volume is the description by Carter of a week he spent with 14 trial judges. For these judges, legal hermeneutics did not even register on their mental maps.

That said, there remain good reasons for scholars to be interested in these questions. A focus on hermeneutics connects legal interpretation with other interpretive practices, most notably in theology and in literary criticism. By enlarging the scope of inquiry beyond the narrow and professionally policed boundaries of the law, this volume provides a way to reconstruct the historical and cultural context of debates over legal interpretation, which hermeneutics itself should tell us is where all the action is in the struggle over meaning.

University of Michigan KIM LANE SCHEPPELE

Essays on the Foundations of Aristotelian Political Science. Edited by Carnes Lord and David K. O'Connor. Berkeley: University of California Press, 1991. 255p. $45.00.

The essays in this volume are united in their effort "to rectify the methodological and substantive weaknesses in current approaches to Aristotle's practical science" (p. 7). Most of all, they aim to show the preeminently practical intent of Aristotle's political works. The first four essays examine Aristotle's "social science" generally, and his "anthropology," "economics," and "sociology." The remaining five give fuller treatment to some of the themes that inform the foundations of Aristotle's political thought: justice, gender and citizenship, the best form of government, and the rule of law. The contributors are political theorists (Carnes Lord, Waller Newell, Stephen Salkever, and Abram Shulsky), philosophers (Richard Bodéüs and David O'Connor), a classicist (Josiah Ober), and an historian (Barry Strauss).

Lord claims in the Introduction that Anglo-American scholarship approaches Aristotle "at once too historically and not historically enough" (p. 1). The main reason scholars, with the exception of Leo Strauss and several contemporary Germans, tend either to historicize or to abstract Aristotle, Lord explains, is their failure to appreciate that he aspires primarily to improve (all) human action, and only secondarily, if at all, to theorize about phenomena. Being neither strictly parochial nor strictly theoretical, Aristotle's practical orientation to the human sciences challenges the historicism and positivism that prevail in philosophy and the social sciences.

In addition to demonstrating Aristotle's commitment to practice, the essays correct several particular interpretations, such as that Aristotle regards man as fundamentally participatory, the household morally sterile, and justice opposed to self-interest.

The best of the nine essays are the two by the editors. In "Aristotle's Anthropology," Lord argues that Aristotle's understanding of the origins of society differs from that of contemporary anthropology, which regards the family and kinship relations as critical stages. Lord contends that the evolutionary or organic reading of Aristotle's account of the development of the city rests on a misunderstanding of his view of man's politicality;

man is fundamentally not a pairing or bonding animal, according to Aristotle, but a combination of spirit and reason. Spiritedness, not sociality, explains the coming-to-be of society; for it impels men to resist being ruled as much as it brings them together, and it explains barbarism and tells why society did not (and does not) come to be according to a gradual, universal process. Aristotle's focus on the household at the beginning of the *Politics* is a rhetorical move, Lord explains. Aristotle does not want to reinforce either conventionalism or agonism in political life but aims, by establishing the naturalness of political rule, to reinforce the comradeship that arises naturally among men from their common perception of, and desire to secure, the just and noble.

O'Connor's "Aetiology of Justice" should be read by all theorists interested in the subject of justice. Its main objective is to weigh the claims of a contemporary view of justice, a view which links justice with altruism and impartiality, against those of Aristotle's view, which regards orientation of desire as central to justice. Presupposing a distinction between self- and other-regarding virtues, contemporary moral philosophers, such as Bernard Williams, define *injustice* as a kind of egoism, an indifference to promoting just distributions. Contrary to contemporary interpretations of Aristotle (e.g., Philippa Foot's), Aristotle does not, according to O'Connor, distinguish self- from other-regarding virtues; justice is the symptom or relational manifestation of correctly oriented desires. The source of injustice is, then, according to O'Connor's reading of Aristotle, not a *specific* vice, like egoism or indifference, but misoriented desire. The problem fundamental to justice, then, is "not the aetiological one of the possibility of altruism" but the political one of the possibility of a coincidence between the good man and the good citizen (p. 147).

Salkever's opening essay contrasts Aristotle's "social science," which seeks to improve praxis by presenting the human telos not in terms of some transcendental ideal but as the mean, with both empiricist and interpretive social science, both of which preempt moral relevance—empiricism by reducing human beings and their creations to composite parts and the interpretive approach by denying any essential human relation to nature (its findings are strictly local and archival). Such criticism is compelling, but Salkever dissolves the tension between politics and philosophy that Aristotle sees by blurring Aristotelian distinctions between the nature of the activities, their ends, and the faculties engaged.

Shulsky's essay, which argues that economics, for Aristotle, is a practical science, lacks discussion of Book 5 of the *Nicomachean Ethics*, part of which might be understood to be an attempt to theorize about economics that is compatible with his practical counsel.

Ober's piece on Aristotle's "political sociology" argues that Aristotle's categories of sociopolitical analysis converge not with the materialistic Marxist concept of class, which is fused with the concept of exploitation, but with the more ideological, Weberian categories of status and order, and are useful in showing the societal differentiation of the polis that Aristotle himself recommended. The contemporary relevance of the *Politics*, then, with its presentation of an ideal ordered state with a homogeneous citizenry, is its attempt to show how the political and social can be linked to a socially diverse world so as to shore up moral community as far as possible.

Salkever's second and more successful contribution (published, like his first, in two previous versions) argues that Plato's and Aristotle's countercultural reservations about how far political activity can make human beings virtuous go hand in hand with their appreciation of things private, such as the family and women.

Newell, Strauss, and Bodéüs write on Aristotle on regimes. Newell points out that Aristotle's association of monarchy with absolute justice reveals both monarchy's status as a theoretical standard and political community's nature as a creature of compromise. Strauss claims that Aristotle intentionally—for socioeconomic, political, rhetorical, and philosophical reasons—depicts Athens as an extreme, rather than as the constitutional democracy it was. Bodéüs argues the importance, in Aristotle's eyes, of moral education in the form of law to the duration of regimes and contends, moreover, that Aristotle evaluates regimes on the basis only of their internal consistency. As Paul Vander Waerdt has elsewhere pointed out against Bodéüs, however, Aristotle encourages legislators to work for a second telos, the unqualified betterment of the regime, in addition to preservation.

Lord notes in the Introduction that the collection might have included essays on other topics. A piece on the relevance of Aristotle's biology to his political works, written by Leon Kass or Roger Masters, for example, would, indeed, have been welcome, as would a piece on psychology by, for example, William Fortenbaugh. (Also, Arnhart's book title on p. 44 should be *Aristotle On* Political *Reasoning*.) Not missing or in need of changing, however, is the good index.

Overall, this collection, by improving much predominant thinking about Aristotle's treatment of the human sciences, reveals the Aristotelian challenge to contemporary methods of social science. It also provides a service to the field of political theory by furnishing models of how to illuminate philosophical texts with historical information. In both of these ways, it encourages needed dialogue between social scientists, philosophers, historians, and classicists.

Similar in scope, but not in purpose or unity, to this collection (and providing an *index locorum* instead of a subject–author index), is *A Companion to Aristotle's Politics* (1991), edited by David Keyt and Fred D. Miller, Jr.

Boston University JUDITH A. SWANSON

Ricardian Politics. By Murray Milgate and Shannon C. Stimson. Princeton: Princeton University Press, 1992. 169p. $32.50.

Given Ricardo's reputation as a rather difficult economic theorist and little else, the thesis is novel: "We do wish to claim that Ricardo formulated much of what is characteristic of the democratic political creed of early philosophical radicalism in a distinctive way, that he linked his economic and political speculations in a systematic and novel fashion, and that a re-examination of the grounds upon which he made those arguments is a useful and necessary exercise in historical and theoretical recovery" (p. 6). The incredible thing is that this monograph goes a long way to prove these points.

This is done in a twofold manner: (1) by a meticulous examination of the basis for Ricardo's adherence to a radical reform of the parliamentary franchise (near universality of male suffrage, frequent elections, redistribution of seats, the secret ballot, and greater liberty given

to public assemblies) during the period of preparation for, and his actual, parliamentary career (1818–23); and (2) by a continuous counterpoint with the political thought of James Mill, who was, to a certain extent, Ricardo's political mentor. To this is added a subsidiary counterpoint to the thought of Bentham and John Stuart Mill. Were the counterpoint as sound as the reading of Ricardo, this would be a book to place in the same class as Donald Winch's *Adam Smith's Politics* (1978). But there is a major problem in the comparison with the thought of James Mill.

Ricardo is placed in a new light by suggesting that his thought was far more progressive than Mill's in the extent to which he accepted the class-divided nature of the society of his day and was remarkably unafraid of seeing class tensions expressed in the political and parliamentary arena:

> "Ricardo . . . never once thought that this underlying economic rivalry would simply vanish once the vote was granted. In fact, quite the reverse was the case. Even when . . . those competing interests were not just those of competing individuals but were those of different social classes, Ricardo seems to have been perfectly comfortable with the idea that politics existed precisely to allow all those economic interests to gain political expression, rather than to quiet them" (p. 106; see also pp. 118–19).

This is proved beyond the shadow of a doubt by the texts cited from Ricardo's articles, speeches, and letters. For this alone, the work would be worth reading.

But the comparison with the thought of James Mill misses an important problem. It is based upon a close reading of Mill's essay "Government," as well as a certain number of his other works. The difficulty with taking "Government" as representative—or even as the core—of Mill's thought is simple: it is not his *strongest* essay but, rather, one of the *weakest* of the 12 he wrote for the *Encyclopaedia Britannica* between 1816 and 1823, since he wrote it with one arm tied behind his back. Macvey Napier, the editor, had hired Mill to write a series of articles representative of the Benthamite position in social, political, and legal thought (s.v. "Jurisprudence," "Law of Nations," and "Prisons and Prison Discipline," in particular). But Bentham had published his *Plan of Parliamentary Reform* in 1817, and either it or the review James Mackintosh made of it in December 1818 in the *Edinburgh Review* put the fear of God (or rather of political radicalism) in Napier. As a result, Mill was forced to give but a truncated version of the Benthamite position, as the contradictory and feeble nature of much of the argument reveals. Had the authors of *Ricardian Politics* used Mill's later article "The *Edinburgh Review* on Parliamentary Reform" (*Westminster Review*, July 1825), they would have realized that Mill was substantively closer to Ricardo than they here make out. (But their case that Ricardo was more progressive and less backward looking would have still been sustained.) Elsewhere, they also reveal a slightly uncertain hand with regard to Bentham; his unpublished critique of judicial establishments in France (see p. 133) was published as early as 1790, for example; and their remarks about his inadequacies as an economic theorist reveal a certain lack of sympathy with the ends to which his analysis was to be put.

But despite these comments, this book marks a new stage in the consideration of political economy in the development of liberal thought in the nineteenth century. It is lucidly, at times elegantly, written. The authors have an enviable command of the relevant social, economic, and political literature of the period. Their reflections on the links of Ricardo's economic thought to later political radicalism—from the misnamed "Ricardian Socialists" (they were neither Ricardian nor socialists) to the thought of Marx and beyond—are most stimulating. As an essay in revisionism it should go a long way in readdressing how political economy's relationship with political thought is viewed.

University of Toronto ROBERT A. FENN

Managerial Dilemmas: The Political Economy of Hierarchy. By Gary J. Miller. New York: Cambridge University Press, 1992. 254p. $47.95.

Managerial Dilemmas is a fascinating book that introduces political scientists to an array of organizational difficulties with rational underpinnings. Miller is interested in organizational problems that cannot be solved merely by improving communication between superiors and subordinates or by correcting decision makers' flawed thinking. The situations he analyzes share several properties. They can arise even if decision makers have perfectly accurate beliefs about the organization's behavior and outcomes and even think in fully rational ways. Their essential feature is that individually rational behavior can produce collectively undesirable outcomes. Hence, they can justifiably be regarded as enduring dilemmas confronting public and private organizations.

There are, broadly speaking, two types of rational choice scholars in political science. The first type, inspired by models of perfectly competitive markets, believes that institutions are Pareto-optimal. If they were not, someone could make a buck, get elected, or otherwise gain by proposing a new, Pareto-superior institution. The second type, relying on noncooperative game theory, is more pessimistic and is quite willing to believe that situations can endure indefinitely (i.e., as Nash equilibria) yet be collectively suboptimal. Miller belongs squarely in the second camp. In the heart of the book, chapters 4–7, Miller analyzes several important kinds of authority systems and incentive schemes and demonstrates how they can lead to suboptimal outcomes.

The white knight that rides in, in chapter 9, to restore Pareto-optimality to organizations is repeated play. As Miller emphasizes, it is well known that if future payoffs are valued enough, behavior that is not individually rational in the short run (e.g., cooperation in the prisoner's dilemma) can be sustained as a Nash equilibrium in the repeated game. This means that organizationally desirable outcomes can be realized despite the failures of formal authority and incentive systems.

Unfortunately, as Miller also emphasizes, the white knight has a weakness—the folk theorem. This result says, roughly, that when the future is sufficiently important, *any* outcome that gives each player at least that player's "security level" (what he can unilaterally guarantee himself) can be supported as an equilibrium by appropriately chosen strategies. Miller convincingly argues that the folk theorem poses two serious and related problems for decision makers: How will they coordinate on reaching a particular equilibrium? and How will different players fare in the chosen equilibrium? *Managerial Dilemmas* is probably the first book in political science that confronts the implications of the folk theo-

rem head-on. For example, Miller fully recognizes that "repeated play makes cooperation in a team possible, but not inevitable" (p. 214).

Though Miller draws on a technical literature, all the basic ideas are explained in clear English. There is no unnecessary jargon, and no unnecessary mathematical notation or equations. The book is therefore highly accessible to non–technically trained political scientists and organization theorists who are interested in learning about the "new economics of organization." Indeed, it is probably the best introduction to this literature now available to political scientists.

Occasionally the presentation's informality causes problems. For example, chapter 6 examines an interesting impossibility result: given interdependent production that is unobserved by the boss, there exists no incentive system that simultaneously (1) is Pareto-optimal, (2) is a Nash equilibrium, and (3) balances the budget (the incentive system distributes the organization's revenues without deficits or surpluses). Unfortunately, the discussion fails to make explicit that this theorem presumes one-shot interaction. The reader is thus led into believing that the theorem's implications are more far-reaching than they really are.

Similarly, chapter 5 models the difficulty of reaching a Pareto-optimal deal via piece-rate contracts in the context of a one-shot game; again, this context is implicit. Because Miller does not provide an explicit model of piece-rate deals in a repeated game, the reader does not learn whether Pareto-optimality can be sustained as a Nash equilibrium when contracts extend over time. The discussion then moves to an empirical examination of a firm, Lincoln Electric, where such contracts do seem to work. But although the description clearly shows that this case involves repeated play, the reader has not been analytically prepared for the role of ongoing interaction in securing cooperation and may therefore be puzzled why Lincoln's system succeeds.

Miller provides an answer: the owner has *committed* himself to abiding by the terms of the deal (not cutting back on the rate once he sees how much the workers can churn out). But invoking the notion of credible commitment does not clarify matters. Sometimes the idea is used to explain how a particular outcome is selected in a game with multiple equilibria. (One thinks of Schelling's example in which a driver in a game of chicken throws his steering wheel out the window.) But equilibrium selection does not seem to be the whole story in Miller's analysis of the Lincoln case; for in his account, management continues to demonstrate, in an ongoing way, its commitment to the cooperative equilibrium. Thus, credible commitment helps to sustain an equilibrium as well as select one. But because the Lincoln story involves repeated play, where strategies of conditional cooperation can form Nash (and even stronger) equilibria, commitment may be unnecessary. Once the workers and managers of Lincoln are in the cooperative equilibrium, little is added to the analysis by saying that the parties have committed themselves to their strategies. Each person is simply playing optimally, given everyone else's strategy; and this is all that standard Nash equilibrium analysis has to say about the matter. One therefore suspects that Miller is going beyond the standard analysis. In several places, he refers to the "fragility" of the cooperative equilibrium and to the role of managerial commitment in making that outcome more robust. Miller is onto something here; perhaps future work will clarify these issues.

Miller reaches for an admirable integration of rational choice theory and fieldwork based on social-psychological and sociological perspectives. Sometimes the integration comes off beautifully. For example, the analytical discussion of the bad equilibrium in some companies using piece rates is accompanied by a perfectly appropriate recapitulation of William F. Whyte's descriptions of workers and superiors locked in lose–lose conflicts.

Occasionally, the integration is more forced. Miller tries to argue that "mechanistic" hierarchical incentive systems are inefficient and that the "warm fuzzy" approaches from the human relations school of organization theory, involving nonhierarchical work teams and close interpersonal ties, help greatly in reaching Pareto-optimal outcomes. But it is not always clear how all of these components of the human relations school help pick out the socially desirable equilibrium. (The role of some components, such as trust, is clear.) Conversely, it is not always clear why hierarchy must go. If it is repeat play that is doing most of the work in stabilizing benign outcomes, then hierarchical authority structures (and even "mechanistic" incentive schemes) could be maintained; they simply have to include the appropriate stick-and-carrot components. Indeed, one could argue, as Miller does, that "leadership" (a more positive word for hierarchy?) is crucial in solving the equilibrium selection problem.

But these problems reflect the ambitiousness of Miller's efforts: he does not shy away from hard problems on the frontiers of organization theory or game theory. At the same time, he has written a spirited book that can be profitably read by strong undergraduates and entering graduate students. It is a fine achievement.

Stanford University JONATHAN BENDOR

Love and Power: The Role of Religion and Morality in American Politics. By Michael J. Perry. New York: Oxford University Press, 1991. 218p. $29.95.

In the contemporary debate between liberals and communitarians, most participants have come to accept some important concerns of their opponents. Communitarians acknowledge the importance of social pluralism and individual freedom, and liberals realize that the moral development of citizens is vital to the political system. However, liberals have difficulty incorporating into their theories conceptions of moral standards that are nourished by groups with a comprehensive conception of the good life—religious groups, for example. Liberals fear that in teaching people how they ought to live, such groups may fail to engender forms of moral development that are compatible with respect for individual rights. At the same time, the political order may need the moral standards taught by groups with a comprehensive view of the good.

In *Love and Power*, Perry argues that too many Americans wrongly neglect the contributions of religious groups in the justification of, and deliberation about, political questions. Perry, an acknowledged Catholic, believes there is a widespread and mistaken assumption that religious morality should be, or can be, excluded from political discourse in a pluralistic society that is committed to freedom. He insists that liberal thinkers

seek neutral politics by excluding discourse premised on conceptions of the good that everyone does not accept. He claims that the removal from discourse of unshared premises is itself not neutral because it unfairly excludes from public consideration the moral positions based on religion. Moreover, this exclusion denies the political order the benefits of religious morality.

In place of an impossible neutral politics, Perry defends an "ecumenical politics," in which "beliefs about human good play a basic role in public deliberations about, and public justifications of, contested political choices" (p. 43). In particular, religion offers "a vision of final and radical reconciliation, a set of beliefs about how one is or can be bound or connected to the world—to the 'other' and to 'nature'—and above all, to Ultimate Reality in a profoundly intimate and ultimately meaningful way" (p. 77). Since every religious vision comprises moral beliefs about how one should live, every such conception of what it means to be fully human implies acceptance of some basic responsibility for the well-being of others. In this sense, all religion is political, not private.

According to Perry, ecumenical politics is communitarian because it strengthens political bonds. For example, religious dialogue can celebrate and support basic American values like our constitutional premises about human rights. Also, Perry emphasizes that dialogue based on different and competing religious and moral traditions can serve an integrative function by transforming sensibilities and by bringing cognitive changes. Of course, many readers may think that Perry gives too little attention to the divisions that religious discourse can bring. He insists that ecumenical politics is fallibilist, because it embraces self-critical rationality, and pluralistic, because it respects divergent ways of life as a source of deepening moral insight. These aspects, plus compassion, will lead to political tolerance, not to authoritarianism; the state will not be used for coercion. Contrary assumptions, he thinks, can be traced to the widespread ignorance about religion of otherwise well-educated persons and to the assumption that all religious persons must make the same extreme political demands that are made by some fundamentalists.

Perry correctly argues that political discourse based on the sharing of religious values does not necessarily lead to attempts to enforce a view of the good on others. However, Perry's ecumenical politics is an ideal form of dialogue that will not always be practiced. We need to understand how the political system can draw upon the views of groups with full theories of the good—views that may support moral standards helpful to society—without imposing a comprehensive conception of the good. Some general guidelines may be given, guidelines that should be considered by those taking Perry's viewpoint. Political authorities must support only moral standards that clearly benefit the entire society, and individuals must retain the freedom to reject these standards in their personal lives. Moreover, government must only support the aspects of morality that are not inextricably bound up with a particular view of the good; and government must not proclaim that some conception of the good is superior to other ways of life. Such guidelines require continued commitment to the distinction that Perry too cavalierly dismisses, namely, the liberal distinction between the right (principles of justice guaranteed by the state) and the good (the way of life chosen by individuals). Adherence to this distinction does not preclude Perry's goal, the enrichment of political discourse as citizens share their deepest understandings of the good life, understandings that may be religious in nature.

University of New Orleans ROBERT B. THIGPEN

Equal Justice. By Eric Rakowski. New York: Oxford University Press, 1991. 385p. $49.95.

Justice requires that equal consideration be given to the welfare of everyone affected by some system for distributing benefits and burdens. The presumption is in favor of equal shares; inequality, not equality, demands justification. This is the starting point for most modern discussions of equality, and *Equal Justice* is no exception. But equality of consideration is, as Eric Rakowski's study demonstrates, often an ambiguous, complex, and controversial criterion for choosing among possible distributive schemes. It is, for example, sufficiently indeterminate to accommodate such rival goals as maximizing the aggregate utility or equalizing for each person the consequences of some distributive scheme. Rejecting both of these schemes, that is, utilitarianism and equality of welfare, Rakowski argues that justice is "equality of fortune." This means that "no one should have less valuable resources and opportunities available to him than anyone else, simply in virtue of some chance occurrence the risk of which he did not choose to incur" (p. 1). Thus, no individual should be worse off or better off than anyone else simply because of good or bad luck (e.g., unequal genetic endowments, certain illnesses, a serendipitous discovery or invention). When inequality is the result of "option luck," like a successful or failed business venture, justice does not require that equality be restored. The general idea is that people's shares should be kept equal "except to the extent that their voluntary actions give rise to inequalities" (p. 75). This is a thesis best revealed in its details. What sort of things count as resources? How is their value to be determined? Do those with expansive tastes need more resources than others? Does one forfeit a claim to compensation or help from others by failing to insure against some foreseeable risk? *Equal Justice* is a long and often demanding book. It is also an uneven one.

If justice means equality of fortune, then it seems that every misfortune that one could not reasonably have taken measures to avoid generates a claim for compensation from society. In effect, every *mis*fortune is an *in*justice. Illness, injury, genetic endowments, physical handicaps, social environment, family, opportunity—where any of these constitute a disadvantage for which one cannot be held responsible, a just society will seek a remedy. Where, alternatively, they constitute an advantage for which one cannot take credit, a just society will insist that the benefits of that good fortune be shared. Rakowski is the first to admit that the ideal of equality of fortune, given the complexity and range of factors that must be considered, would be difficult to honor. Indeed, I would imagine it clearly impossible. Rakowski himself notes (to take one instance) that it is probably impossible to determine which part of some successful business enterprise is attributable to voluntary effort and which part is attributable to natural talents for which one does not deserve reward. Rakowski concedes that the theory of equality of fortune will not always (often) yield criteria

for feasible policies. In short, *Equal Justice* is a work in ideal theory pretty much removed from a concern with possible transformations of the world. It is, as much of moral philosophy can be, a suppositional inquiry in which the chief concern is to discover what justice means in cases where actual practice and conventions are bracketed as largely irrelevant. Not surprisingly, therefore, Rakowski is at his best when comparing rival principles and criteria for distributing benefits and burdens. He is less successful at probing the sensibilities that might inform conventional understandings of justice. This is especially evident in the chapter where he defends the nonvoluntary harvesting of cadaver organs and, under certain restrictive circumstances, compulsory organ transplants from live donors. Allowing for a moment that "human tissue is a resource" and that equality of fortune mandates some sort of redistributive donor scheme, it seems equally obvious that cultural meanings and shared understandings must count for a lot here. My complaint is not with how Rakowski balances these considerations but with his failure to see that some balancing is necessary.

It is probably inevitable that part 1 of *Equal Justice* will receive the sort of disproportionate treatment that I have given it. This is unfortunate because part 2, where Rakowski outlines a theory of corrective justice in the context of torts and rules of negligence and strict liability helps to expand the thesis of equality of fortune argued for in part 1 and is, I think, more successful in demonstrating when injury requires compensation. It includes an important chapter criticizing Richard Posner's theory that law should aim at the maximization of social wealth. Part 3, however, which takes up questions about the relevance of numbers to life-saving decisions and the killing of animals to benefit humans, stands alone and should have been published elsewhere.

University of Florida ALFONSO J. DAMICO

Contractarianism and Rational Choice: Essays on David Gauthier's "Morals by Agreement." Edited by Peter Vallentyne. New York: Cambridge University Press, 1991. 339p. $59.50 cloth, $18.95 paper.

Peter Vallentyne has brought together 17 important essays discussing David Gauthier's book, *Morals by Agreement*. A central theme that pervades these articles is summed up best by Peter Danielson, who asks, "Why should agents concerned only to satisfy their own preferences constrain themselves by impartial moral principles?" (p. 292).

Gauthier's social contract emanates from an ideal bargaining position in which each individual as a utility maximizer of one's own preferences comes to realize that each person does individually better by accepting constraints against exploitative conduct, in situations, as Jean Hampton says, where each "has good reason to believe that their partners are inclined to cooperate" (p. 40). In a Pareto-efficient world or a free-market setting, characterized by perfect competition, each would (as Jody Kraus and Jules Coleman say) do as well as possible by acting in purely self-interested ways; and in this case, "it would be irrational for any agent to impose restrictions on the pursuit of self-interest" (p. 255). But when, as in the real world, perfect competition does not take place, this situation creates a Pareto-inefficient environment (p. 256). In this setting, each agrees to constraints each individual believes to be conducive to one's own quest to maximize preferences. Individuals would accept, as the main principle of acquisition, the Lockean proviso, which, in the words of Peter Vallentyne, says that interactions are "subject to the constraint that no one makes him/herself better off by making someone else worse off" (p. 7). The principle of *minimax relative concession* (MRC), which encompasses the Lockean proviso, defines, as Hampton says, the fair terms of cooperation (p. 41). To quote Vallentyne, the MRC indicates that "rational agents would agree to an option for which the highest relative concession is as low as possible" (p. 8).

Gauthier's argument for "constrained maximization" suggests that individuals seek to avoid exploitation, in the hopes that each will be able to maximize one's own preferences. In this vein, both the Lockean proviso and the MRC principle are norms that individuals would voluntarily comply with. Here, the basis for a motivation to support constraints is not Hobbes's Leviathan, which might end up trampling the freedoms and rights that liberal regimes are supposed to secure but an agreement entered into by individuals on a voluntary and noncoercive basis.

But can a motivation to support Gauthier's rules be located solely in self-interest maximization arguments that culminate in a need to avoid exploitation? The book's most important contribution to contemporary scholarship is found in the essays suggesting that contrary to Gauthier, individuals support moral norms not solely from rational choice calculations but from the presence of a moral personality, which suggests rules that individuals find morally valuable for their own sake. Jean Hampton, in her insightful essay, argues that Gauthier needs to recognize that implicit in the commitment to avoid exploitation is the notion of respect for persons and that this idea is a necessary "price" we must pay if we are to avoid making ourselves prey for others (p. 53). In the same vein, David Braybrooke wonders how rights for all and moral progress of any sort can be made unless individuals have "humane feelings" for each other; and so long as Gauthier's theory is ambivalent on this point, his argument "offers only mixed assistance to moral progress" (p. 70). Holly Smith argues that for utility-maximizing individuals, adherence to moral rules means sacrifice and that as a result, individuals would have to approach life with an extreme sense of another important moral trait, perseverance (p. 247).

In the same vein, Geoffrey Sayre-McCord argues in his important essay that many individuals have "sound self-interested" grounds, influenced by the logic of moral argument, for "abandoning egoism in favor of morality" (p. 195). In particular, there are individuals for whom the goals of a moral community are a matter of prime importance over and against self-interest maximization or egoism: "This last group is especially important . . . because almost all human beings belong to it" (p. 195). Peter Danielson, in his fine essay, rejects Gauthier's linking cooperation to the market (and thus to a notion of "exclusive property rights") and says that cooperation, agreement, and bargaining presume "appeals to equality and noncoercion" and thus to "precontractual individual rights [that] do not need [to be] fully developed property rights" (p. 107). Here, it would seem, the ground of community is not market relations

and property rights but a Kantian respect for persons. Jan Narveson says in his interesting essay that even if the initial bargaining position incorporates the assumption that individuals are predatory, they will forgo predatory conduct in the future because they will each realize that predation wastes "efforts which could instead be converted into mutual gains" (p. 144). But even in this case, moral progress would require a sufficiency of "humane feelings" so that individuals would be able to believe that they could work together in future encounters. Finally, David Copp's excellent essay demonstrates that individuals, even if they conditionally comply with Gauthier's rules in a hypothetical bargaining position, may find, given a variety of moral and nonmoral motives, that in the actual world, it is not "rational to dispose themselves to comply with these [Gauthier's] principles" (p. 223). Often, peoples' moral preferences are integral to their core identities; and these preferences govern their actual choices, irrespective of the requirements of constrained maximization (p. 224).

Gauthier, in the concluding essay, goes a long way to diffuse the arguments of some of his critics. Gauthier argues that his individual is much more accommodationist than he would at first seem: "In his interactions, others are led, through knowledge or belief about his dispositions, intentions, and plans, to behave in ways that enable him to do better, even though he does not maximize, than he could expect to do were he believed to be disposed to perform only maximizing acts" (p. 328). The accommodating individual is more willing than the interest-maximizing individual to accept the norms of cooperative life as principal goods to be striven for, whether or not they can be derived from a rational choice argument.

While this volume is an excellent contribution to the understanding and critique of the leading ideas in Gauthier's approach to the liberal social contract, the volume would have been made substantially more useful if essays had been included dealing with the kinds of political and social policy that are necessary in order to secure the liberal social contract that Gauthier's theory requires. In effect, what is missing here is a concern for the shape of politics in a Gauthier-oriented regime. The article by Donald Hubin and Mark Lambeth showing that the Lockean proviso is not an adequate foundation for rights is very helpful in this regard; but more essays of this sort dealing with questions like the importance and place of various rights should have been provided (p. 126). Finally, in this regard, how is the rational choice approach useful in confronting many of the current problems facing liberal societies, like abortion, medical care, welfare reform, race, and so on? By not addressing concerns of this sort, the book, while excellent in explaining and providing telling criticisms of Gauthier's concept of constrained maximization, fails to demonstrate the importance of these arguments to the current effort both to justify and sustain the liberal social contract.

Miami University, Ohio STEVEN M. DELUE

AMERICAN POLITICS

The Logic of Congressional Action. By R. Douglas Arnold. New Haven: Yale University Press, 1990. 282p. $29.95.

The purpose of this book is to explain why Congress enacts the policies that it does. Amid the many issues this concern raises for Douglas Arnold, one question is paramount: Why does Congress sometimes break free of particularized interests and enact legislation that serves the general public? Since reelection is the primary concern of legislators, Arnold asserts, incumbent support for particularized group or geographic interests fits their needs by facilitating "advertising" and "credit claiming." But these same legislators sometimes approve policies that deliver collective goods to the general public while imposing demonstrable costs on voters. Why?

To answer this question, Arnold creates the most sophisticated model of Congress yet proposed by electoral incentive theorists. In Arnold's world, legislators are concerned not just with the *expressed* preferences of attentive citizens but with the *potential* preferences of an inattentive public. After all, legislators might respond so completely to particularized interests in a policy area as to produce general policy effects detrimental to the public as a whole and thereby generate electoral backlash against themselves. Legislators must thus develop extraordinarily fine-tuned electoral antennae, constantly estimating not just immediate policy positions of groups but probable public reactions once a program's effects begin to be felt.

Proponents and opponents of policies develop procedural and statutory strategies that cater to legislators' electoral calculations. In particular, they arrange voting processes so that electorally beneficial policy decisions will be traceable to visible public votes, whereas detrimental decisions will be less visible. And they draft legislation so that electorally beneficial policy action will have immediate and salient policy effects, whereas detrimental policy effects will be delayed and less salient. Once these and related issues are understood, Arnold argues, it then becomes evident how and why Congress can enact policies serving general, rather than particularized, interests. It can do so when a program's general beneficial impact is salient, or potentially salient, to a substantial number of citizens; when coalition leaders can employ strategies that allow citizens to connect the positive general effects to their legislators' votes; and when coalition leaders can hide the connection between the legislators' votes and any negative particularized impact of the program. Arnold illustrates these processes with case studies drawn from the 1970s and 1980s, concluding that during these years legislators' electoral calculations supported general, as well as particularistic, policymaking.

Arnold's effort in developing and illustrating this thesis is impressive, producing one of the most illuminating books I have read on Congress. Every page provides a new insight into some aspect of congressional policymaking. Students of congressional elections, coalition building, roll-call voting, and legislative careers; specialists in budgetary and economic policy, tax policy, and energy policy; theorists of political representation and rational choice analysts—all will find nuggets of golden insight into their special concerns in Arnold's analysis. Arnold's differentiation between actual and potential preferences and legislators' concern for the foreseeable effects of policies are major innovations in electoral incentive theory. And the effort to demonstrate that electoral calculations can account for the enactment of general, as well as particularized, benefits is one of the great tasks confronting students of representative democracy. For all the brilliance of the effort, unfortunately, the book fails to deliver on this central goal.

The primary problem with Arnold's argument lies in a confusion over the distinction between particularized benefits and collective goods. The allure of Arnold's basic puzzle flows from the long-standing debate over whether a representative assembly can serve the public interest by providing for collective goods, particularly when private interests and particularized goods would seem to be the salient concerns of voters. At the outset, Arnold identifies his effort with this classic debate, arguing that general benefits refer to "collective goods that people value because they believe that everyone profits," with such benefits falling "uniformly on members of society" (p. 26); particularistic goods, by contrast, are the benefits that fall on identifiable group or geographic interests. The trouble is that Arnold assumes that group or geographic interests are the only particularistic interests that concern voters. In point of fact, much of the tension between private and collective interests arises from citizen's desires for highly salient, specific, and divisible *individual* benefits, illustrated by their concern for policies that maintain low prices and, in the post–Proposition 13 world, lower taxes. Electoral incentive theorists have long considered such individualized and private benefits to be the "meat and potatoes" of reelection campaigns, with incumbents invariably positioning themselves to claim credit for such salient benefits.

By equating particularized policies solely with those that relate to group or geographic benefits, Arnold includes in his general policies the programs that deliver private and divisible goods to individual citizens. Arnold's theoretical logic and empirical proof then becomes preoccupied with demonstrating that Congress is willing to enact policies that deliver highly salient, specific, divisible and individualized benefits. The resulting analysis provides powerful proof that private and particularized *individual* benefits can compete with particularized *group* or *geographic* interests for the support of electorally attentive legislators. It does not demonstrate that the electoral calculations of legislators support the enactment of general policies that deliver diffuse collective goods.

Missing from the book is a serious consideration of what constitutes a collective good. General programs providing collective goods probably should be expected to meet at least three conditions: (1) the program's primary benefit should be substantial, shared, and indivisible among citizens (e.g., clean air); (2) there should exist real reason to believe that the enactment of the

policy would deliver the shared and indivisible benefit (so that such claims are not simply symbolic); and (3) the program should have some substantial private costs, so that government action is required to obtain the beneficial effect. The existence of a collective good cannot be assumed simply as a result of the reversal of group or geographic interests, as a result of an assertion by coalition leaders of a collective benefit, or as a result of broadly distributed effects of the policy. Rather, there must be substantial reason to believe that a real, shared, and indivisible benefit would accrue from government action.

Applying this definition, none of Arnold's policy cases could be convincingly demonstrated to be general policies designed to provide collective goods. Most clearly provide individualized and private goods (low prices or limited taxes) or restructure the distribution of particularized benefits among individual, group, and geographic interests (the combined taxing and spending cuts of 1981 and the Tax Reform Act of 1986). Others are largely symbolic programs enacted at a time when no real prospect existed for their implementation (gasoline rationing in 1980) or drafted in such a way as seriously to decrease the likelihood of full implementation (as with Gramm–Rudman). In addition, appeals to the collective benefits of programs were often based on highly suspect scientific theory (as in the appeal to supply-side economics to support Reagan's tax and spending cuts).

Seen in this manner, Arnold's logic and case studies explore not how electoral calculations and the reliance on strategic ploys account for the provision of collective goods during the 1970s and 1980s but, rather, just the opposite. Arnold has chosen to study congressional policymaking in a period in which the great cyclical mood swing of American politics (as demonstrated recently in the statistical analysis of James Stimson) was moving away from a sustained concern with collective goods and toward a concern with private goods and individualized benefits. This move took place even in the face of considerable argument that such developments would create great collective costs. He is also studying Congress in the wake of the reform movement of the early 1970s that decreased the institution's capacity for sustained and independent deliberation, and expanded the capacity of citizens and interests groups to audit legislators' individual policy positions. The reform movement thus made members highly vulnerable to the growing public pressure for salient, specific, divisible, and individualized goods. What Arnold has demonstrated is that coalition leaders exploited legislators' increased vulnerabilities in the 1970s and 1980s in order to pursue an expansion of privatized and individualized benefits and to reverse historic concerns with collective goods and the general public interest.

The pressing theoretical question is whether the logic that Arnold outlines will work when the public again becomes concerned with collective problems. Do the same electoral calculations and coalition strategies that produce support for private goods and individualized benefits also generate support for collective goods? I have my doubts. It is simply much easier to design policies that provide immediate and specific benefits of a private and individualized nature than it is to design collective policies that do so (seen in the contrast between the immediacy of lower taxes or restrained prices and the long-term benefit of, e.g., cleaner air). How, then, does one proceed who truly seeks to enact diffuse and shared collective benefits? Can Arnold yet demonstrate that the electoral calculation of legislators is compatible with the provision of collective goods? I believe that he can; but to do so, he must make at least two additional adjustments in his theory.

First, Arnold must develop a more sophisticated interpretation of the voting public, with citizens, as well as politicians, attentive not just to immediate policy benefits but to long-term policy effects and concerned, therefore, not just with private and particularized policy agendas but with shared and indivisible policy benefits. Such an approach would place considerable emphasis on the role of ideas and political explanation in electoral politics, with the prospect that the greatest electoral weapon to sustain incumbents in office may be policies that demonstrate the collective benefit of government. It would also need to recognize that because voters assess private, as well as collective, effects of policies, government must provide collective benefits without undue private costs, and in a manner that ensures that the private costs that are incurred generate the desired collective benefits.

The incorporation of such considerations into electoral incentive theory would then require a second innovation, a determination of the institutional arrangements that facilitate authentic congressional deliberation over whether to deliver (and how best to fund) collective goods. Arnold alludes to such concerns briefly in the concluding chapter and appears to believe that a realistic path to a more responsible Congress comes through voting procedures that inhibit floor amendments (thereby undermining particularized pressures on members) and through increased visibility of final votes on substantive legislation (thereby stressing the salience of such votes). My suspicion is that such strategies, in and of themselves, will only increase the electoral pressures on legislators to respond to the desires of citizens for salient short-term divisible individual benefits. For Congress to reassert its legitimacy as a policymaking institution capable of pursuing collective goods in a responsible manner, it must strengthen its age-old stalwarts of collegial decision making: the parties, centralized leadership, and a deliberative committee system. The ultimate key to such strengthening is for politicians, journalists, citizens, and academicians(!) to grapple with the central issue of democratic government, namely, whether there are collective goods and a public interest that we require a representative government to address.

These, then, are my reactions to *The Logic of Congressional Action*. Others will undoubtedly respond to Douglas Arnold's arguments and closing reflections with different concerns. It is to Arnold's credit that he has tackled one of the most central and unsolved puzzles of political science and has framed the debate in new, innovative and provocative ways. *The Logic of Congressional Action* is destined to become a classic investigation of congressional decision making and a welcomed stimulus to a new generation of theorizing and policy analysis. Arnold has yet to demonstrate that the electoral calculus of legislators yields general policies providing collective goods, but he is clearly knocking at the door.

University of Colorado, Boulder LAWRENCE C. DODD

Nominating the President. Edited by Emmett H. Buell, Jr. and Lee Sigelman. Knoxville: University of Tennessee Press, 1991. 300p. $42.95 cloth, $18.95 paper.

Super Tuesday: Regional Politics and Presidential Primaries. By Barbara Norrander. Lexington: University of Kentucky Press, 1992, 239p. $29.00.

In introducing *Nominating the President*, Lee Sigelman argues that political scientists often focus on the presidential general election "to the virtual—or, in a few cases, total—exclusion of the most time-consuming, expensive, and formative portion of the campaign" (p. xiv). His book with Emmett H. Buell, Jr., he explains, is an attempt to "rebalance the scales through a sustained examination of the politics of the presidential nomination process" (ibid.).

Although Sigelman exaggerates how much scholars have ignored the nominating process (as the chapter references and the "Selected Bibliography" reveal), the book provides an important contribution to understanding the nominating process in general and the 1988 campaigns in particular. And Barbara Norrander, who is both a contributor to the Sigelman and Buell collection and the author of *Super Tuesday*, provides what will probably be the definitive analysis of the attempt by Southern and border states to influence the nominating process by coordinating their primaries on a single day, 8 March, 1988.

Nominating the President contains nine chapters. Buell and James W. Davis analyze the strategic environment of the 1988 nomination race and provide an especially valuable analysis of the Republican and Democratic delegate selection rules. Alan I. Abramowitz, Ronald B. Rapoport, and Walter J. Stone employ their own mail-back survey of Democratic and Republican participants in the 1988 Iowa caucuses and demonstrate the extent to which the Iowa campaign is based upon personal contacts between candidates and their organizations and the caucus participants. Norrander studies voting behavior in the 1988 Super Tuesday, mainly by analyzing the National Election Study's Super Tuesday survey.

Clyde Wilcox provides a detailed analysis of campaign fund-raising and spending in the 1988 contest but acknowledges that it is difficult to pinpoint the effects of campaign spending without an extensive analysis of state polling data and free media coverage. Kenneth D. Wald discusses the importance of religious appeals by both Pat Robertson and Jesse Jackson, arguing that their campaigns demonstrate the permeability of the political parties, the strength of moralistic appeals, the importance of churches as a base of political action, and the diversity of religious appeals in America. Buell studies press coverage of the candidates through a content analysis of campaign coverage in the *Atlanta Constitution*, the *Boston Globe*, the *Chicago Tribune*, the *Los Angeles Times*, and the *New York Times*. Despite extensive "horse race" coverage, these papers provided readers with substantial information about the policy positions of the candidates. Michael J. Robinson and S. Robert Lichter, on the other hand, study television network coverage, and find that "horse race" coverage prevailed over news about the candidates' issue positions.

Buell and John S. Jackson III provide an interesting history of presidential nominating conventions, arguing that although conventions no longer perform the function of actually nominating presidential candidates, they do perform important secondary functions, mainly by providing an opportunity to unify the party before the general election campaign. Finally, W. Wayne Shannon provides a highly critical appraisal of the current nominating system. Setting forth five criteria for evaluating the nominating process, Shannon concludes that most of them are not met and that "the system is *inherently* subversive of party unity" (p. 264). Although Shannon refers to earlier chapters on media coverage, his harsh assessment of the nomination system does not seem warranted by the eight earlier chapters.

Nominating the President is an important collection that contains a great deal of material interesting to students of candidate strategies, media coverage, and the history of the nomination process. There are two shortcomings, however. First, a book focusing mainly on the 1988 nominating process should have included a chapter on the New Hampshire primaries. New Hampshire was the crucial turning point in the Republican contest. Robert Dole's win in the Iowa caucuses, along with George Bush's third-place finish, clearly led to a bounce for Dole; and he actually gained a lead over Bush in New Hampshire Gallup polls. Bush's skillful effort to turn back Dole was crucial to ending Dole's momentum. Second, the editors could have provided a concluding chapter that tied together the earlier chapters, pointing to lessons about the nominating process. Although Shannon's final chapter does draw to some extent upon the earlier chapters, Buell and Sigelman could have provided a more balanced assessment.

Of all the events of the 1988 nominating contest, Super Tuesday was the most interesting innovation. On 8 March 1988, three weeks after the New Hampshire primaries, both the Democratic and Republican parties held primaries in 16 states. Of the 11 states of the old Confederacy, 10 (all but South Carolina) held primaries, as did the border states of Kentucky, Maryland, Missouri, and Oklahoma. Two New England states, Massachusetts and Rhode Island, also held primaries. Five states held Democratic caucuses (including Texas, which held both a primary and caucus). Washington state held both Democratic and Republican caucuses. Although there had been a modest version of Super Tuesday in 1984, Super Tuesday 1988 selected far more delegates on a single day than any previous day in the history of presidential primaries.

Norrander's *Super Tuesday* provides a comprehensive analysis of these primaries, focusing on the impact of the coordinated efforts to create a regional primary. She explains the reasons that southern Democratic leaders wanted to establish a common primary date relatively early during the delegate selection season and also examines Republican hopes that Super Tuesday would backfire on the Democrats by persuading Southern Democrats to participate in Republican primaries. For the most part, she shows, the expectations of party leaders were not fulfilled. Super Tuesday disappointed Southern Democrats by failing to produce a moderate Democratic nominee who could compete effectively in the South. It had little effect on the type of candidates who ran, primary turnout, or the rate at which candidates secured the nomination. Super Tuesday did not fill Republican hopes of inducing Democrats to participate in Republican primaries. On balance, Super Tuesday "did not change the basic process by which candidates captured the Democratic and Republican nominations" (p. 179). In her analyses, Norrander supports her conclusions by drawing upon a wide variety of data sources,

including campaign spending records, aggregate election statistics, and public opinion polls, including the National Election Study's Super Tuesday survey. In addition to providing an excellent analysis of candidate strategies, Norrander makes an important contribution to the study of campaign momentum by arguing that scholars should pay greater attention to the process of candidate attrition.

It is a tribute to Norrander's scholarship that she reaches rather modest conclusions about the impact of Super Tuesday. Authors often tend to exaggerate the importance of their subject matter, but Norrander resists this temptation. If anything, she may somewhat underestimate the impact of Super Tuesday on the outcome of the Democratic contest. She maintains that "Super Tuesday did not decide the Democratic delegate race, but it did establish the three major players for the last half of the primary season" (p. 175). She later concludes that although Super Tuesday helped Bush bring the Republican contest to a rapid close, "in the more open Democratic nomination fight, Super Tuesday made Dukakis's victory more difficult" (p. 188). I would argue that Dukakis' limited successes in Super Tuesday were almost as important for his ultimate victory as Bush's sweep of the Super Tuesday primaries was for Bush's victory. Super Tuesday helped established Dukakis as the only white Democrat with national support; and although he did not eliminate Albert Gore until the New York primary on 19 April, his Super Tuesday wins were crucial in establishing his front-runner status. Questions of interpretation aside, Norrander's book is a valuable contribution to our understanding of the presidential nomination process.

Michigan State University PAUL R. ABRAMSON

The Technology Pork Barrel. By Linda R. Cohen and Roger G. Noll. Washington: Brookings Institution, 1991. 400p. $36.95 cloth, $16.95 paper.

The federal government's support of research and development programs designed to support commercial applications has had episodic visibility, inconsistent funding, uncertain political support, and little agreement about its success. Market failure, national interest, risk sharing, and the need for large amounts of money up front are the primary rationales used for the federal government's involvement in research and development programs that seek to convert a promising technology into a commercially viable form. As Cohen and Noll note, this mixture of politics, economics, and technology has combined in ways that produce both research and development successes and failures. Their analysis is directed at determining the factors that characterize successful programs, and they intend for their recommendations to provide guidance for executive branch and congressional policymakers.

A central weakness in the book is the authors' uncertainty about what standard to apply in evaluating the effectiveness of federally funded commercial research and development. The authors initially and correctly argue that public sector funding of research and development with commercial applications is distinctly different from private sector funding or management of research and development. Unfortunately, a large portion of the authors' analysis rests upon literature-based comparisons of private efforts. Given the authors' own distinctions, a more appropriate research design would have primarily focused on past successful and unsuccessful federal research and development commercial applications programs to derive evaluative criteria that could be used in the analysis of the six case studies.

The analysis suffers when it compares complex federal research and development programs with the theoretical perfection possible when private firms make research and development investments in commercial applications. This perspective is unfortunate, since one of the key features of the real-world private sector is the very high level of failure both for small start-up firms and long-established firms.

The authors show an appropriate and realistic understanding of the strengths and weakness of relying on six case studies. They do not, however, give sufficient attention to what universe their set of six cases represents. The selection criterion used, in part, by the authors appears to be an interest "to minimize overlap with two previous collections of case studies" (p. 77).

Particularly because the authors evaluate the set of six cases as containing three failures, two mixed instances, and one unambiguous success, the reader needs to have confidence about how these cases were selected. The authors' conclusion, however, that the results of their analysis "obviously justify skepticism about the wisdom of government programs that seek to bring new technologies to commercial practice" (p. 365), may be overdrawn. A review of their own data reveals a success rate of 13%–50% for high-risk projects that the private sector had shunned. This suggests that the process works fairly well. Especially in an economic world where 90% of all new businesses fail within five years and only a fraction of large businesses retain their position on the Fortune 500 list for more than a decade, these success rates are significant.

The analysis gets confusing at times when the authors are inconsistent in their categorizations, for example, in one place identifying one success, two mixed cases and three failures (p. 78) and in another passage listing one success, one mixed, and four failures (p. 365).

There is always a tension in a book of case studies about where and when to draw conclusions. Cohen and Noll draw theirs in several places, causing some confusion because the reader is uncertain whether it is a conclusion or a standard against which the case studies are to be evaluated.

A potentially important contribution made is the authors' attempt to determine when information is revealed that determines a research and development project's success and whether the information is provided in a timely fashion to political decision makers. Each of the six case studies gamely addresses this, but clear conclusions are hard to draw.

A second useful contribution developed is the concept of building demonstration projects earlier than needed so that technical or economic failures can be converted into short-term political successes by giving proponents "brick and mortar" and local payrolls to point to as evidence of success.

The authors' idea of creating a commercial research and development agency and congressional committee seems unworkable and likely to become one more level of unneeded bureaucracy.

Cohen and Noll begin their analysis auspiciously by acknowledging the high risk the government under-

takes when it funds and manages research and development commercial application projects that are too difficult for the private sector. In many ways the flaw in their analysis is analogous to trying to compare Small Business Administration success rates to those achieved by commercial banks when only those rejected by banks use the Small Business Administration. Cohen and Noll have taken on a difficult task and have been most successful in their analysis when their criteria are drawn only from other governmental research and development programs. Private sector research and development theories and frameworks are useful for background purposes, but not for evaluating federal research and development projects. Given the increased interest in this area, policymakers may find this book useful in identifying the political, economic, and technical complexities inherent in any large-scale federal research and development commercial applications project.

Ohio State University RAYMOND W. LAWTON

The Presidential Odyssey of John Glenn. By Richard F. Fenno, Jr. Washington: Congressional Quarterly, 1990. 302p. $19.95.

When Incumbency Fails: The Senate Career of Mark Andrews. By Richard F. Fenno, Jr. Washington: Congressional Quarterly, 1992. 309p. $19.95 paper.

As political scientists and political observers, we owe a continuously accruing debt to Dick Fenno for his skills as a scholar, careful researcher, incredibly perceptive observer, and keen analyst of the institutions, processes, and people filling the American political scene. These two books are the second and fifth in Fenno's series on U.S. senators. When Fenno began his extensive study of the Senate, he chose to concentrate on a few senators, studying, among other things, the relationship between their work in Washington and their experiences in their home states. In so doing, Fenno undoubtedly sought to produce a companion piece to *Home Style*, his remarkably acute analysis of how members of the House of Representatives relate to their constituents and how those relationships impinge on the work of the institution. But political events interceded.

These events demonstrated that Senate careers are not quite so predictable as House careers. In 1984, John Glenn ran for the presidency. His career and his relationship with the Ohioans who sent him to the Senate took a turn that, while not unusual for a sitting Senator, strayed from the norm. In 1986, Mark Andrews, another object of Fenno's attention, was upset in his bid for reelection and another pattern for study emerged. Finally, in 1988, a third of Fenno's subjects, Dan Quayle, was tapped as the Republican party's vice-presidential nominee. While Fenno's sample of senators was diminishing, he also found himself in the unique position of having a manuscript on a largely unknown vice-presidential nominee all but completed. *The Making of a Senator: Dan Quayle* was quickly produced and recognized as the definitive (and at times sympathetic) study of the rise to prominence and Senate career of a politician often abused and made the butt of jokes by less careful analysts.

One result of these twists and turns of political events was Fenno's decision to write individual accounts of various senatorial careers, rather than synthesizing the material into one larger work. The two books under review constitute an interesting subset of Fenno's work, the "loser's bracket." In these works Fenno attacks a most interesting question: Why do successful politicians sometimes run notably unsuccessful, and even losing, campaigns?

The Presidential Odyssey of John Glenn looks at the 1984 presidential campaign from one narrow perspective, that of one of the losing nominees for the Democratic nomination. Fenno's method is particularly appropriate for this study. Fenno is the consummate observer, the "soaker and poker." But "soaking and poking" is not random observation. Fenno observes individuals, interactions, and events in context. In this case, the individual was a very public figure, a career Marine Corps officer, a daring test pilot, an astronaut who became an American hero, a losing—and then a winning—candidate for the U.S. Senate, and a nine-year veteran of Senate politics and maneuvering. The context was the most open and public event in American politics, a presidential campaign.

Fenno's observations began with our discipline's accumulated wisdom on how presidential campaigns are run. He applied to that what he had learned about John Glenn the individual from a careful study of all aspects of Glenn's career, then observed Glenn in the presidential campaign. His conclusions are that some of the problems Glenn faced were predictable from a study of his earlier campaigns and from statements of his own views of public service; others followed directly from poor campaign decisions. But in the final analysis, the failure of Glenn's presidential odyssey was a failure to understand the difference in the constituency for a series of Democratic presidential caucuses and primaries and the constituency that is comfortable having John Glenn represent it in the U.S. Senate.

Fenno is most persuasive in arguing that John Glenn has a view of public service—and of his own role as an American hero—that precluded him from reaching out to constituencies, from giving those with particular stakes in the nominating contest a reason for voting for him, from becoming a candidate who evoked passionate responses. Fenno's is a sympathetic analysis: John Glenn never quite figured out what was needed to obtain the nomination; he felt that the public should want him and that he would serve, but not that he needed to convince voters to believe in him wholeheartedly. Nationwide contests are different from those in any single state, even a large one that is in many ways a microcosm of the nation. John Glenn had a certain appeal; but neither he nor his campaign managers (largely strangers who were attracted to his candidacy because it looked like a winning one) ever understood how to convert his very appealing qualities into a winning candidacy.

The loss by John Glenn, the successful Ohio vote-getter, can be explained by his failure to adapt to a new political environment. But what of Mark Andrews? Fenno writes of a "campaigning–governing–campaigning" sequence that is typical of all legislators. The puzzle in this case is how a successful political career could suddenly turn for the worse. Andrews had represented North Dakota in the Congress for more than two decades. Four times, after the state lost its second seat in the House following the 1970 census, he won his House seat in statewide elections. In 1980 he won election to the Senate with over 70% of the vote. Yet he lost in 1986.

Should not Senate incumbents in very small states be as safe as House incumbents? Should not this safety have been especially in evidence for someone like Andrews, who, in Fenno's terms (chap. 2), adapted the governing style of a "House member in the Senate"?

But Fenno's skillful observations and careful analysis demonstrate differences. Andrews worried about Byron Dorgan as a possible opponent and adapted his style somewhat. He thought about a national constituency and adapted somewhat again. But mostly, events—Andrews's explanation of a critical farm vote and filing of a controversial malpractice suit against prominent local doctors—changed the relationship between Andrews and his constituents. Fenno observed Andrews talking to small groups of people. The group gathering over coffee in the little town of Lehr was a typical example. And Fenno saw that the dynamic between Andrews and the locals changed, but Andrews never came to understand that. He not only lost touch with constituents, but he failed to recognize that they saw him differently. Thus, he lost.

These two books tell important lessons about the relationship between politicians and their constituents. They lead us to focus on politicians' perceptions of that relationship, of the role self-deception plays in political defeat. That is a lesson all losers who can reflect objectively on their losses know all too well. Dick Fenno clearly respects and likes the subjects of his studies; some may feel he has become too close to them. But the genius of his work, in my view, is his ability to reflect those positive personal evaluations while still maintaining his stance as an objective and critical analyst, noting lessons about the political process on which we can all draw with profit. A careful reading of the corpus of Fenno's work on the Senate, in these books and the other three, reveals a wealth of insights that add significantly to our understanding of the careers of senators and how the interplay of their work in Washington and their relationships with their constituents affect the life of the Senate and the working of the legislative process.

Colby College L. SANDY MAISEL

Learning to Legislate: The Senate Education of Arlen Specter. By Richard F. Fenno Jr. (Washington: Congressional Quarterly, 1991). 192p. $28.95 cloth, $19.95 paper.

Forging Legislation. By Paul C. Light. New York: Norton, 1991. 273p. $22.95 cloth, $8.95 paper.

When all is taken into account, lawmaking—the enactment of public policy—is the bottom line on the congressional balance sheet. Citizens want Congress to make policy; the legislators want to enter their pet proposals in the statutory ledger. These two works offer complementary insights into the lawmaking process. Paul Light focuses on a single bill, the Department of Veterans Affairs Act, while Richard Fenno approaches policymaking from the perspective of the individual legislator, Republican Senator Arlen Specter of Pennsylvania. Both are "participant observation" studies. Light served as a professional staff member of the Senate Governmental Affairs Committee (John Glenn, D–OH, chair); Fenno's study, another installment of his seminal analyses of senatorial behavior, reflects his travels and conversations with Specter and his staff. Not surprisingly, the conclusions of the two studies converge to offer interesting insights about Congress's assets and liabilities as a formulator of public policy.

Light's book, seemingly aimed primarily at undergraduate students, is a more or less conventional case study of "how a bill becomes a law." It provides a thorough (if a bit verbose—undergraduates may find the many details tough going) overview of an 11-step legislative process (pp. 241–46) from agenda-setting to final passage. Such theorizing as Light attempts appears in a short appendix, where he offers a typology of six senatorial "decision-making styles": rational actor, university teacher (pursuing "the imperfect search for truth"), business tycoon (playing the policy entrepreneur), medieval warrior (seeking to exercise "raw political power" in search of "total victory"), small-town neighbor (trading favors and making decisions through "friendly give-and-take"), and garbage collector (using a bill to "carry other issues that would never pass on their own").

The styles vary both across issues and over the various stages of the legislative process. For instance, the garbage collector role was the "dominant style" at the fact-finding and floor-amending steps of the Veterans Affairs Act but was largely irrelevant during consideration of the Presidential Transitions Effectiveness Act (another Governmental Affairs Committee bill on which Light worked). The styles tax different senatorial resources. For example, rational actors must invest substantial time and energy but little political capital, while the small-town neighbor does not need to invest either (to any significant degree) in seeking policy influence. There are equivalent variations with respect to the needs for information and expertise. Academic students of Congress will find the appendix, containing this set of general theoretical notions, thought-provoking but will be disappointed in the paucity of significant details about their measurement and significance.

Fenno starts from his now-familiar "campaigning–adjustment–governing–campaigning" career sequence. He finds that Specter's "reliance on aggressive, independent, individual effort" (p. 74) characterized his initial election, his adjustment to Senate service, and his subsequent lawmaking activities (esp. passage of the 1984 Armed Career Criminal Act). Fenno highlights the tension between member individualism (heightened, in Specter's case, by his prelegislative experience as a prosecutor and his less-than-comfortable relationship with the Pennsylvania Republican party establishment) and the collective (or "communitarian") effort essential to forge coalitions to enact legislation. While, as always, it is prudent to be cautious about generalizations that derive from a single case, Fenno's analysis, especially when read in conjunction with his treatments, in other works, of John Glenn, Pete Domenici (R–NM), Dan Quayle (R–IN), and Mark Andrews (R–ND) underscores his fundamental proposition that extralegislative considerations profoundly influence lawmaking performance in the Senate.

More specifically, some significant themes emerge from the two studies. For one thing, individual member calculations loom large in congressional deliberations. Light points to John Glenn's desire to leave his mark on what was otherwise a symbolic bill elevating the Veterans Administration to cabinet rank. The Ohio Democrat was unwilling to give the veterans' "iron triangle" its way or simply to move ahead a bill the House had enacted. Rather, he was determined to redress what he

saw as an inequity: the inability of veterans who were denied benefits to sue for redress. While Glenn hoped that judicial review would force the Veterans Administration to improve an admittedly ineffective claims process, veterans groups feared that recourse to the courts would undercut their cozy connection to the Veterans Administration bureaucracy. In addition, Glenn's flirtation with his party's vice-presidential nomination diverted his attention from his lawmaking efforts.

Fenno makes a similar point. Specter's approach, his self-confident, individualistic orientation to legislating, shaped his efforts to enact his career criminal bill. He sought to solicit support from the Reagan White House, as well as his congressional colleagues mostly through personal lobbying, one-on-one, rather than through the cooperative action needed to assemble a winning legislative coalition. His inclinations (perhaps rooted in his personality) gave form to his legislative strategy as they did to his electoral strategy, impeding, Fenno implies, his capacity to advance his legislation easily. In any case, members' motivations and predilections impinge directly on their policymaking activities.

Second, coalition-building efforts are central to congressional policymaking. As suggested, Specter learned only slowly about the need to pursue collective strategies. It took him six months of personal struggle to get a slim three-member majority on his five-member Judiciary subcommittee. His individualism led to uncertain relationships with the full committee chair, Strom Thurmond (R–SC), and the full committee staff. He eventually realized the need to compromise and he accepted amendments that altered but advanced the bill. He agreed to a logroll with Representative William Hughes (D–NJ), chair of the House Judiciary Subcommittee on Crime, attaching the latter's pet proposal to the Senate crime bill as the price for getting his provisions into the House legislation, through the full committee, and passed on the House floor. His career criminal provisions survived in conference only to go down to defeat when Ronald Reagan vetoed the crime package. Specter persevered, however, and in 1984 saw his bill, tailored to meet congressional and executive complaints, signed into law. The arduous battle, while successful, illustrates the limits on individualistic effort: Specter's "learning was . . . constrained by the stylistic preferences he had brought with him to the Senate" (p. 125).

The Veterans Administration cabinet-status bill teaches a similar lesson. To make a nonincremental impact on veterans affairs, Glenn had to maneuver adroitly among the veterans groups, which opposed linking judicial review of denied benefits to cabinet status; Senator Alan Cranston (D–CA), who favored judicial review but opposed linkage for fear it would sink cabinet status; and Senator Alan Simpson (R–WY), who insisted on linkage. He had to play hardball to overcome the opposition of House Veterans Affairs Chair, G. V. ("Sonny") Montgomery, who, as a part of the Veterans Administration subgovernment, was against judicial review in principle. In conference, Glenn won clear victories on only three of his six "must" provisions. He agreed to drop the court review from the cabinet-status bill; but when Cranston put a "hold" on the conference committee report on that legislation, Montgomery was forced to accept a separate judicial review bill (which he had been able to thwart on four previous occasions) to protect Veterans Administration cabinet status.

A third moral that emerges from these stories is the critical centrality of staff. Fenno refers to the "Specter enterprise, . . . the senator and his closest aides" (p. viii). Specter's staffers played important parts in setting the agenda for crime legislation; in managing hearings intended to promote interest in, and support for, the career criminal act; and in dealing with the House Judiciary Committee and its staff. Glenn's staff contributed in similar ways. In addition, it drafted statements for its principal. Interestingly, it prepared two statements—one for, and one other against, linkage—because the senator's position on the matter was uncertain. (Glenn read the prolinkage speech.) For the conference committee, the staffers did the lion's share of the work. Together with their House counterparts, they forged the agreements that comprised the language of the bill; Glenn and his opposite number, Representative Jack Brooks (D–TX), met and largely ratified staff suggestions. There never was a "full dress . . . meeting" of the conference (p. 219). Given the multiple pursuits of elected legislators, the staff plays a distinct and often unappreciated role with regard to congressional lawmaking.

Policymaking, as these books portray it, is nothing if not a messy business. While more theoretically inclined scholars would prefer hard-and-fast generalizations, they may have to settle for broad tendencies. Members matter; for their concerns—electoral requisites outside Congress and policy preferences—and decision-making styles give form to what they do and how they act. Term limits or other changes that would bring different sorts of legislators to Congress might produce quite different policy outcomes than does the present legislature. Coalitions count; for in a decentralized institution, even on relatively minor issues such as those Light and Fenno treat, allies are essential and compromise is the order of the day. More centralized, more disciplined political parties would surely change the character of both the process and the content of congressional decision making. Staff is significant; for hard-pressed lawmakers need and use all the help they can get. To reduce staff availability, as some contemporary reformers propose, might well alter congressional policymaking capacity significantly. Senators and representatives, in short, act in an extraordinarily complex environment. These works suggest that unless scholars understand the range of forces that play on the legislators, they may miss much of the subtlety and nuance that help explain Congress' lawmaking assets and liabilities.

Indiana University LEROY N. RIESELBACH

Toward a Usable Past: Liberty under State Constitutions. Edited by Paul Finkelman and Stephen E. Gottlieb. Athens: University of Georgia Press, 1991. 448p. $45.00.

Among the most noteworthy legal developments of the past two decades has been the rediscovery of state constitutions by state judges and legal scholars. The appointment of Warren Burger as chief justice, which seemed to signal a conservative shift on the U.S. Supreme Court, encouraged some state courts to attempt to insulate their rights-affirming rulings from Supreme Court review by basing them on state declarations of rights. Most proponents of this "new judicial federal-

ism" quickly recognized, however, that an opportunistic reliance on state constitutions could not succeed. If they wanted to develop an independent body of state civil liberties law, they would have to ground it in the distinctive language, principles, and history of state constitutions.

This has proved a daunting task. In interpreting the U.S. Constitution, one can draw upon a rich historical record, an abundance of legal precedent, and a wealth of scholarly commentary. In contrast, state constitutional interpreters have often found the historical record scanty, the precedents scattered or unhelpful, and the legal commentary virtually nonexistent. This collection of essays, written primarily by historians and legal scholars, originated as an attempt to alleviate these problems.

This is not to say that this volume promotes a particular constitutional agenda. Morton Horwitz cautions his fellow contributors about "roaming through history looking for one's friends" (p. 148); but the warning seems unnecessary. If anything, professional suspicion of "law office history" leads to an excess of caution. *Toward a Usable Past* will disappoint readers seeking definitive interpretations of state constitutional provisions. However, there are other uses of the past.

The narrowest function of constitutional history is to provide a context for understanding important cases. Several essays in this volume serve this function. Mary Dudziak traces the controversy over birth control in Connecticut prior to *Griswold*, arguing that the state's ban on the distribution and use of contraceptives indeed limited their availability. Morgan Kousser chronicles legal developments regarding segregation in Louisiana prior to *Plessy* and in Kansas prior to *Brown*. Charles Ogletree examines the treatment of the Supreme Court's jury discrimination cases in state courts. While these case studies have some interest, they are not theoretically grounded and do not permit generalizations.

A second function of constitutional history is to clarify changes in constitutional interpretation over time. The essays in this volume focus less on substantive changes than on changes in interpretive approaches. Although state judges have, since the mid–nineteenth century, emphasized textualism and "strict construction" in interpreting state constitutions, three essays reveal a wider range of interpretive traditions in the states. Ellen Peters demonstrates that earlier judges looked to the common law to clarify constitutional meaning, and Suzanna Sherry shows how judges employed standards of "natural justice" in protecting rights. H. Jefferson Powell analyzes the different understandings of judicial review among Virginia judges a decade before *Marbury*.

Perhaps the most valuable function of constitutional history, however, is to illuminate how changes in legal culture have affected the substance and interpretation of constitutions. The several essays analyzing state constitutions in terms of legal and political culture are among the strongest in the volume. Donald Lutz, James Henretta, and Peter Onuf examine the influence of republicanism on state constitutions. Their emphasis on political participation and on the inculcation of virtue underscores the distinctiveness of state constitutional politics for almost a century after independence. Lawrence Friedman's excellent essay analyzes how a concern about "the dangerous classes" during the late nineteenth century influenced the interpretation of state criminal justice guarantees and discouraged the development of new protections for the rights of the accused.

The danger with an edited collection, however, is that some important questions will not be directly addressed by any of the authors. That seems to have occurred in this volume. Most of the essays assume without examination that the similarities in state constitutional development are more important than the differences. Although some essays recognize that the constitutional agenda in the states during the nation's first century differed from the federal constitutional agenda, no essay systematically considers whether such differences continue to exist. Finally, no essay considers whether constitutional history can provide a "usable past" for state constitutional interpreters.

Nevertheless, *Toward a Usable Past* is an important book. The first volume devoted to state constitutional history in recent years, it demonstrates both the importance of the enterprise and how much work remains to be done.

Rutgers University, Camden G. ALAN TARR

Gaining Access: Congress and the Farm Lobby, 1919–1981. By John Mark Hansen. Chicago: University of Chicago Press, 1991. 265p. $38.00 cloth, $15.95 paper.

To what extent and under what conditions do interest groups influence congressional policymaking? Probably no other question elicits as much fascination and consternation from the American public and scholars of American politics. John Mark Hansen's first book, *Gaining Access*, develops and illustrates a theory of access to account for the variable influence of farm lobbies on agricultural policymaking in the U.S. Congress from 1919–1981.

In general, "interest group access results from congressional strategies for dealing with electoral uncertainty" (p. 5). Assuming that reelection is the primary goal of lawmakers, Hansen treats interest group access to the policymaking process as a decision that legislators make in order to obtain the information necessary to secure that goal.

The influence of lobbyists is a function of the degree to which legislators grant lobbyists both access and privileged consideration to the policymaking process. Access is granted (1) "when legislators judge that interest groups enjoy competitive advantage over rival informants" and (2) "when they expect the conditions that maintain competitive advantage to recur" (p. 215). Used repeatedly throughout the work, the term, *competitive advantage*, means "an interpretation of the advice that helped legislators most in their past election," while *recurrence* means "a prediction that the advice will help legislators in their future elections" (p. 6). Access is thus something decided by and for political elites (p. 3).

Though interest groups compete with parties and policy entrepreneurs for access to members of Congress, they have a number of advantages in this struggle for congressional consideration: "First, they provide information and propaganda more efficiently than their rivals. Second, they promote the electoral aims of their congressional clients more effectively than their competitors" (p. 103). As to the classic trade-off between parties and interest groups, Hansen concludes that "strengthening political parties will not overcome the biases and

inefficiencies of pressure politics," since "parties cannot absorb the social conflicts that otherwise find their outlets in the pressure system" (pp. 226–27).

Hansen's is not however simply a theory of interest group access. Rather, it is a more general theory of how access to congressional policymaking is determined and influence achieved by any individual or group seeking to bring a demand for authoritative action to the attention of the governing regime. This is a key strength to the book, even if its implications, especially for normative democratic theory, are not fully explored by the author.

Gaining Access is not without a few shortcomings. To begin, some scholars may find Hansen's theory difficult (though not impossible) to falsify. Since there are few objective measures of competitive advantage or recurrence, the reader must accept the author's interpretation of how both were interpreted by lawmakers at the relevant time. If lawmakers continued to grant access because they were misinterpreting the competitive advantage of farm lobbies and the recurrence of farm lobby issues, there's no way to know if access resulted from a misinterpretation on the part of lawmakers or from the traditional "iron triangle" explanations of agricultural policy.

The explanatory power of Hansen's theory of access is ultimately dependent upon a theory of macro agenda building to explain why certain issues become and remain salient for government officials and the polity. Patterns of access, observes Hansen, change as (1) issue salience changes, (2) the contentiousness of issues changes, and (3) elections prove informants right or wrong (p. 105). Beyond noting the "circumstances that create competitive advantage" and "guarantee recurrence," such as economic development, industrialization, population growth, and migration (p. 102), a systematic explanation of how the macro political agenda is created and how it shapes the policymaking process is not provided, limiting the theory to explanations based on the micro electoral calculations of incumbent politicians.

The theory's applicability is also limited by its arena of derivation. Many important studies of interest group politics have been based upon the politics of agricultural policymaking. Yet policies and the politics they engendered are not created equal. Notwithstanding Hansen's illustration that the theory is applicable across time and space to other issue arenas (pp. 217–22), agricultural policy may be distinctive as an election issue, providing greater empirical support for the theory than would another policy. Not only may policies determine politics within the governing apparatus of the state (to borrow Theodore Lowi's influential observation), but certain policies, like agriculture, may better solidify the electoral connection that links interest groups to lawmakers than do others.

Finally, Hansen's theory "works" so long as its underlying assumption, that members of Congress are "single-minded seekers of reelection," is the prevailing norm. Challenge this assumption, and the foundations of the theory begin to crumble. Lawmakers not primarily motivated by reelection may grant access to address important exigencies, rather than to "be on the right side of an issue in the next election" (p. 17). Studies increasingly show that reelection may not be the primary motive of national legislators. The level of public-spirited behavior is important for understanding the results of American policymaking, as are values and ideas. These may be more determinative of policy outcomes than the self-interested pursuit of reelection.

Notwithstanding these criticisms, *Gaining Access* should be taken very seriously as an informative and theoretically suggestive contribution to the study of congressional policymaking and the politics of interests. Hansen's theory of access will become a familiar feature in future studies of interest group politics, and also merits attention in studies of political parties, policy entrepreneurship, social movements, and political consultancy. The theory's widespread diffusion will establish the boundaries of its applicability as it continues to expand our knowledge of how political power is distributed in the American regime.

University of California, Irvine MARK P. PETRACCA

Is Anyone Responsible? How Television Frames Political Issues. By Shanto Iyengar. Chicago: University of Chicago Press, 1991. 195p. $19.95.

This book extends Iyengar's earlier work with Donald Kinder, *News That Matters* (1987), which explored the agenda-setting role of television news. The first book refined the work of McCombs, Shaw, and others and broadened it to include framing effects. The concept of framing refers to "subtle alterations in the statement or presentation of judgment and choice problems, and the term 'framing effect' refers to changes in decision outcomes resulting from these alterations" (p. 11).

The research reported in *Is Anyone Responsible?* examines two ways in which television news frames issues: the episodic and the thematic. The episodic frame is the more prevalent one. It ordinarily takes the form of an event-based news report. An example would be the depiction of the terrorism issue in the context of an Irish Republican Army bombing in Northern Ireland. The thematic frame provides a broader perspective; it reports the issue in the context of "collective outcomes, public policy debates, or historical trends" (p. 18). An example would be a news story that discusses the terrorism issue against the backdrop of the historical bitterness between Northern Ireland's Protestants and Catholics.

On the basis of experimental research, Iyengar concludes that episodic framing on television encourages viewers to assign the blame for society's problems to individuals, rather than to social and political institutions, such as Congress or political parties. Thematic framing is concluded to have the opposite effect. Because television news emphasizes episodic framing, says Iyengar, it deflects the blame for problems from government, resulting in a weakening of political accountability—hence the book's title.

Iyengar's experiments were based on paired groups of participants: one group was shown a videotape that included an episodic news report on a particular issue problem and the second group was shown a thematic report on the same problem. The issues of crime, terrorism, poverty, unemployment, racial inequality, and the Iran–Contra affair were included in the experiments. After their exposure to a twenty-minute videotape, which contained seven news stories, including the story that was subject to experimental manipulation, participants completed a post-test questionnaire that included open-ended questions about the causes and treatment of the problem at issue. Responses were categorized as

societal (e.g., a response that attributed crime to impoverished social conditions) or individualistic and punitive (e.g., a response that attributed crime to the character deficiencies of criminals). A comparison of the episodic treatment group with the thematic treatment group revealed that the episodic group's responses usually contained more individualistic and punitive attributions and fewer societal attributions.

The experimental work was backed by a content analysis of all ABC, CBS, and NBC television news stories on the issues in question during the period 1981–86. The content analysis was based on the Vanderbilt Archive's *Abstracts*. The content analysis indicated that although most news stories include both episodic and thematic material, the majority of stories were skewed in the episodic direction.

Iyengar's conclusions are unequivocally stated. Early in the book, for example, he writes: "While there can be no denying the influence of stable dispositional characteristics or cultural norms on attributions of responsibility for political issues, there is compelling evidence that contextual or circumstantial factors are equally—if not more—important . . . . Today, the most important of these contextual influences is television" (p. 10). In the book's final paragraph, he lays the blame for the erosion of political accountability in recent years squarely on television's preference for episodic framing: "The portrayal of recurring issues as unrelated events prevents the public from cumulating the evidence toward any logical, ultimate consequence . . . . Television news may well prove to be the opiate of American society, propagating a false sense of national well-being" (p. 143).

Iyengar's book has substantial merit, but his conclusions are less careful and exact than the experimental research on which they are based. This type of research requires a healthy respect for the limitations of the experimental method. It would have been astonishing, for example, if Iyengar had not found that his subjects, after having just seen an episodically or thematically framed news report on a particular issue, reiterated aspects of the report when asked about the causes and treatment of the issue. To reason from the fact that viewers do repeat such material to the conclusion that network television is behind Americans' tendency toward individualistic attributions is a giant step. This manifestation is deeply embedded in the American political culture and has been for more than two centuries.

Moreover, although it is true, as Iyengar says, that television news focuses "on concrete acts and breaking events" (p. 14), this form of reporting did not originate with television. This model of journalism, which dates at least back to the 1830s, is the dominant form of newspaper reporting and has been taught in schools of journalism since the early 1900s. In fact, newspaper journalists depend even more heavily on episodic reports than do television journalists. Compared with the newspapers' inverted pyramids, television emphasizes the interpretive news report due to its need for tightly structured stories. If stories are to be readily understood by a listening audience, they cannot be allowed to trail off as a newspaper story may. Accordingly, as Dennis Lowery's content analysis indicated (*Journalism Quarterly* [1971]), television news stories tend to be built around inferential sentences, often of an unattributed nature.

It is this thematic quality of television more than the episodic orientation it shares with print that was the foundation, nearly two decades ago, for Michael Robinson's "video malaise" thesis (*American Political Science Review* [1976]). Robinson sought to explain the same phenomena that intrigued Iyengar, but he reached the opposite conclusion. Both cannot be right. Curiously, Iyengar's book contains no recognition of Robinson's notion of "video malaise" anywhere in the text, the notes, or the bibliography. Perhaps Iyengar's view is the correct one. If it is, the problem is more severe than he suggests. It goes beyond television news to *all* news; a focus on events and episodes is deeply embedded in the practice of American journalism, print and broadcast media alike.

Despite its shortcomings, this book is recommended reading for media scholars. Iyengar's experimental methods offer a precision of measurement that media effects research seldom attains. I believe, moreover, that Iyengar's notion of framing effects is one of the truly important theoretical concepts to appear in recent years. Iyengar's challenge is to balance his noteworthy methods and concepts with an equally measured accounting of the substance of American politics and journalism.

Syracuse University THOMAS E. PATTERSON

On the Man Question: Gender and Civic Virtue in America. By Mark E. Kann. Philadelphia: Temple University Press, 1991. 364p. $39.95.

Emmanuel Le Roy Ladurie once suggested that historians could be divided into parachutists and truffle hunters. Parachutists take a panoramic view as they descend to earth. Truffle hunters root around for details on the ground. Mark Kann is plainly a parachutist. *On the Man Question* presents a sweeping thesis and surveys an enormous scene. Kann promises to investigate nothing less than "the intellectual ferment of Restoration England, analyze John Locke's articulation and anticipation of it, and trace its evolution in eighteenth-century and nineteenth-century America" (p. 32). He discovers a dark "other" liberal tradition in America born of a "misanthropic fear of individualism," involving "a desperate search for father figures" and culminating in a culture of "engendered civic virtue" (pp. ix, 14, 19).

Kann's thesis relies upon three large, related claims. The first asserts the truth of Louis Hartz's theory about a liberal Lockean consensus in America, once civic republicanism and patriarchalism are grafted onto it. This entails the second claim: Locke's own political theory is not merely liberal but is, in fact, a "synthesis of individualism, civic virtue and gender" (p. 114). The third claim follows from the first two and asserts that Locke's political theory created an ideological culture within the American republic that sought to "restrain men's selfishness, women's treachery, and young men's passions" by creating citizen–fathers, civic mothers and patriot–warriors (p. 191). In sum, "the subtext of American history" is a quasi-republican, liberal patriarchal Lockeanism that involved "a single-minded effort to make individualism conditional and compatible with social order" by controlling middle-aged men and manipulating young men and women through the rhetoric of civic virtue (pp. 32, 217). We are asked to read this "subtext" in colonial Puritanism, Revolutionary militia service, Jacksonian expansionism, Civil War soldiering,

Victorian marriages, Theodore Roosevelt, and World War I conscription—to mention only a few.

Since a parachutist's thesis can carry decisive advantages in historical explanation, what matters is whether the apparatus that supports it is sturdy enough to carry it safely to earth. I wish it were so in this case, because Kann intends well. He has turned to gender as a necessary outgrowth of investigations into democracy, radicalism, and the history of ideas. However, I have serious doubts about Kann's three claims. Least convincing is the assumption that Americans consciously "adopted" Locke's political theory. We might expect even a reconstituted version of the Hartz thesis to provide some substantive historical and textual evidence for the notion that Locke's writings were integral to American political consciousness. But Kann offers nothing to ground assertions that "Lockean civil and political rights were etched into the nation's founding" or that Locke "articulated the democratic hopes and liberal fears that eventually wed an American dream to an American demonology" or "authored modern liberal hegemony, and his American heirs extended the copyright" (pp. 217, 300). Some truffle hunting would help. Exactly who were these "American heirs"? Exactly how and when did they etch and wed Locke into their understandings? Exactly how and when did this become a consensus? The evidence Kann needs to support these conclusions is simply absent. Until such evidence is forthcoming, the idea of America as a "Lockean consensus," no matter how amended, is in no better shape than it was when Hartz developed it in the 1950s, especially after three decades of subsequent criticism.

The question of how Locke's theory should be interpreted raises other concerns. By reading Locke as a civic republican as well as a liberal patriarchalist, Kann hopes to appropriate the republican revisionist argument—and at the same time finesse it—by strengthening the Lockean consensus theory. This is an inventive move; but everything depends upon the plausibility of the idea that Locke is, at the same time, a patriarchalist, a civic republican, and a liberal. Unless these categories can mean whatever anyone wants them to mean, the historical, textual, and contextual bases for these judgments are, in fact, extremely weak. For us to believe them, we have to forget the point of Locke's critique of Filmer in particular and of patriarchalism more generally; rewrite Locke's few references to *private* virtues as though they were the civic ones of Machiavelli or Harrington; and transform Locke's doctrine of the individual, natural law, and Calvinist calling into words about "individualism" or "companionate marriages" that only John Stuart Mill could have written. Unfortunately, Kann's tendency to read Locke as contributing to social relations that did not yet exist or as able to "anticipate," "prefigure," and "pave the way" toward nineteenth-century America (p. 174) only furthers the suspicion that much of his textual interpretation lacks sensitivity to context and history. Confidence is not bolstered by such claims as that Locke "anticipated Louis Hartz's early American consensus" (p. 117). This calls to mind Quentin Skinner's warning that interpretation should not be a pack of tricks we play on the dead.

Kann's third, and most promising, claim about the engendering of civic virtue in America thus hangs on some slender threads. This is unfortunate, because the book raises the important point that political theories often function to sustain unequal relations of power and to legitimate specific cultural norms along dimensions of gender. But Kann provides neither a theory of gender nor even an account of how he intends to use gender as a category of analysis. Given all that has been written on gender in feminist theory in recent years (very little of it cited by Kann), it is also somewhat surprising that the complexities introduced by race and ethnicity are absent in his big groupings of women and men, where only men are divided into young and middle-aged. (As for class, there are only occasional references to the middle-class.) Moreover, Kann's particular application of gender to the study of American history focuses almost exclusively upon manly themes: masculinity, fraternity, filial bonds, and the psychohistorical saga of fathers and sons. Women are fit to these themes as wives and mothers. If this captures a certain reading of Locke or of the so-called Lockean consensus, it does not adequately explain gender relations in American history or exhaust the ways in which the female has been culturally and symbolically represented. Thus, the diversity of real historical women in the United States and the range of their actual political activities, as well as the complexity of representations of the feminine and femininity, are in the end simplified and sacrificed to the large claims of this well-intended book.

University of Minnesota MARY G. DIETZ

Beyond Persuasion: Organizational Efficiency and Presidential Power. By Matthew Robert Kerbel. Albany: State University of New York Press, 1991. 214p. $16.95 paper.

This book purports to demonstrate that organizational efficiency plays an important role in presidential power, a wonderfully interesting idea. Using data from media coverage on 4 Carter and 4 Reagan bills and 26 other Johnson, Nixon, and Ford cases, Kerbel correlates news story content (e.g., complaints about inefficiency or compliments concerning persuasiveness) with administration success. For example, on the two Carter successes studied, 86% of the 206 media mentions of Carter's persuasion said that Carter "used" persuasion, while on the two Carter defeats, only 54% of the 226 mentions of persuasion said that Carter "used" it. The Reagan percentages and those for the whole closely resemble these (87% and 53% and 88% and 56%, respectively). From such differences on a number of topics, Kerbel draws substantive conclusions about which elements matter: "Efforts to bargain were particularly intense and effective in advance of instances when these presidents successfully achieved their objectives" (p. 44). The last substantive chapter also assesses a LOGIT model predicting success on the 34 cases using five variables: three news-story-based variables, unemployment, and inflation. The combination of differences and the organizational variable's LOGIT performance led Kerbel to his efficiency conclusion.

While this work promises to realize the oft-heard clamor for more systematic empirical analyses, it falls short. Some problems derive from conceptual difficulties, others, from purely empirical sources. As most critics see it, presidency research has too often depended on secondary sources of information, such as reportorial anecdotes, which too often carry no systematic validity or (worse yet) provide "disinformation." Yet

Kerbel proposes to build an empirical analysis based entirely on reportorial sources. Even the substantive data (as opposed to controls) in the LOGIT derive from news counts.

Kerbel fails to suggest the validity of these data. In 1965, for example, Larry O'Brien gave an interview to the Public Broadcasting System and Congressional Quarterly about administration liaison. O'Brien's principal point was that administration practices rarely involve anything other than providing substantive information or organizational efforts—certainly not "arm twisting." This is very similar to Kerbel's findings. Banking chairman Wright Patman, however, seeing the interview transcript, circled all those statements and sent it to O'Brien with the personal suggestion that he would probably need to "take this along with you to your next confessional." One might imagine that sources would *want* to present *their* version. Communicating with reporters, after all, serves to shape how others perceive. Of course, as with O'Brien and Wright Patman, reality varies distinctly from what is reported.

Even when informants do not plan to generate a particular perception, the reports have no greater validity. For example, one of Kerbel's findings downplays the effectiveness of presidential threats. Consider the bargaining situation. If a president threatens a member or faction and the target gives in, no one reports that success—certainly not the administration and certainly not the "target" who has given in. On the other hand, if the president threatens and that threat turns out ineffective, the target may want to talk, that is, build a reputation for constituency commitment. Without direct observation of administration bargaining, however, news counts produce data with a strong bias to reveal ineffective, rather than effective, threats.

At the center of Kerbel's problem lies a question of which strategic interaction to analyze. One characterization of bargaining describes it as involving unitary actors. Reporters tend to focuses on bargaining at this institutional level—the president versus Congress; so, per force, does Kerbel. But bargaining with the president actually involves individuals. If the administration convinces members to change *from* their predispositions, then it has exercised influence. If it then loses, administration efforts may still reflect well on its competence and influence, given that other variables effect the aggregate outcome. Assessing influence requires understanding starting points, not just outcomes. A better LOGIT could help; but Kerbel analyzes only 34 outcomes and excludes the standard controls familiar to Edwards, Covington, and Bond–Fleisher models, which describe predispositions, such as core support, partisan seats, agenda size, election cycles.

At the institutional level, more detail about the administration's agenda seems essential. We know the public (Congressional Quarterly) agenda differs from the administration's internal agenda. Analyzing issues mostly from the public agenda misrepresents the essential: How much administration commitment? When administrations face losing situations on nonpriority issues, might their best strategy generate a losing fight *of necessity*? If Nixon lacks the numbers, might he pursue a defeat that distinguishes carefully between him and his opponents? Kerbel's operationalized strategic situation, based on reporters' asking why this policy failed, might not appreciate Nixon's strategy. Southern Democrats not privy to the plan but wanting the policy might easily blame administration ineptitude. More detail of the initial strategic situation would improve things. Kerbel's questions ring true. But his considerable sensitivities would have gone farther if he had focused on analyzing internal administration materials drawn from archives, rather than on using the media.

University of North Carolina, Chapel Hill TERRY SULLIVAN

Social Norms and Economic Institutions. Edited by Kenneth J. Koford and Jeffrey B. Miller. Ann Arbor: University of Michigan Press, 1991. 258p. $29.95.

The rational choice analysis of social institutions has significantly advanced our understanding of the role of institutions in social life. The relationship between rational self-interested action and the constraints produced by institutional rules has been the source of a variety of static explanations of institutional effects and dynamic explanations of institutional emergence and change. But an important feature of many social institutions is lacking in most accounts employing the rational choice approach—the effects of social norms and ethical values. Here we confront questions about values that are not reducible to self-interest and rules that constrain choice for nonconsequential reasons.

Social Norms and Economic Institutions, a volume of essays edited by Kenneth J. Koford and Jeffrey B. Miller, presents a variety of attempts to incorporate norms and values into a rational choice framework. The main focus of the essays is economic institutions, but the issues they address and the insights they develop are applicable more broadly to other social institutions. The essays are organized into five categories: a general model of economic organization incorporating norms, an analysis of the role of ethical values in economic decision making, experimental results on the role of norms in simple social interactions, theories of the emergence of social norms, and more applied analyses of the effects of social norms in particular economic settings.

Overall, the essays are thoughtful and represent an intelligent attempt to address the interrelationship among values, norms, and rational action. For this reason alone, the book can be recommended as a point of departure for anyone interested in these important questions. As with any emerging field of study, the positive proposals for understanding this interrelationship are preliminary and open to debate. Such a debate will be furthered by the clarity with which these proposals are presented in the various essays. By sketching briefly some of the arguments in individual essays, I can highlight some of the areas in which I would encourage future analysis and debate. I will not focus here on the essays on either experimental results or evolutionary theories of norm selection, because such literature is probably more familiar to the readers of this journal.

In "Habit, Customs, and Norms in Economics," Koford and Miller offer a general analytical approach to economic activity that incorporates social norms. The authors intend this essay to serve as the framework with which to locate and assess the other contributions to the volume. Thus, it allows us to consider some general questions about the main approach advocated by many of the authors. Koford and Miller conceive of social norms as a subset of the more general categories of

habitual behavior and social custom. The discussion is somewhat imprecise, since several overlapping distinctions among norms, habits, and behavior are offered; but the main force of the approach that they recommend is a claim that norms constrain behavior either unconsciously through custom or consciously through the effort to avoid sanctions for noncompliance.

As a recommendation of a conception of social norms on which to ground future research, this approach raises two questions. First, while the conception does capture some of the ways in which norms affect choice behavior, it underestimates an effect that is central to most sociological accounts: norms establish roles and appropriate standards of behavior. In this way, norms affect conscious behavior: People consciously follow normative prescriptions because it is the appropriate thing to do, not merely because they fear sanctions for noncompliance. Second, are norms merely self-enforcing rules (or regularities in behavior) or are they also laws (with the accompanying force of external sanctions)? Norms are treated both ways in the literature and Koford and Miller might have clarified their position on this point. This makes a difference for both explanations of effects and emergence.

More importantly, Koford and Miller argue that explanations of the role of norms, habits, and custom will be found in the "efficiency properties" of these rules. They do not make the standard mistake of arguing that all social outcomes will be optimal. They even show how certain persistent norms may produce inefficient behavior when external circumstances change. They do, however, rest their entire discussion on the collectively beneficial effects of these rules. Norms simplify social interactions by (1) minimizing the complexity of decision making (limiting the spheres of conscious activity) and (2) increasing the predictability of social interactions (by establishing expectations about the actions of others).

For an approach that rests primarily on individual decision making, the invocation of collectively beneficial functions as the fundamental source of explanation is problematic. The standard result of the prisoner's dilemma analysis emphasizes the tension between individual self-interest and collective benefits. If we are to rely on collective benefits and functional needs to explain norms, then these explanations must rest on mechanisms that are not grounded in rational self-interest. In this essay, the emphasis on functionalist logic (minus such a mechanism) does not sufficiently further our understanding.

"Amending the Economist's 'Rational Egoist' Model To Include Moral Values and Norms" by William B. Griffith and Robert S. Goldfarb—and the accompanying commentaries—present a wide-ranging discussion of the methodological and conceptual issues involved in deciding how to treat values and norms in a rational choice model of individual decision making. Griffith and Goldfarb argue for the importance of incorporating moral values into a model of rational decision making and assess the alternative ways in which that might be done. Given their conception of the effects of norms in terms of the effects of moral values, it is not surprising that they opt for modeling normative effects as preferences, as opposed to constraints on choice.

The commentaries challenge the authors' recommendations on the grounds that (1) the importance of norms for economic analysis is overemphasized and (2) the recommended method of incorporation of norms is open to question. I think that the commentators are wrong on the first front but potentially on to something on the second. While the Griffith–Goldfarb suggestions are illuminating, they are not yet persuasive. The effects of norms as constraints should be given more serious consideration, especially if the suggestion by Dennis Mueller that the most important consideration is empirical testability is to be taken seriously. Also, the tension between individual and collective preferences and values should be given more prominence in the analysis.

The final two essays in the volume, "Corporate Crimes and Innovative Punishments" by Peter A. French and "Bargaining and Contracts" by Jules Coleman, Steven Maser, and Douglas Heckathorn, are highly stimulating efforts to understand the effects of principles of fairness and ethical behavior on economic activity. Both are normative in emphasis. French's piece presents a normative argument in support of a principle of ethical behavior (the *principle of responsive adjustment*) for corporate managers, while the Coleman, Maser, and Heckathorn paper is a normative analysis of what fair bargaining outcomes would look like and which principles of bargaining and enforcement might further them. Future research should definitely include more analyses of this kind that enhance our understanding of the normative consequences of such norms. In addition, we would benefit from similar analyses that apply aspects of the logic developed in this volume to positive explanations of the role of norms and values in economic affairs.

Washington University, St. Louis JACK KNIGHT

The Framers and Fundamental Rights. Edited by Robert A. Licht. Washington: American Enterprise Institute Press, 1991. 194p. $19.95.

That ours is an age grown drunk on the rhetoric of rights is attested to by everything from ideologically brutal Supreme Court confirmation hearings, to the endless articles spewing from the law reviews, to the shelves of books being pumped out to satisfy the requirements for academic tenure and promotion. Talk of rights bombards the public imagination.

Sadly, there tends to be a great deal of poverty amid the plenty, however. Most often, the rhetoric of rights has far more to do with rhetoric than with rights. The great flights of fancy that all too often pass for scholarship are rarely burdened by the weight of history or facts. The result has been, in Mary Ann Glendon's phrase, the impoverishment of political discourse. Thus, it is a major event when a collection of essays as thoughtful as those contained in *The Framers and Fundamental Rights* appears. Our public discourse over the nature and extent of rights will be enriched by this good volume.

The Framers and Fundamental Rights appears as the most recent offering of the American Enterprise Institute to the bicentenaries of the Constitution and Bill of Rights. This, the eleventh volume in the series, is the first not under the judicious editorship of Robert A. Goldwin. But Robert Licht has certainly lived up to the high standards Goldwin established. This collection is a model of intellectual depth, stylistic clarity, and ideological balance. It will repay reading by students and experts alike.

The contributions include Benjamin Barber's "Consti-

tutional Rights—Democratic Instrument or Democratic Obstacle?," Judith Best's "Fundamental Rights and the Structure of the Government," Robert A. Goldwin's "Congressman Madison Proposes Amendments to the Constitution," Lino Graglia's "Constitution and 'Fundamental Rights'," Thomas L. Pangle's "Republicanism and Rights," Mark Tushnet's "*Federalist* and the Institutions of Fundamental Rights," and Michael P. Zuckert's "Thomas Jefferson on Nature and Natural Rights." Taken as a whole, they examine the topic of fundamental rights from nearly every conceivable angle.

None of the these splendid essays is likely to rest comfortably with everyone; there is something here to arouse and antagonize anyone who picks up the volume. Yet it is precisely the ability of the authors to provoke us, each in a different way, that makes *The Framers and Fundamental Rights* so valuable: it makes us think about these important issues in new ways.

Generally, the essays agree on certain points, albeit for different reasons. The authors share the underlying belief that there is something terribly wrong with how we have come to think about fundamental rights. Our times are plagued by what Barber calls our "rights obsessions" (p. 36), a view of rights that has largely denied the propriety of politics in the best sense of the word. "Never has there been so much chatter," writes Pangle, "about 'community', 'bonding', 'empathy', 'nurture', and 'gentleness'; and never has there been so icy and thorough a disconnectedness between women and men, between generations, between fellow-citizens and workers and neighbors" (p. 119). When it comes to rights, talk is cheap—in both senses of the word.

The second point in which the authors find some sturdy common ground is the belief that we have lost sight of the historical foundations of rights—where they came from, what they are, and how best to secure them. Thus, we are tossed about on an allegedly philosophic sea with neither compass nor anchor. When it comes to rights, we have simply lost our bearings. Most of the things spoken of as "rights" are but pale imitations of what earlier and more thoughtful generations understood as being worthy of that hallowed name. In particular, as Graglia points out in what is surely one of the most provocative essays in the bunch, "fundamental rights" in the modern sense are nothing more than "mythical supports of a fraudulent structure built and maintained by America's intellectual elite on the conviction that government with the consent of the governed is undesirable in America" (p. 101).

Together, these essays demand of us a serious reconsideration of where we are and how we got here. They urge a return to the fundamental texts of our liberal tradition not to find the answers so much as to remember the questions we should be asking about fundamental rights. This we can do, first, by considering anew how rights are dependent upon government (Best) and, second, by recalling how our Bill of Rights came into being (Goldwin).

Among these very good essays, two demand to be singled out as at once the most profound and the most provocative. Pangle's survey of the history of republicanism and how America sought to reconcile its demands with rights will administer a wake-up blow to those who have nodded off with visions of civic republicanism dancing through their constitutional heads. In a similar way, Zuckert's excursion through Jefferson's theory of nature and natural rights will deepen any reader's understanding of Jefferson's great contribution to the American notion of fundamental rights. These two essays alone make a reading of *The Framers and Fundamental Rights* important; combined with the other essays, it becomes necessary.

Harvard Law School GARY L. MCDOWELL

Interpreting Tocqueville's "Democracy in America." Edited by Ken Masugi. Savage, MD: Rowman & Littlefield, 1991. 526p. $68.65 cloth, $24.95 paper.

At first Masugi's collection appears eclectic—useful for the breadth of its contributors' concerns but held together only by their common subject, Tocqueville's *Democracy in America*. Topics range from women's status in Tocqueville's, as well as contemporary, America to Tocqueville's use of Jefferson's writings on the corrupting effects of administrative centralization. The essays are grouped into three loose sections, dealing with Tocqueville's place in the history of political philosophy, his analysis of American institutions, and his views concerning American character.

But an important theme holds this fine collection together—Tocqueville's attempt to preserve human greatness in democratic times. Most of the contributors note Tocqueville's worry that democracy may undermine liberty. Several build upon this analysis, fleshing out Tocqueville's concern that liberty serve the higher goal of virtue.

Bruce James Smith points out that for Tocqueville, the "taste" for liberty is aristocratic and hence unnatural to the democratic majority. James Caesar outlines Tocqueville's analysis of the institutions (principally a decentralized press, common law lawyers and juries, and townships) fostering liberty by inculcating character traits necessary for local cooperation and proper respect for tradition (p. 311).

In mutually reenforcing ways, contributors point out that for Tocqueville, liberty (improperly understood) may destroy itself, along with proper character. Delba Winthrop argues that rights claims on government for the fulfillment of "equal" needs encourage a fixation upon material equality, destroying diversity and virtue (p. 419). Only when we recognize that rights are meant to secure personal and communal self-government can we encourage honorable conduct (p. 424).

Peter Augustine Lawler points out that even the desire for honor can be corrupted by democracy. For Tocqueville, democracy asserts and demands equality. But we cannot accept being reduced to indistinguishable elements in the mass of mankind. Intellectuals reclaim their individuality by "standing outside" the humanity they themselves "analyze" into a soulless mass (p. 103). Nonintellectuals seek distinction by deifying the universe—and themselves as part of that god (p. 118). In either case, we lose sight of our mortality and our need to preserve our dignity by combating materialism.

To retain our true individuality, we must replace some of our faith in reason with faith in God. John Adams Wettergreen points out that for Tocqueville, Christianity fostered democracy by asserting our equality before God. Yet if we are to check democratic materialism and our consequent susceptibility to soft despotism, we require Christianity's focus upon eternity and the soul (p. 213).

Ralph Hancock adds that religion's utility, for Tocqueville, arises from its truth. While democrats revel in the supposed indeterminacy of their character, religion shows that we have a universal, limited, human nature, rather than any right to limitless satisfactions (pp. 364–65). Only by accepting our limits can we retain the natural grace of our souls and can both recognition and grace take the form of virtuous action. William Kristol even suggests that by inculcating proper morals, we may reclaim, in Tocquevillean fashion, our religious faith (p. 492).

Not all contributors praise Tocqueville's concern with the soul. Edward Banfield castigates Tocqueville for asserting that good societies depend upon good citizens, created by "some central authority" (p. 254). Both John Marini and Caesar contradict Banfield, citing Tocqueville's statement that any "human idea of unity is almost always sterile," whereas God's plan for mankind is carried out by our participation in numerous, seemingly conflicting actions (pp. 282–83, 304). But even Banfield's dissent points out the revolution in Tocqueville scholarship represented in this book. Banfield attacks Tocqueville, the "student of self-interest," for failing to follow Mandevillean precepts. Masugi himself provides another measured criticism of Tocqueville: despite his prescience, Tocqueville "concedes too much to history"; his rejection of equality as a founding idea for political societies leaves the statesman with too little to guide him in ruling (p. xvii).

One may disagree with some arguments raised in this volume (e.g., that Tocqueville conceded too much to the preexisting institutions, beliefs, and practices that, after all, largely form human character). One might also note that Tocqueville's insistence upon the unity of our character and our need to follow God's will in order to live a good life places him within the tradition of natural law and not merely (as is often asserted) of natural rights.

Almost all essays in this volume are quite good. And the book's weakest essay by far (that by Robert Bellah) could not have been excluded without drawing criticism. Bellah's popular work has renewed interest in Tocqueville. But his essay adds none of the textual evidence so sorely needed to show that he understands Tocqueville's thought and applies it properly in his own work. Instead, Bellah continues to use Tocqueville's name and a few strategically chosen passages on "individualism" to further his own political agenda. With this exception, however, the questions raised in this book are precisely the kind one would expect from a serious and thoughtful study of an important thinker.

Oglethorpe University BRUCE FROHNEN

The Political Economy of Defense Contracting. By Kenneth R. Mayer. New Haven: Yale University Press, 1991. 232p. $28.50.

Kenneth Mayer's central argument is that the pork barrel theory of defense spending is both inadequate and inaccurate as a description of the defense contracting process. Instead, he finds the process of defense spending and Pentagon contracting to be largely driven by nonpolitical factors, overlayed by a veneer of congressional credit claiming, position taking, and all the other political phenomena that the Washington establishment uses to make points with the public and gain reelection.

He concludes that "one reason pork barrel explanations are so attractive is that they are simple, parsimonious, and persuasive. They are also mostly wrong" (p. 210). But they persist. They persist because it is in the interest of congressional members, interest groups, defense contractors, and the Department of Defense for there always to be the fallback explanation that a decision was made because of politics or the pork barrel.

Mayer's evidence falls into four areas. He first examines defense political action committee (PAC) contributions to members of Congress, finding that defense contractor PACs give disproportionately to the members of the military committees. This money is also strongly concentrated among those who are generally supportive of the industry and favor high defense spending. His question is whether the money influences votes. He finds that defense PACs are different from other PACs in timing their contributions around both the electoral cycle (the norm) and the budget cycle, so that there is an increase in PAC contributions about the time of budget hearings, markups, and conference committee meetings. His conclusion is that PAC money does not buy votes: "PAC money may provide members with some clues as to where their support lies, but it will not as a general rule guarantee their vote". (p. 96).

Mayer's second area of evidence concerns votes in Congress. Are local interests the primary determinant of a member's attitude on weapon systems? He finds the influence of local spending on the particular project to be relatively minor compared with other factors, such as a member's general attitude toward defense spending.

The third sort of evidence is defense contracting, where he finds the contracts themselves to be relatively nonpolitical. However, legislators typically surround both negative and positive decisions with political credit claiming, blaming, and the like, thus making what is actually a relatively nonpolitical process look very political. He contrasts the selection of prime contractors with the much more political processes involved in military base closures and the politics in Congress of writing restrictive language in authorization bills to attempt to keep programs important to one's district going. The latter, (in contrast to the actual selection of contractors) are very political, as is the selection of subcontractors, where the prime contractor endeavors to spread the subcontracting around to as many states and congressional districts as possible.

Finally, he looks at defense contracting and the electoral cycle, finding that contract awards are given out disproportionately in the two months before each November election but that these are normal contract awards whose processing is accelerated, that is, they would have been given out anyway. Again, credit claiming is maximized, a veneer over a relatively nonpolitical selection process.

Can we believe all this? I think Mayer is right on the essentials, but there are some troubling aspects that we ought to think about. First, in accepting the story that there is a political veneer of credit claiming and position taking over a relatively nonpolitical process, Mayer (and the others in the distributive politics area) is accepting a more sophisticated Washington myth over a less sophisticated, more journalistic one. While we have more evidence to support "our" myth, there are areas where the evidence is just as impressionistic, based fundamen-

tally on quotes from those involved in the process and limited case studies, just like the journalistic descriptions of defense contracting that he rejects. I am thinking specifically of the evidence on contractor announcements.

Second, if we have such a strong political veneer over an essentially nonpolitical process, what is the boundary between the two? What keeps them apart? How strong are those institutional boundaries, and what kind of a theory of bureaucracy do we need to maintain the boundary? Since the public administration subdiscipline has all but destroyed the politics/administration dichotomy, of which the more sophisticated Washington theory is beginning to sound uncomfortably like an adaptation, we need a better theory of bureaucracy to deal with this subject.

Political scientists interested in decision making in the Department of Defense, in congressional–bureaucratic interactions, or in distributive politics will find the data and descriptions of the decision-making process and outcomes interesting and very competently done (e.g., some very comprehensible leverage charts in the chapter on congressional voting); but the major findings have been prefaced in earlier work in the distributive politics area. For journalists, this book will be a major eye-opener.

I strongly recommend it. It is a solid piece of work, with interesting data, good use of several different methodologies ranging from case studies to probit analysis, and one of the few comprehensive examinations of the distributive aspects of an entire department's budget–procurement process.

California State University, Los Angeles J. Theodore Anagnoson

Dual City: Restructuring New York. Edited by John H. Mollenkopf and Manuel Castells. New York: Russell Sage Foundation, 1991. 477p. $39.95.

The Global City: New York, London, Tokyo. By Saskia Sassen. Princeton: Princeton University Press, 1991. 397p. $39.50.

The two books reviewed here are important benchmarks in the study of urban politics. They document the changing character of cities and highlight the increasingly global context of their economic activity. In doing so, *The Dual City* focuses primarily on New York but gives some comparative attention to Los Angeles and London. *The Global City* examines New York, London, and Tokyo—the three major centers of finance and specialized producer services in the world economy. Together, the two books paint a complex picture of the process known as restructuring, the process through which advanced technology and capitalism have modified class relations and transformed cities worldwide.

Sassen's major achievement in *The Global City* is to show that even though New York, London, and Tokyo are located in radically different cultural and national contexts, they display similar trends. According to Sassen, they are shaped by being command-and-coordination posts in an otherwise dispersed world economy. As their role in the manufacture and transport of consumer goods has declined, all three cities have experienced an expanding high-income professional and managerial class but a shrinking middle class. Casual labor and a growing informal economy (i.e., one largely outside of formal regulations, union contracts, and guaranteed benefits) have put much of the population on insecure economic ground.

Heightened affluence among a consumption-minded class of managers and professionals side by side with the increased need and vulnerability in the remainder of the population is at the heart of the "dual city" thesis in the collection edited by Mollenkopf and Castells. Indeed, as both books indicate, the ability of the affluent to pay well for what they want drives up costs for the nonaffluent, sometimes pushing the latter toward privation. A cost squeeze in affordable housing encourages homelessness, for example.

Tokyo is a particularly interesting case. Not only are the beginnings of homelessness evident, but Tokyo now has a significant illegal immigrant population, notwithstanding Japan's tradition of being a relatively closed society. For inhabitants of the Third World, the informal economy of a global city is inviting despite the hardships entailed.

What is the source of these trends? *The Global City* highlights structural change, the transition from an industrial to a postindustrial society. *The Dual City* concurs but gives a more variegated account and brings the account closer to everyday experience in chapters such as "Women Clerical Workers" and "Crime and the Social Fabric." Whereas Sassen concentrates on global processes, the collection edited by Mollenkopf and Castells gives more attention to interconnections between different levels of activity in the postindustrial city.

Both books are attentive to social, economic, and political complexity but also identify a dominant pattern—the diminution of a stable and economically secure middle class. The erosion of stable employment is, of course, the principal contributor to a diminishing middle class. It is plain that the mobility of finance capital and the dispersion of economic production have undercut organized labor and weakened resistance to the casualization of labor.

The two books thus illuminate the persistence of poverty even under conditions of overall economic growth. Specifically, they call into question the argument that welfare dependence has spawned an urban underclass. Social assistance has been eroding, along with the decline of labor unions and the weakening of the "social compact" that mitigated industrial conflict. By inference, the major contributor to the "culture" of poverty is the shortage of permanent jobs with adequate income and benefits. Economic conditions can undermine "family values" and other features of the mainstream culture. In short, casual labor is not conducive to family responsibility and other long-term considerations.

As both books make clear, there is no turning back to an earlier era. For example, stimulation of industrial jobs is no guarantee of economic improvement. In *The Dual City*, Edward Soja's chapter on restructuring in Los Angeles shows that many of the new industrial jobs are low-paying; and, as Sassen argues, a significant amount of industrial work now occurs in the informal economy. In their chapter in *The Dual City*, Ian Gordon and Michael Harloe go to the heart of the matter, citing the reduced incentives for individuals to acquire human capital. The economy itself encourages a live-for-the-moment mentality.

Escaping long-term commitments and obligations is

not restricted to the lower class; it is now pervasive. The ability of mobile capital to induce flexible, profit-serving arrangements has altered society in important ways. The weakening of the implicit "social compact" stands out as an especially important example. But the two books reviewed here depict something much more complicated than a society unraveling. There is a dynamic to the process of restructuring. Mollenkopf and Castells, particularly, give sustained attention to a business and professional core capable of concerted action and to the political handicaps of a "disorganized periphery" of women, minorities, and lesser-paid service *and* industrial workers (pp. 17, 402, 404).

It is not possible to capture here the full richness of these two books and the many issues they raise. There is, however, a major line of argument that unites the two books and guides their discussion. Conflict and division are characteristic of postindustrialism; and this is most evident in the tension between a sector that provides upward mobility and increasing mobility and a sector characterized by shrinking opportunities and deepening deprivation. These two sectors "are not separate and distinct, but rather deeply intertwined products of the same underlying processes," according to Mollenkopf and Castells (p. 11). How, then, will social tension be managed? By separation, by selective disinvestment and retreat into "defended enclaves" (p. 8)? Or, as Sassen suggests, by an ideology of collective powerlessness and inevitable subordination to the forces of the market (p. 334)?

Is there another alternative? The democratic ideal would have us realize and appreciate our shared fate, would see us attracted to the aim of an inclusive society of widening opportunities, and would encourage us to develop the appropriate understanding and build the necessary coalitions to pursue that aim. *The Dual City* and *The Global City* tell us much about how difficult an undertaking that will be. They also tell us how necessary it will be in order to avoid living in a fragmented and conflict-ridden world of uncertain long-term promise. By posing that choice, these two books provide a valuable foundation for beginning a more positive reconstruction of urban life in the postindustrial world.

University of Maryland CLARENCE N. STONE

Media Messages in American Presidential Elections. By Diana Owen. Westport, CT: Greenwood, 1991. 198p. $42.95.

The Formation of Campaign Agendas: A Comparative Analysis of Party and Media Roles in Recent American and British Elections. By Holli A. Semetko, Jay G. Blumler, Michael Gurevitch, and David H. Weaver. Hillsdale, NJ: Lawrence Erlbaum, 1991. 197p. $39.95.

Both of these books examine elections and the media; but they do so from very different analytical, theoretical, and methodological perspectives. The Semetko, Blumler, Gurevitch, and Weaver book is a comparative analysis of the formation of campaign agendas in the 1984 American presidential election and the 1983 British general election. The authors focus on the relative power of the media, candidates, and political parties in shaping the news. Diana Owen's *Media Messages in American Presidential Elections* examines the differential effects of political advertising, news stories, polls, and debates on voters' perceptions of candidates and the campaign during the 1984 and 1988 American presidential campaigns. The emphasis in the Owen book is placed on voters' needs and motivations for using the media during the campaigns and how different kinds of campaign communications might meet those needs.

The Formation of Campaign Agendas builds on the classic agenda-setting work of Maxwell McCombs and Donald Shaw to examine how news agendas are shaped. Rather than concentrate on the media's power to set the agenda of public opinion, however, the authors argue that the media are only part of an interactive process in which candidates and parties struggle among themselves, as well as against news organizations, to control the terms of political debate presented in the news. As the authors clearly demonstrate, the process is a deeply political one and is affected by systemic, institutional, and particularistic conditions that shift the balance of control between the journalists and the politicians.

Starting with a lucid explanation of the major similarities and differences in the media and electoral systems of the United States and Great Britain, Semetko and her colleagues set forth hypotheses to examine the power of the press and politicians to affect campaign agendas. The hypotheses explore the extent of respect or suspicion for politicians and the political process, journalists' pragmatic versus sacerdotal orientations to political coverage, the degree of professionalization of election campaigns, and variation in media competition. In testing these hypotheses, the authors present results from a meticulous content analysis of campaign agendas in selected television and newspaper sources, presidential candidates' stump speeches, and British parties' daily morning press conferences.

As with much agenda-setting research, the authors observe the similarities and disparities among the agendas for a given period of time. One major criticism of this kind of work is that it reveals very little about the direction of influence: Who or what set whose agenda? Or were both agendas affected by a third, external force? In order to address this potential problem, the authors introduce data from their observation of, and interviews with, journalists and editorial staffs of the British Broadcasting Corporation and National Broadcasting Company. This is an excellent example of the use of complementary research methods. The authors give insights into the activities of the daily news operations, the time pressures, the sometimes conflicting journalistic roles, and the relative lack of policy deliberation about the election coverage.

The Formation of Campaign Agendas is a well-researched and clearly written piece of scholarship that advances knowledge about the transactional relationships between the institutions of the press and politics. There are three points that the authors or others working in this tradition might pursue in further research. First, the observations of television newsrooms could be expanded to other participants in the process, particularly the campaign and party organizations. This would provide a more balanced discussion of the reciprocal influences of the campaigns and news staffs. Second, additional cases should be examined in order to test the hypothesis regarding media competition. The differences observed between the campaign agendas in the United States and Great Britain may be due to the demands of a commercial, versus a public service, broadcasting system, as the authors suggest. However,

more than two cases need to be considered to provide conclusive evidence. Finally, to their credit, the authors examine the content of visual communications. This work could usefully be developed to allow for more than the five visuals per television story measured in the current book and to examine the interplay of verbal and visual messages.

Media Messages in American Presidential Elections also examines the 1984 American presidential election, but in comparison to the 1988 election. The book is in the media-effects tradition and asks about the relative influence of different forms of campaign communications on the voters and their electoral decisions. Owen views voters as active and aware consumers of campaign messages. Employing a "uses-and-gratifications" model, the author examines the characteristics of political advertising, television and newspaper news, public opinion polls, and presidential debates in order to assess how they may (or may not) gratify voters' needs.

The first chapter is devoted to explaining the "audience-centered model of mass media use and outcomes" (p. 15) and the survey research utilized to assess audience evaluations of candidates, uses of campaign communications, and perceptions of media effects. Owen argues that there are two main types of effects: subjective (reflecting feelings about how well voters' needs are satisfied by mass communication) and objective (dealing with cognitive and behavioral change). The author focuses on subjective effects by analyzing the self-reports of survey respondents in two (1984) or three (1988) counties in the United States. By relying on the self-reports of subjective phenomena, the author considers only the audience's conscious awareness of motivations for using the four communication types. The data largely confirm findings from past research.

Assuming that the four types of campaign communications have unique characteristics that will meet particular audience needs, the bulk of the book describes and analyzes campaign ads, news, polls, and debates with examples from the 1984 and 1988 elections. In each media chapter, Owen briefly examines the shaping of message types by candidates, their campaign staffs, and mass media organizations. She also explores how candidates and the media use each message type and presents analyses of the survey data to discuss voters' uses and perceptions of messages. The last two chapters examine more directly how voter needs and motives can be satisfied by the four message types.

Owen presents a number of findings from previous research, as well as from the county survey data. This is both a strength and a weakness. It is a strength because Owen supplements the disparate findings of past media effects research with results from her 1984 and 1988 election studies. However, because so much information is presented, it is difficult to trace the central points of Owen's argument. The uses-and-gratifications model the author employs asks us to focus on the role of the active audience and does not seem adequate to explain the differences in types of campaign communication. Since most of the book describes and analyzes types of political communications and the characteristics they exhibit that might satisfy voters' reported motives and needs, it seems that a more appropriate theoretical framework would take into account the motives and goals of the communicators and the nature of the content, as well as the motives of the voters. In a discussion of image formation, Owen hints at an interactive process of "mutual exchange of perceptions" between campaigns and voters (p. 13) but does not develop it. The author is clearly interested in content differences, since she examines advertising, news, polls, and debates. Her ideas would be better served either by elaborating on the connection between the theoretical framework and the data or by using a transactional or constructionist model (e.g., those of Sidney Kraus and Dennis Davis's *Effects of Mass Communication on Political Behavior* [1976] or David Swanson and Dan Nimmo's *New Directions in Political Communication* [1990]).

Both books are well grounded in the literature on media and elections and should be of interest to students of American presidential politics, political communications, or media influence. *Media Messages in American Presidential Elections* adds data from the 1984 and 1988 presidential campaigns to our understanding of media and elections. *The Formation of Campaign Agendas*, with its comparative, multimethod perspective makes a particularly important and original contribution to the existing literature on agenda-setting and electoral politics.

University of Southern California ANN N. CRIGLER

The Rational Public: Fifty Years of Trends in Americans' Policy Preferences. By Benjamin I. Page and Robert Y. Shapiro. Chicago: University of Chicago Press, 1992. 488p. $59.95 cloth, $19.95 paper.

The Reasoning Voter: Communication and Persuasion in Presidential Campaigns. By Samuel L. Popkin. Chicago: University of Chicago Press, 1991. 291p. $19.95.

Public Opinion in America: Moods, Cycles, and Swings. By James A. Stimson. Boulder: Westview, 1991. 156p. $49.95 cloth, $14.95 paper.

Most areas of research display a predictable pattern, characterized by a surge of excitement and energy, followed by solid, normal science, followed yet later by ever-growing calls for new questions and perspectives. The study of public opinion is no exception. Philip Converse's "The Nature of Mass Belief Systems" served as the catalyst for nearly three decades of systematic inquiry into the nature of people's political attitudes and preferences. But during the past few years, political scientists began to express a concern that research had hit a plateau. The plaintive plea, "We need some new ideas and some alternative ways of thinking about public opinion" dominated more than one dinner conversation at professional conventions.

Students of public opinion owe a great debt to the authors of the three books listed above. Published almost simultaneously, these works, individually and collectively, undoubtedly will provide the impetus that researchers have been seeking. In a very real sense, the next generation of graduate students will be building upon a new and exciting foundation.

For those who place faith in the ordinary citizen, the authors have done much more than generate a renewed interest in the research endeavor. All three books resurrect the place of citizens and their preferences within the context of American representative democracy. Not that any of the authors portrays the typical citizen as a philosopher–king, steeped in knowledge about national politics. But democrats will find great satisfaction in the conclusion that public opinion is far from capricious,

arbitrary, and ill-founded. Contrary to the prevailing and rather pessimistic assessment (some might say, indictment) that arose from decades of analysis of individual responses to survey items, these works find in public opinion a capacity to guide public policymaking.

The Rational Public represents the culmination of a project that the authors began a decade ago. Based on a massive data set that combines nearly every existing poll or survey question related to national politics, the study reports, in detail and with great care, trends in Americans' policy preferences between 1935 and 1990. Those who teach introductory American politics or public opinion courses will find a gold mine of information here. Page and Shapiro not only present the trends in easily digestable form but discuss the larger economic, political, and technological contexts that help to explain them.

One who graded public opinion on the basis of reading *The Rational Public* would surely conclude that an *A* is in order; for public opinion, Page and Shapiro argue, meets the "three *R*s" criterion: it is reasonable, responsible, and rational. Not any public opinion, mind you, but *collective* opinion. The latter, immune to problems arising from measurement error or temporary opinion changes by individuals, reacts in an understandable and a predictable fashion to events, broad social and economic changes, and statements of experts and visible public officials. Moreover, public opinion often influences government policy.

Collective public opinion may be rational, but by no means is it foolproof. Page and Shapiro present striking examples of the manipulation of information by officeholders, especially in foreign affairs, which led the general citizenry to prefer policy options that served the politicians', but not the public's, interests. The rationality of representative democracy depends not only on the rationality of collective opinion but also on a supply of honest and complete information provided by those who have direct access to it. Political competition among elites increases the likelihood, but does not guarantee, that such information will be forthcoming.

Like Page and Shapiro, Stimson conceptualizes public opinion as an aggregate phenomenon—and with convincing justification: "If our topic is people, then [the focus on individual psychology] is relatively harmless. . . . But if the topic is politics, then it matters a great deal if aggregate opinions are more orderly and meaningful than individual ones. For it is the aggregate that matters in politics." (p. 2). Presumably, most politicians and journalists would agree.

Whereas Page and Shapiro have written a long book, Stimson has written a short one (an interim report on the way to a larger study of political representation). Whereas the former construe collective opinion in terms of rather specific policy areas (civil rights, civil liberties, wage and price controls), Stimson develops the idea of *policy mood*. What distinguishes mood from specific policy preferences is that the former involves two aggregations: over individuals and over issues. Mood, then, is akin to liberalism–conservatism (Should government do more or less?) but is a function of a whole package of specific policy preferences.

I cannot begin to do justice to the character and quality of Stimson's work. It represents some of the most sophisticated analysis to be found in the field of public opinion. Moreover, the care and thought that go into the conception and measurement of mood will set a new standard of research. Despite all of the technical complexity and sophistication, Stimson retains a commonsense perspective throughout the book. Truly insightful work always becomes "self-evident" upon completion, a criterion that every chapter in *Public Opinion in America* meets.

Mood, Stimson demonstrates, became increasingly liberal during the 1960s and early 1970s, shifted to the Right during the remainder of the decade, and then (surprise!) turned in an undeniably liberal direction just about the time that Ronald Reagan captured the presidency. Just as Page and Shapiro find rationality in the movement of collective policy preferences, Stimson sees a logic in the cycles in mood, a determination that depends on the author's conception of political representation.

To Stimson (in contrast to Page and Shapiro) the linkage between citizens and representatives is not a direct one whereby public opinion and public policy share a one-to-one (even if rough) correspondence. Rather, citizens respond to government performance. If a generally liberal agenda appears to be working, people will, within a "zone of acquiescence," permit politicians to take the lead in developing specific programs. If, however, they begin to perceive problems, citizens will become less tolerant of government action, which will be reflected in a growing conservative mood. Thus the increased conservatism of the mid-to-late-1970s reflects a growing disenchantment with the liberal agenda that dominated the 1960s just as the shift to a liberal mood during the 1980s reflects a dissatisfaction with Ronald Reagan's policies.

One might argue that Page and Shapiro's and Stimson's similar conclusions about the rationality and reasonableness of public opinion depend wholely on aggregation. Analysis at the individual level, such an argument would go, will inevitably lead to a very different conclusion, as it has for nearly three decades.

Not so, contends Popkin. Ironically, Popkin reaches this verdict by turning to the very research that Page, Shapiro, and Stimson likely find unnecessary and superfluous. Especially important are the insights of psychologists Amos Tversky, Daniel Kahneman, and Jerome Bruner, who, through sustained programs of research, have demonstrated that people use heuristics and other informational shortcuts when making decisions. Three cognitive principles loom especially large in Popkin's work: (1) the representative heuristic (inferring what kind of person someone will be by comparison to "typical" people one knows); (2) the recency effect (weighing recent information more heavily than past information); and (3) the Drunkard's Search (simplifying the decision process by focusing on a limited number of criteria). Reliance on these three heuristics, especially in combination, comprises what Popkin calls "low-information or gut-level rationality."

People do not use these rules devoid of context, however. Thus, the framing of alternatives influences the final judgments as much as the informational shortcuts themselves. With respect, specifically, to electoral politics, candidates compete among themselves to frame the campaign to their advantage. Five changes of frame occur repeatedly in presidential primary and general elections: (1) portraying the incumbent as president or as a candidate; (2) discussing the candidates as people or as holders of public records; (3) portraying the candidates as battling to win the nomination or as certain nominees;

(4) emphasizing economic or international issues; and (5) emphasizing unemployment or inflation.

Popkin then goes on to argue that people in fact make reasoned (and, by implication, "good") choices in primary and general elections. The abrupt changes in support for primary candidates make good sense once one recognizes that voters use heuristics and that candidates continually strive, sometimes successfully, sometimes not, to change the prevailing frame of the campaign. Moreover, the mystical campaign momentum that numerous scholars have documented and tried to understand is no more than a result of voters making reasoned choices.

Unlike the other two books reviewed here, *The Reasoning Voter* presents little quantitative analysis. What Popkin has demonstrated, then, is the value of carefully and systematically integrating the literature and then deriving implications.

I inevitably have a few quibbles about each book. Because Page and Shapiro had no control over the timing of the surveys and polls, they cannot completely rule out history effects. Moreover, it may be that the rhetoric surrounding policy proposals does more to influence opinion than the policies themselves. If so, Page and Shapiro may have underestimated how far the actions of public officials hold sway. Stimson unabashedly construes mood in cognitive terms. Perhaps mood reflects a growing anger on the part of citizens toward government. In this light, it would be helpful to know whether the documented changes in mood occur principally among certain segments of the population (e.g., the less interested). At times, Popkin presents as fact what others might consider an open empirical question.

In no way should these quibbles detract from the significance of the works. If there is any doubt, consider two questions political scientists now face. First, if the purpose is to understand the nature and role of public opinion in American representative democracy, should scholars redirect their attention wholeheartedly to aggregate-level, rather than individual-level analysis? That *The Rational Public* and *Public Opinion in America* have just appeared is not completely happenstance. Many disciplines seem headed toward holistic perspectives, as exemplified by the emergence of chaos theory. Ironically, this movement within the discipline is occurring at the very time that political psychology, a highly reductionistic approach, has gained unprecedented prominence. The easy and perhaps correct answer is that we need both perspectives. Whatever one's own choice, the works reviewed here have brought the level-of-analysis problem to the fore, precisely where it belongs.

Second, what criteria should we, as a discipline, bring to bear when evaluating representative democracy? All three works reviewed here reach optimistic conclusions about the quality of public opinion in American politics. Page, Shapiro, and Popkin, like others, view cue taking as an effective means by which people can make proper political judgments. One might argue, however, that the very fact that citizens must look to politicians and other elites for cues underlines a basic problem with large-scale and increasingly complex political systems. "Knowing" via cues may or may not be equivalent to knowing via possessing factual, rooted-in-life information. If not and ordinary citizens cannot gain access to relevant data, what then? Or if, as Stimson argues, mood changes because the citizenry-at-large becomes disenchanted with government programs, why the continuing disenchantment?

Thanks in good part to Benjamin Page, Robert Shapiro, James Stimson, and Samuel Popkin, these kinds of "big" questions beg answers more than ever before. The future of public opinion research could not be more challenging and exciting.

University of Illinois, Urbana–Champaign JAMES H. KUKLINSKI

The Culture of Spending: Why Congress Lives Beyond Our Means. By James L. Payne. San Francisco: Institute for Contemporary Studies, 1991. 225p. $24.95.

Why do members of Congress vote to increase federal spending in an era of $300-billion deficits? In a lively and contentious study, James Payne argues that members vote for higher spending levels because they are "persuaded" to do so by the pro-spending culture in which they work and by their prior socialization, rather than because of an electoral need to appease voters.

This pro–spending culture is a world filled with pro-spending cues, with few if any that encourage opposition to spending. Rather than serve, for example, as a forum for an evenhanded evaluation of federal programs, committee hearings are "rump hearings," where the witnesses are overwhelmingly in favor of greater spending. Payne's review of witness lists also interestingly reveals that anti-spending groups and organizations, such as the Cato Institute and the Heritage Foundation, rarely make the effort to testify and so to educate members during hearings about why program budgets should be cut. Thus, there is little balance to the testimony presented at these hearings.

While agency officials and pro-spending lobbyists dominate the formal hearings' witness lists, the members' informal contacts and other sources of information, including the media, are also dominated by groups and individuals seeking new programs and increased spending. The "data base" on the effectiveness of federal programs that is available to members, for instance, is heavily weighted toward the proposition that government is the appropriate instrument for solving national problems. This data base is produced by the various agencies and interests that benefit from federal spending, who are loath to recommend anything more than marginal improvements in program performance. The alternative of reducing or terminating federal spending is rarely if ever proposed. Government programs are in fact evaluated by true believers, so that "true critics are excluded from the policy-evaluation process because, simply, they contradict the purposes of those who fund and participate in the evaluation projects" (p. 34). The consequence of all this pro-spending information is not just persuasion, but conversion.

Perhaps more important than their surroundings, Payne indicates that prior experience and socialization of members predisposes them to spend. In the case of both Democrats and Republicans, prior governmental office-holding is positively associated with a greater tendency to vote for spending. Moreover, the longer one has served in Congress, the greater the tendency to spend. When distinguishing between Democrats and Republicans, Payne finds that representatives of the two parties exhibit very different frames of reference when they consider whether to vote for a spending bill. Democrats emphasize what Payne calls a "manifest" impulse,

which "leads them to favor immediate government solutions to apparent problems" and thus a tendency to support spending. Republicans emphasize a "latent" impulse and "are more inclined to weigh the long-run costs or harm of a government program, and are therefore less enthusiastic about funding it" (p. 111).

To control spending, Payne advocates creating a culture of anti-spending. This counterculture would include a federal agency that would supply members with information on the inefficiencies and ineffectiveness of federal programs; closed hearings that would shut out lobbyists; a ban on witnesses' testifying before the same committee two years in a row; congressional term limits; restrictions on the amount of time a member might serve on a particular committee; and cost–benefit analysis and program evaluations that would consider the benefits occurring from leaving proposed expenditures in the private sector. Significantly, Payne rejects procedural reforms, such as Gramm–Rudman–Hollings, because members easily find ways to circumvent legal spending limits.

Payne's study is a well-written work that reminds us of the importance of information, attitudinal predispositions, institutional culture, and socialization in budgetary decision making; and he warns that not all spending votes are tied to electoral purposes. The author, furthermore, tells us what any lobbyist working the Hill already knows, namely, that organized opposition to spending is limited and that in their analyses, agencies like the General Accounting Office regularly pull their punches and recommend only marginal programmatic changes.

The culture Payne describes, however, is more complex than he allows. Particularly during the last 12 years, for example, members have regularly heard testimony, regardless of the truth of the matter, that deficit spending would cripple the nation's economy. This theme is constantly replayed by the media; and, to name only one official source, the Congressional Budget Office annually identifies potential program and budget cuts. Moreover, although this study focuses on Congress, the Reagan–Bush administrations have regularly refrained from vetoing appropriations bills, thus significantly contributing to the culture of spending. Finally, although Payne offers welcome relief from the mechanical decisions offered by public choice voting theories, he underestimates the importance of the voters' influence on congressional spending decisions. Members are not paranoid when they think twice about making deep cuts in middle-class entitlements like Social Security. This point is especially important, because the big spending growth in the budget resides in the entitlement, rather than the discretionary accounts.

These comments aside, Payne's book is a valuable addition to the budgetary literature, one that renews the importance of the sociological–cultural framework for studying spending decisions. This book would be appropriate for both undergraduate and graduate students.

University of Virginia JAMES D. SAVAGE

Toxic Politics: Responding to Chemical Disasters. By Michael R. Reich. Ithaca: Cornell University Press, 1991. 316p. $45.00 cloth, $15.95 paper.

The theme of this book centers on the question of how the victims of chemical disasters obtain redress. Specifically, the book takes a comparative view of the politics of chemical disasters by focusing on three such disasters—PBB contamination in cattle in Michigan; dioxin contamination from a factory in Seveso, Italy; and PCB contamination of cooking oil in western Japan. Understanding why the victims of toxic contamination confront similar political obstacles is a major objective of his study. Reich's analysis proceeds in three parts. First, he observes concrete decisions made by political elites and the use of material resources in political bargaining. Next, he pays attention to the processes of agenda formation and analyzes why some issues are organized into politics while others are not. Finally, he analyzes efforts to influence, shape, and determine the wants and demands of powerless groups.

In carrying out his analysis, Reich utilizes case studies as "plausibility probes" for his conceptual schema. The chapters on the three disasters are based on field research conducted during 1978–79, with subsequent follow-up. His methods include collection of primary and secondary documents, as well as elite interviews with approximately 80 major participants in each incident. These participants include victims, politicians, journalists, civil servants, businessmen, private and public interest activists, and legal and medical specialists.

Essentially, Reich argues that the victims of toxic contamination seek redress through politics in three phases: making the problem public, organizing group actions, and mobilizing political allies. In doing so, they often find themselves involved in a process that can "become as poisonous as the chemicals themselves"—"toxic politics" (p. 2). Not long into the process of redress, contamination victims quickly discover that they are powerless. The book concludes with a somber message that incremental tinkering with the regulatory regime may be achievable yet have little impact. Significant change, on the other hand, will have to confront deep political obstacles. Ironically, to obtain even incremental changes in laws, institutions, and goals, more disasters may be required. Thus, to prevent more chemical disasters, "paradoxically and tragically, we may need more chemical disasters" (p. 280).

Other similar books have used the case study approach to analyze the politics of chemical or nuclear disasters. Such books include those on Love Canal (Levine), Bhopal (Shrivastava), Seveso (Conti and Whiteside), and Three Mile Island (Goldsteen and Schorr). In combination with this recent literature, the book suggests that disasters have both detrimental and advantageous effects. From a more positive standpoint, such disasters awaken the public from their complacence and instill in them the understanding that they must take much (perhaps most) of the responsibility for managing their own safety and health, since all too often, corporate and public actors resist their pleas for redress and assurances from technological risk. For example, in Goldsteen and Schorr's *Demanding Democracy after Three Mile Island* (1991), citizens became more favorable toward participatory democracy after discovering that responsible officials misled them in the aftermath of the disaster. In the United States, we have observed the rise of a grassroots movement on toxic chemicals in the aftermath of disasters at Love Canal, Times Beach, and others. The evidence discussed in this book continues to suggest that disasters are bringing about a transformation in the way citizens relate to government. In one instance after another, citizens are "demanding democracy" or even turning toward new

forms of citizen participation that may bring them into the forefront of policymaking and implementation.

In summary, this book is a welcome addition to the growing literature on disaster management. More specifically, it could be useful in courses on environmental politics and policy, as well as public policy more generally. While it is constrained by the usual limitations of case study research, it nevertheless contributes a great deal to ongoing discussions about citizen participation in the wake of chemical disasters and contemporary politics of disaster management.

Colorado State University JAMES P. LESTER

The End of Realignment? Interpreting American Electoral Eras. Edited by Byron E. Shafer. Madison: University of Wisconsin Press, 1991. 187p. $35.00 cloth, $12.95 paper.

This collection is an outgrowth of the Harold G. Lasswell Symposium at the 1989 meeting of the American Political Science Association. The first three chapters, by Joel H. Silbey, Everett Carll Ladd, and Byron E. Shafer, are revisions of the papers presented at the session. The chapters by Samuel T. McSeveney and Walter Dean Burnham are revised and expanded versions of responses to the papers. An excellent bibliography of the literature on realignment by Harold F. Bass, Jr., completes the volume.

It cannot be reported that this collection will end the continuing debate over the utility of realignment theory (synthesis, perspective, model) either by demolishing it once and for all or by providing new and convincing support. The authors of the first three essays question the capacity of realignment formulations to explain either historical or contemporary American politics. They differ, however, in both the nature and degree of their skepticism. Silbey attributes value to realignment formulations for understanding past, but not contemporary, politics and attempts to subsume such formulations under a broader conceptual framework. Ladd's view is summarized by his essay subtitle, namely, "On the Uselessness of 'Realignment' for Understanding Change in Contemporary American Politics"; and he clearly holds the same view of its applicability to historical politics. Shafer is carefully noncommittal in discussing the utility of realignment formulations for understanding past politics but views them as of little or no value for understanding the contemporary era. All three argue that the political system has passed beyond realignment and no longer has the capacity to realign.

The commentators take different positions. For McSeveney, realignment formulations retain value for understanding political history, although he is more dubious where the present era is concerned. Burnham reasserts the value of the realignment framework for understanding both past and contemporary politics and argues that realignment in fact did occur during the years from 1968 to 1972. He does so, however, by introducing an additional type of realignment, one of non- or even antipartisan form.

Discussions of realignment have rarely been entirely consistent from one to the other and often fall short as models of conceptual clarity or definitional agreement. Unsurprisingly, this collection is no exception. In the initial essays, it is often unclear whether discussions of realignment refer only to the rare historical elections that some scholars have described as realigning or to broader formulations that attempt, whether successfully or unsuccessfully, to describe not only realigning elections but also the politics of the years between realignments.

Discussions of the late 1960s and the early 1970s have similar characteristics. These years are seen as marked by profound political change leading to a new electoral era quite different from those of the past. These changes cannot be seen, however, as realignment—at least not of the "classical" form. The first reaction of a naive reader might be that this may be at least in part a matter of definition. Since the authors make much of the differences between historical realignments, it is not completely clear what the properties of a "classic" realignment are supposed to be. Treating these years as a further example of realignment, only of a different form (as Burnham does), may only add an additional complexity to an already overly complex formulation. The test is, of course, further exploration; but even without such an addition, realignment remains an important (though perhaps time-bound) component of our understanding of historical American politics.

These essays are important contributions to the continuing debate over realignment and to our understanding of American politics, past and present. The panelists at the Lasswell Symposium may have come, as McSeveney suggests, "to bury realignment" (p. 95). Taken in total, however, the resulting volume suggests that burial would be, to say the least, premature.

University of Michigan JEROME M. CLUBB

Reasoning and Choice: Explorations in Political Psychology. By Paul M. Sniderman, Richard A. Brody, and Philip E. Tetlock. New York: Cambridge University Press, 1991. 304p. $47.95.

Although woefully uninformed about politics, most citizens manage to provide answers to the myriad demanding and often obscure political questions that confront them in typical mass opinion surveys. How do they do it? This is the central question of *Reasoning and Choice*, a collection of separately written papers woven together by the book's three principal authors.

One possible answer is that bewildered by it all, most citizens respond to surveys in a random or otherwise meaningless fashion. But Sniderman, Brody, and Tetlock, dubbing this the minimalist thesis, reject it as inapplicable to the majority of cases that concern analysts of public opinion, thus setting the stage for the main body of their analysis.

That analysis focuses on "heuristics," or judgmental shortcuts to opinion. The most important of these is the "likability heuristic"—the tendency of people to favor good things, like government aid and freedom of speech, for individuals and groups they like and to oppose good things for those they dislike. Thus, in a series of roughly parallel chapters, the book shows that affect toward blacks, homosexuals, liberals, and conservatives (as measured on hundred-point feeling thermometers) is a reliable determinant of people's opinions on related policy questions. Citizens appear to use the likability heuristic not only for gauging their own policy preferences but for making inferences about the preferences of other groups. They attribute positions similar to

their own to liked groups and positions dissimilar to their own to disliked groups. The book further shows that citizens differ in their reliance on the likability heuristic. Well-educated people attach some weight to feelings in making up their minds about issues, but more to ideology; less-educated citizens rely more exclusively on feelings.

Political scientists who do not specialize in public opinion may be unimpressed by such findings. What, they will wonder, have we really learned when we find out that people favor good things for groups they like and bad things for groups they dislike? This is a fair question; but the findings, nonetheless, constitute progress. There is still doubt in the public opinion field whether there is any structure at all in the attitudes of many less-educated respondents. Sniderman, Brody, and Tetlock make a persuasive case that there is, and that it is based on gut-level likes and dislikes.

The book also devotes substantial attention to the nature and operation of political ideology. One finding is that conservatives are distinctly more negative in their feelings toward liberals than are liberals in their feelings toward conservatives. I could not help wondering whether this dour dislike of political opponents might explain why, as another chapter of *Reasoning and Choice* shows, conservatives are less willing than liberals to extend civil liberties to various outgroups, even right-wing outgroups.

Another intriguing finding was that right-leaning citizens appear, in several quite different situations, more susceptible to manipulations of the survey instrument. In one manipulation, respondents were asked whether they supported a certain policy; but half were told that Congress had passed a law in favor of it and half were not. Right-leaning citizens (here measured by low scores on a measure of affect toward blacks) were more apt to favor the policy after being told that Congress had supported the policy (a program to help minorities), whereas racial liberals were unaffected by the experimental manipulation. In another case, self-identified conservatives were more apt than liberals to change their initially stated opinion when the interviewer attempted to counterargue it ("Would you still feel that way even if . . . ?). By way of partial explanation for these findings, the book maintains, with some empirical support, that conservatives may be more conformist than liberals.

A notable strength of the book is its innovative use of Computer Assisted Telephone Interviewing to test substantive hypotheses concerning the motives of different ideological groups. In one experiment, for example, the authors seek to find out whether conservatives or liberals are more likely to take account of the race of a laid-off worker in deciding whether the worker is entitled to unemployment benefits. (The answer is that conservatives are more likely to pay attention to race. They favor a *higher* level of benefits when the laid-off worker is black, but only when told, in another experimental variation, that the worker had been dependable on the job; otherwise, conservatives are, like liberals, unaffected by the race of the worker.)

Because the book covers a great deal of ground, it is open to challenge from many directions. But I found *Reasoning and Choice* an original and stimulating book that merits careful reading and reflection by specialists in the field of political behavior.

University of California, Los Angeles JOHN ZALLER

Politics, Markets, and Congressional Policy Choices. By Peter M. VanDoren. Ann Arbor: Michigan University Press, 1991. 196p. $32.50.

Government intervention in the economy is ubiquitous. The nature, scope, and scale of this intervention suggest strong normative misgivings on the part of politicians about the allocative and distributive consequences of a market economy. The prophylactic policies of Congress engendered by these misgivings are the targets of Peter VanDoren's work.

VanDoren starts with the charitable hypothesis that Congress is well meaning in its micromanagement of the economy and is pursuing in these policies the legitimate provision of public goods or other remedies to classic market failures. Rejection of this hypothesis follows from even the shallowest examination of congressional policymaking. VanDoren then offers alternative hypotheses that range from well intentioned but misapplied policy to the conscious pursuit of goals unrelated to allocative efficiency or, for that matter, distributive equity.

The normative issues related to market intervention are given considerable attention by VanDoren. Intertwined in this discussion are standard microeconomic treatments of relevant topics such as market failure, property rights, and allocative efficiency. For a number of reasons, this is a very useful enterprise. As a service to those less well versed in microeconomic theory, VanDoren defines and clarifies apparent congressional confusion among allocative efficiency, distributive equity, and naked pork barrel politics.

For the central purpose of his book, an empirical analysis of congressional intervention in post-war energy policy, VanDoren combines his discussion of normative issues with a positive theory of congressional policymaking to generate testable hypotheses relating to the formation of energy (coal and nuclear) policy. The positive theory of policy choice seeks to relate the policy preferences of relevant political actors to economic conditions, interest group preferences, and the distribution of costs and benefits. This presentation is somewhat less compelling than the earlier discussion of normative issues. The sampling from the relevant literatures is a bit too selective and proves difficult to synthesize.

The backdrop to the empirical analysis is a two chapter narrative of the history of congressional micromanagement of U.S. energy policy since World War II. This history is indeed a dismal one and VanDoren spares no opportunity to bring to light the consequences of misguided congressional policymaking. In characterizing energy policy as an endless stream of mistakes the author does not hide his own strong beliefs about what goals policy ought to be achieving: allocative efficiency, with distributional issues dealt with by the redistributional opportunities provided for by the economic gains achieved through greater allocative efficiency. Less clear is where the author lays blame. Although Congress is clearly the guilty party, it is not clear whether the mistakes are by design (different policy objectives), the result of congressional confusion about the true nature

or incidence of the effects of policy choices, or the result of severe constraints on congressional action such as interest group, presidential, and bureaucratic pressure.

The empirical analysis of energy policy choice is meant to shed some light on this issue. The analysis is fundamentally interested in explaining policy choice: which policy proposals pass, which do not. The empirical model predicting policy choice is fairly elaborate. A two-stage probit model is used to accommodate the exogenous effects of economic conditions, interest group preferences, and the distribution of costs and benefits as well as the endogenous preferences of relevant political actors (members of Congress, the president, and bureaucrats). The unit of analysis is the wording of passages in proposed legislation having discernible microeconomic consequences for energy producers and consumers.

Where VanDoren shines is in recognizing why the results of his analysis might prove valuable. He is not interested in showing that policy is misguided (that is almost taken as a given) but wants to know if reform is politically feasible. To demonstrate this, he recognizes that he must show that reform is in the interest of members of Congress and that they have the ability (discretion) to implement it.

The answer is not self-evident. The empirical model is complex (relative to other policy choice models) and the results show a myriad of pressures bearing on the policy choices of Congress and that these pressures are sensitive to the issue at hand. VanDoren is quite (unusually) candid about what can and cannot be learned from his results. The one obvious casualty is the ability to generalize directly from this study. The somewhat wide-ranging specification search that precedes the estimation and the sensitivity of the results to particular policies tell a cautionary tale.

The caution called for in interpreting the results is no reason to abandon the approach or, for that matter, the model itself. VanDoren's empirical analysis is preceded by an oft repeated and accurate string of criticisms directed at standard policy studies. At a minimum he provides us with a set of methodological tools for moving well beyond standard (pat) policy models.

Some readers are bound to be upset by the case VanDoren makes for the pursuit of allocative efficiency. The goal of moving from the majority rule principle that governs current policy decision making to something approaching the unanimity of market decisions hinges on the successful implementation of a mechanism for redistributing efficiency gains. The logic is there but the feasibility has yet to be demonstrated. It would be a shame, however, if that proved to be sufficient deterrent to try.

University of Texas at Austin BRIAN ROBERTS

COMPARATIVE POLITICS

Explaining Economic Policy Failure: Japan in the 1969–71 International Monetary Crisis. By Robert C. Angel. New York: Columbia University Press, 1991. 333p. $37.50.

The United States and Japan together comprise nearly 40% of the world's gross national product. And the U.S.–Japanese (NichiBei) economy—the sum total of trade and direct investment between the two countries—now contributes more than $150 billion to both economies. Because of the critical importance of the bilateral relationship, its condition and future health are matters of deep concern. Although some in both countries, mainly government officials, continue to argue that the overall relationship remains healthy, there are enough danger signals to warrant close examination of the political economy of the U.S.–Japanese relationship.

Indeed, the problematic growing economic interdependence between Japan and the United States is only one of the more visible manifestations of the collision between the two most powerful social currents of our time: the globalization of local, national economies and the imperative to preserve or enhance cultural or national identity.

Unfortunately, the intellectual architecture necessary to advance our understanding of how "domestic" and "international" political economy forces interact is underdeveloped. Under such conditions, a number of research strategies are appropriate, among them the case study approach used to answer questions that give promise of providing useful guidance for subsequent work. The goal in such work is to use a particular instance as a search strategy to uncover underlying dimensions that may warrant further study. The work under review more than fulfills this goal by providing an efficient description of how Japan responded to the "dollar shock" issue of 1969–71. The author not only weaves disparate material together to illuminate the particular crisis but also succeeds in making a strong contribution to how we should think about the present and future condition of the bilateral relationship.

The issue Angel selected for study presents similarities and differences between 1969–71 and the present. Then, as now, Japan's current trade account with the United States was in surplus. Then, as now, the United States applied pressure on Japan to respond to American demands for Japan to make changes in its international monetary policies. The demands, then as now, were made because of domestic American difficulties. Japanese decision makers proved inflexible and only unilateral American action forced Japan into changes. However, the domestic problems of the U.S. economy are far greater in the 1990s than the late 1960s; and Japan's current account trade surplus with the United States is far greater, as well. In fact, Washington and Tokyo have been negotiating over trade related issues on virtually an annual basis since the 1969–71 crisis, but to little avail. Even after additional yen reevaluations, wave after wave of reductions of tariff barriers, and now agreements by both countries to reduce structural impediments in their domestic economies for mutual advantage, Japan's current account trade surplus with the United States and the rest of the world increases.

The author focuses on the development and implementation of Japan's international monetary policy in response to American pressure to revalue the yen. Angel selects this issue because he questions the "received wisdom" of economists and political scientists, who tend to see national-level economic and political decision making in Japan as smooth, coordinated, and successful. He sets out to (1) identify the participants in the decision-making process, including what accounted for their access in the decision-making process; (2) identify the positions of the participants and how they arrived at their positions; (3) determine who decided Japan's policy response with what level of influence; and (4) decide how all these factors combined to produce Japan's response.

To answer these questions, Angel places the period before the American challenge to Japan in context. By the late 1960s the Japanese had clearly begun to succeed in their effort to catch up with other advanced industrial economies. But their single-minded focus on export-led economic growth precluded much attention, on their part, to Japan's impact on the other societies Japan traded with. Consequently, when the Nixon administration placed a 10% import tax on all imports entering the United States and called for a yen reevaluation, the Japanese government was unprepared to respond. Angel does an excellent job of tracing the Japanese inability to respond to a need for a reassessment of international monetary policies designed to maximize the ability of Japanese companies to compete abroad. He is convincing in arguing that the institutional structures put in place during the American Occupation appear to have been associated with the pattern of Japanese leaders' becoming comfortable with the idea of using foreign pressure (*gai-atsu*) as a means to undertake important domestic reforms. He also notes the somewhat paradoxical legacy of resentment among Japanese elites left by the "dollar shock" case, which was to be only the first in a line of cases in which American policymakers would make unilateral demands and the Japanese would be forced to respond.

The detailed answers to the questions Angel poses is important for comparative political economy theory development in general and state-centered theory development in particular. Japan, like the United States, is a capitalist state; but the resemblance may end there. Angel reminds us that viewing the concept of the state as a social institution anchored in cultural and historical context, rather than as another, albeit powerful, interest group yields important insights about how capitalist states differ. Angel presents a powerful analysis suggesting why it is unlikely that any particular institution in the carefully balanced system of Japanese decision-making institutions will step forward to deal with American pressures in a positive manner. Angel wrote about the 1969–71 crisis, but his analysis is strikingly apt today.

RAND Corporation ROGER BENJAMIN

Grassroots Politicians: Party Activists in British Columbia. By Donald E. Blake, R. K. Carty, and Lynda Erickson. Vancouver: The University of British Columbia Press, 1991. 155p. $39.95 cloth, $19.95 paper.

A dilemma confronts political scientists who attempt to study party politics at subnational levels. On the one hand, a narrative that captures the richness of local politics may contain so much particularistic detail that it appeals mostly to readers who have a specific interest in the locale. On the other hand, a narrative that casts local politics into a broad theoretical scheme may lack sufficient detail to interest readers who are not well versed in macro theories of party politics. To avoid this dilemma requires a careful balancing of empirical observation and theory. Unfortunately, this carefully researched book ends up impaled on the first horn.

The authors seek to explain the pattern of left–right (New Democratic party [NDP] vs. Social Credit) polarization that provincial politics in British Columbia sustained from the inception of a minority Social Credit government in 1952 through the party's 1986 electoral victory under its newly chosen "populist" leader, William Vander Zalm. What accounted for the nearly 40 years of extraordinary electoral success these two provincial parties enjoyed, a run of victories that virtually excluded the Progressive Conservatives and Liberals—the dominant national parties—from the legislative assembly? As their principal data base, the authors analyze the results of mail questionnaires sent to delegates to provincial leadership conventions in summer 1986 (Social Credit), spring 1987 (NDP), and fall 1987 (Liberals), with response rates of 27%, 48%, and 40% respectively. If we accept the authors' claim that notwithstanding these low response rates, usual for mail questionnaires, those who answered are representative of the entire convention population, we can credit the authors with "the first systematic account of party activists at the provincial level in Canada" (p. i).

Indeed, the authors provide some fascinating details about the attitudes and characteristics of provincial party activists. For instance, Social Credit and NDP activists report feeling closer to their party's central group than do delegates to the Conservative and Liberal national leadership conventions. Social Credit activists support government spending that encourages private economic development but split among themselves over social spending in the public sector. New Democratic party activists support government spending and regulation in both the economic and social sectors. Liberal activists seem to adopt positions that are deliberately centrist. While middle-aged males remain dominant in all three provincial parties, union membership among Liberal and Social Credit activists has declined since the mid-1970s. Over the same period, the NDP became more heterogeneous as trade union membership was maintained at nearly 50% but membership in professional associations jumped from 5% to 39%. Over two-thirds of the Social Credit and NDP delegates had attended at least one previous convention, as had nearly 60% of the Liberals. Most activists had been party members for over five years.

The authors characterize British Columbia politics as a struggle between two viable parties, the dominant Social Credit party and its principal opponent, the New Democrats. The SoCreds emerged from populist elements, many of whom were Albertan expatriates with financial and organizational connections to the then successful Social Credit party of Alberta. They combined with antisocialist supporters of the provincial Conservative–Liberal coalition government that collapsed in 1952. The NDP developed from the British Columbia branch of the Cooperative Commonwealth Federation, a national farmer–labor party that promoted socialism through reform. The British Columbia branch, however, contained "a much higher proportion of Marxist socialists committed to fundamental change" than elsewhere (p. 4). This struggle polarized from the 1950s through the 1980s as antisocialist supporters of the centrist national parties, particularly the Conservatives, had to choose between casting their votes for an apparently weak Liberal organization or for the stronger—albeit more radical—antisocialist SoCreds. By the mid-1980s, when the British Columbia Social Crediters no longer ran candidates for the House of Commons, over 91% of the party activists reported voting Conservative in national elections. In contrast, NDP activists, who are required to affiliate with the national party, reported 99% support of the national party candidates. Despite aid from the national party in maintaining a formal presence in most provincial constituencies, the Liberals had not won a single seat in the provincial assembly since 1975.

Borrowing some theoretical ideas from David Butler and others, the authors suggest that the militancy of party activists stands in the way of a more moderate politics. Activists, especially those on the Left, prevent the sort of convergence at the center predicted by the models of party competition developed by theorists like Anthony Downs. Three-quarters of the NDP activists agreed that "there is too much emphasis on public opinion polls and slick advertising in politics today," but so did two-thirds of the SoCreds activists. Similarly, 50% of the NDP activists and 35% of their SoCred counterparts, disdained being "careful not to move too far to the (left/right)" (p. 130). Nevertheless, the authors conclude that the relative flexibility of the SoCred activists "must put the NDP in an inherent comparative electoral disadvantage" (p. 133).

In the fall of 1991, shortly after this book was published, electoral support for the Social Credit party collapsed. In the aftermath of accusations that Premier Vander Zalm had criminally exploited his position for private gain, Social Credit lost not only its majority in the legislative assembly but even its position as the NDP's major opposition. The NDP, led by its "moderate" leader (and now premier) Mike Harcourt, won the government for the first time since the 1972 election, and the Liberals suddenly returned with enough seats to become the official opposition.

Needless to say, the authors' analyses of the attitudes and characteristics of party activists who attended provincial leadership conventions neither anticipate nor provide satisfactory explanations for this turn of events. While readers cannot expect the authors to have predicted Social Credit's imminent collapse, it seems fair to argue that the authors became too imbued with their provincial data. An analysis of why the SoCreds failed in Alberta, for instance, might have provided a perspective on the long-term viability of the SoCreds in British Columbia. Moreover, a broader comparative perspective might suggest that in the age of mass communication, regional parties in modern democracies will decline unless they foster a distinctive ethnic or separatist platform.

Nonetheless, readers with a special interest in local party organization or in Canadian provincial politics will find this book an informative historical study.

University of Cincinnati MICHAEL MARGOLIS

State Organized Terror: The Case of Violent Internal Repression. Edited by P. Timothy Bushnell, Vladimir Shlapentokh, Christopher K. Vanderpool, and Jeyaratnam Sundram. Boulder: Westview, 1991. 312p. $49.95 paper.

Genocide Watch. Edited by Helen Fein. New Haven: Yale University Press, 1992. 204p. $25.00.

The two books are useful and thought-provoking collaborative studies on political repression, state terror, and genocide, practices that have bedeviled the twentieth century and are likely to continue to claim millions of lives unless major efforts are made to prevent such state-imposed death. Yet the respective editors note that their topics, despite their obvious importance, have been neglected by scholars and the general public. The neglect, denial, and distortion of genocide is of particular importance to the contributors to Fein's book, since their focus is on prevention of the most horrible crime that can befall a people. Without recognition of genocide, and an active constituency to oppose this crime against humankind, they believe genocide will continue. Appropriately, *Genocide Watch* is accessible to the general public yet is rigorously argued, avoiding the excesses of either optimism or pessimism. *Organized State Terror*, on the other hand, focuses on how to explain a variety of repressive and violent acts by the state within its own borders. It is a book by and for social scientists, but its quest for understanding grows out of moral passion.

Organized State Terror consists of 16 papers loosely organized under several headings: a theoretical introduction, the structural sources of state organized terror, terror as an instrument of state policy, and the social and political psychology of state terror. Most of the essays are excellent, providing sophisticated analyses of single case studies (and, occasionally, comparative studies) ranging from Argentina, to Cambodia, to the Soviet Union, to Nazi Germany, to Central America, to the totalitarian ministate of Jim Jones in Guyana. In a very real sense, the strength of the book lies in its parts, rather than in any overall theory of why some states, under certain conditions, resort to mass violence against portions of their populations. Indeed, it may not be possible to provide a single explanation of state terror (not to mention, repression and genocide); for, as the case studies make clear, state terror is not a single phenomenon but, at most, a set of family resemblances. It may be possible, however, to find more general explanations for *types* of repression and state terror. Also, perspectives that help locate likely areas of trouble, without offering an explanation in a full sense, can be helpful. Viewed in this light, the editors' "theoretical template" summarizing the diverse case studies might be acceptable. They, however, make a much stronger claim for it, namely, that it can illuminate the general conditions for the emergence of political repression and state terror.

The model consists of four main conditions of repression and terror: (1) a distorted conception of the state and the relationship between the state and society ("When the state views its monopolization of power and authority as absolute, society's institutions loom larger as potentially competing centers of power. The state then operates to preclude that competition." Again, such regimes are "typically animated by ideologies sharply at odds with reality and by unrealistic ambitions for social and economic control" (pp. 10–11); (2) conflict and disarray within the state apparatus, leading to political rivalries and a rhetoric of absolutes—in short, weakness; (3) deep economic and/or ethnic conflicts; and (4) strong state-dependence on a foreign power, so that if established by a foreign power, the state lacks legitimacy and resorts to repression and terror, and if recipient of large amounts of foreign aid for the military and other security forces, it not only has the means for repression but develops a military outlook that emphasizes security at any cost.

Such a model is suggestive—but misleading in that it presupposes that a single theory can explain all examples of state terror. In another sense, the theory is partially misplaced, since it does not attempt to locate and explain *particular* types of family resemblances within the larger category of state-imposed violence. Nor does it indicate whether the four general conditions specified in the model are, singly or in some combination, necessary or sufficient conditions for state terror (repression, genocide). Further, the model offers few hints about the relations between repression, terror, and genocide. Are these different in kind, locations on a continuum, or causal sequences? Is it the case that all states that resort to terror also commit genocide? If they do not, what has prevented them from doing so? Lack of opportunity, external pressure, the effectiveness of terror itself? Unfortunately, these issues are not addressed by the editors. Nor do they attempt to draw upon their analysis of the causes of state repression and terror to suggest ways to prevent the states selective and mass violence against its own population.

The thoughtful essays in *Genocide Watch* by experts in a variety of fields address a number of issues related to the prevention of genocide. None of the contributors has any illusion about moral considerations' prevailing in the calculations of governments either to commit or to deter genocide. Rather, they call for systematic campaigns by nongovernmental human rights organizations to expose genocidal acts and build a constituency that will pressure governments and international organizations to take steps against perpetrator states. They also call attention to the costs of genocide in terms of international and regional stability and security, underdevelopment, aid for refugees, and famine relief. Fein points out that for governments to ignore genocidal acts tells the perpetrators that they can get away with it and details the relocations, massacres, and gassings of the Kurds in Iraq as an example of how the process works.

Yet as Leo Kuper notes, since genocide is not a "unified phenomenon," the possibilities for prevention differ with the "annihilatory contexts" (p. 157–58). In any case, action that is likely to be successful must come in the early stages of persecution.

In this light, several of the essays focus on how to detect genocide, (distinguishing it from, say, civil war or isolated killings as reprisals) and on how medical, demographic, and testimonial information from refugees can help alert us to what is taking place. The press has a major role to play here; but too often, it has—through perceptual bias, fear of offending a government, and

distrust of accounts of refugees—ignored, distorted, or only briefly covered major genocides, including Cambodia, the Stalin terror–famine in Ukraine, and Burundi in 1972. Fein reports the argument of Richard Cohen, the columnist for the *Washington Post*, that the press and public will take genocide seriously only if the victims are viewed as innocent, there is an American constituency to support and publicize their cause, and the United States is perceived as having considerable leverage.

The essays identify two other problems that affect the possibility of preventing genocide. One is the need for coordination of the various elements in a constituency for protection against genocide. Although this is a persistent weakness among human rights groups, the report in *Genocide Watch* on the campaign to deter Iran from genocide against the Bahá'ís indicates that fruitful cooperation is possible. The other is that of the bystander who looks on and does nothing. Ervin Staub, in his sensitive and thoughtful study, examines the nature and causes of uninvolvement on the part of the many but argues on empirical grounds that it is possible to transform bystanders into more active, responsible citizens.

Genocide Watch is an insightful and practical guide to the prevention of genocide. Surprisingly, however, only a paragraph is devoted to indigenous peoples, groups that are under constant threat of genocidal attacks. Nevertheless, this is a sobering book, one that should be widely read, and then acted upon.

College of William and Mary ROGER W. SMITH

The Revenge of History: Marxism and the East European Revolutions. By Alex Callinicos. University Park: Pennsylvania State University Press, 1991. 159p. $28.50 cloth, $13.95 paper.

The Left Unraveled: Social Democracy and the New Left Challenge in Britain and West Germany. By Thomas A. Koelble. Durham: Duke University Press, 1991. 162p. $34.95.

These books have little in common beyond an interest in the fate of contemporary socialism. While proclaiming the triumph of Western liberalism as premature, Callinicos' book is a plea to take advantage of the "great opportunity" provided by the Eastern European revolutions for the revival of the Marxist tradition that includes Lenin, Trotsky, and the Left Opposition since that tradition is now freed from its Stalinist incubus and betrayals. For Callinicos, European social democracy does not differ significantly from the conservatism of Reagan and Thatcher, whose liberal economic institutions are still subject to the "deep crises of profitability" foreseen by Marx.

While Callinicos sees the death of Stalinism as an opportunity to revive the spirit of Lenin and Trotsky, Koelble, conceding that the European social democracies are perhaps not too different from their conservative rivals, argues that labor parties are healthy insofar as they have rid themselves of most variants of socialism—Bolshevik or otherwise. Indeed this interesting and careful comparison of the New Left challenge of the 1970s in the British Labour party and in the German Social Democratic party (SPD) shows that any successes they may enjoy are partly due to the triumph of the forces of the "social democratic" Right, as against the socialist Left. Koelble's study indicates that such a strategy has enabled most European labor parties to retain their electoral strength and concludes that social democracy is not the end of the road but is "currently the only viable alternative to the conservative project" (p. 130). This adaptive process is one piece of evidence used by Koelble to show that the elites of the European labor parties are responsive to public opinion, that they disprove Michels's theory of oligarchy, and that the parties and their elites have lost ground by either ignoring new constituencies, like the ecology movements, or taken the old ones (especially the unions) for granted.

While, for Koelble, the overall story of these parties is one of a systematic scrapping of the doctrines of nationalization and workplace democracy and a concentration on softening the harder edges of a market economy, Callinicos scorns theories of market socialism and makes a plea for doing away with the party system as we know it. He envisions a revived system of soviets, a system of workplace democracy that should replace the representative institutions of liberalism, of which the party system is an integral part. Noting the frequency with which soviets have appeared in many of the modern European revolutions, Callinicos argues, quoting Lenin, that such a system is not utopian, "is not anyone's invention," but, rather, "*grows* out of the proletarian class struggle as that struggle becomes more widespread and intense" (p. 111). Callinicos is hardly the first theorist to have observed this phenomenon. While he extols the soviets for their "tendency to break down the distance between economics and politics," Arendt's *On Revolution* (1963), a far more extensive and tragic history (to which Callinicos never refers), sees this breakdown as the basis of the soviets' failure once the necessities of economic housekeeping—better done by bureaucracies—replaces the realm of political freedom in the soviets. Callinicos fails to tell us the conditions under which soviets will arise spontaneously at the turn of the century. He quite effectively condemns the complacency of liberalism and the shortcomings of Keynesianism and predicts that Poland and Hungary will fail to integrate with Western economies and, instead, come to resemble Argentina or Brazil. But since strong soviets have not arisen in Mexico, it is quite another matter to hope or predict that soviets will spontaneously arise in Eastern Europe, not to speak of Western welfare states dominated by citizenries of suburban small property holders.

Why should Callinicos pick fights with social democracy by condemning attempts by Nove, Miller, and others to formulate a "market socialism" (pp. 95) when he himself notes his hero Trotsky's support for the NEP, which was characterized by "various combinations of socialist planning and market mechanisms" (p. 120)? To argue, as Callinicos does, that the forces of history necessitated such "transitional" compromises as a strategy for survival is only to concede the prudence of the European labor parties in making similar political calculations.

Finally, if the history of Western working-class organizations is at all instructive, Callinicos' vision of a relatively decentralized system of soviets and trade unions would be as likely as not to accept market forces and be less concerned to use the power of the state to "force" history. Lenin's condemnation of syndicalism, "tailism," and economism affirms this tendency. It is perhaps significant that the author refers disparagingly to the Stalinist "vanguard" (p. 17), while never linking

this term to Lenin or explaining to us adequately why it was Lenin, not Stalin, who was the first to undermine the power of the soviets. Even if soviets were to develop, Callinicos cannot tell us why workers would not be betrayed once again by the state or by an elite of Marxist intellectuals. Whatever Lenin's view, Callinicos' own vision of a soviet future remains utopian.

Koelble's account of the economism of British and West German trade union politics highlights the clash between working-class economism and socialist theory. As Koelble points out, the predominance of British trade unions through a system of bloc membership and voting constituted a large part of the basis of the triumph of social democracy in the postwar Labour party. It was only by ignoring union interests during periods of economic crises and necessary wage constraints, especially by the second Wilson government, that a critical number of unions came to support the New Left calls for controls on the autonomy of the parliamentary party. Since 1983, however, the unions have consistently rejected party executive candidates associated with the New Left and supported the social democratic policies of Neil Kinnock.

The case of the German Social Democratic party is in some respects even more telling. Although by law trade unions are politically neutral and membership in the SPD is individual, the unions, in what can only be called a triumph of economism, rejected ecologist calls for limits on building, on the grounds that they would be harmful to full employment. They regarded the new leftists as cranks and nuisances; and, as Koelble notes, the social democratic countermobilization against them "depended quite frequently on the mobilization of trade union supporters" (p. 92). It is certainly true that much of the thinking in the New Left of both parties cannot be assimilated to the various proletarian ideologies either of old Marxism or of the Western labor movements. Indeed, what are the prospects for a revival of classical Marxism when to one observer, the students and intellectuals of the Left-dominated Young Socialists "wanted to represent the workers but could not understand their language" or, worse, when workers could not understand Young Socialist jargon?

Koelble emphasizes the sometimes agonizing strategic and organizational choices confronting the two parties in the last generation when confronted with the changing constituencies of the various party tendencies and how they dealt with the successionist movements—the Greens on the SPD Left and the Social Democrats on the Labour Right. He addresses the question of why the two parties reacted to circumstances in different ways in regard to coalition building and in regard to responses to very different external events, especially economic conditions. It is a very worthwhile, readable, and valuable study in the best tradition of political science.

Yet we are led to ask whether the work stands as an adequate refutation of Michels's iron law, because the parties responded to outside pressures and to internal bargaining from multiple elites. Michels's (1949) *Political Parties* stipulates that only when elites remain united are they "always victorious" but that when the dominant classes "struck by blindness pursue a policy that strains relationships to the breaking point," the party masses overthrow the party oligarchies (pp. 167, 172). Koelble himself succeeds in showing how Labour blindly ignored shop stewards who precipitated the trade union revolt of the 1970s. By his own account, the party's partial revival is due in part to a ousting of the New Left and the revival of the old PLP–trade union alliance. A similar general conclusion could be drawn from Koelble's account of the SPD, which successfully resisted, through tactics that would have made Machiavelli blush, a New Left takeover (similar to that experienced by the Labour party). In both cases, we are reminded that the more things change, the more they remain the same.

University of California, Riverside JOHN L. STANLEY

Driving a Bargain: Automobile Industrialization and Japanese Firms in Southeast Asia. By Richard F. Doner. Berkeley: University of California Press, 1991. 371p. $39.95.

The role of transnational corporations in Third World economic development is a highly controversial subject, albeit one whose terms of debate are at least coming into sharper focus. Until the early 1980s, there was primarily a "dialogue of the deaf" between modernization theory, which stressed development through laissez-faire economics in which transnational corporations played a key role, and dependency theory, which viewed transnational corporations as the leading actors in perpetuating the "underdevelopment" of the Third World through the exploitative relationships of world capitalism. Since then, the debate has become more nuanced with the realization that significant industrialization in the developing world is possible for both economic and political reasons. Economically, the operation of the "international product cycle" pushes the manufacture of labor-intensive and standardized goods into nations with an abundance of cheap labor. Politically, "strong and autonomous" states in the Third World have been able to exert countervailing power against transnational corporations and thereby to harness their economic activities to domestic development projects.

In *Driving a Bargain*, Richard Doner makes a major contribution to this controversy by presenting a theoretically centered case study of the automobile industry in four Association of Southeast Asian Nations (ASEAN) countries: Indonesia, Malaysia, the Philippines, and Thailand. These ASEAN nations form an excellent basis for a case study of transnation corporate economic power in the "periphery." With the exception of the Philippines, they have had dynamic economies based on export-led drives in the last several decades and have, at least potentially, substantial domestic markets. Transnational corporations, especially Japanese ones, have entered their markets in force. Their governments have shown keen interest in promoting policies of economic nationalism and industrialization. They occupy a distinct niche in the global political economy, trying to follow Japan and the East Asian "gang of four" (Hong Kong, Singapore, South Korea, and Taiwan) up the international product cycle. In respect to the automobile industry, all have had considerable assembly operations generally dominated by Japanese transnational corporations and some automobile parts industries in which local businesses have played a stronger role; and all tried to upgrade their automobile industries by promoting greater local content, industry rationalization, exports, and the role of domestic capital. All experienced some success but fell far short of their stated goals. However, very considerable differences emerged among the indig-

enous automobile industries in these four nations. Thailand's was clearly the most successful, and the Philippines', the least successful, while Malaysia's and Indonesia's occupied an intermediate position.

Doner presents detailed histories of the evolution of the automobile industry in each country, focusing upon such factors as the mix of local and foreign ownership, the differences between the components and assembly industries, the strategies of transnational corporations (especially their efforts to impede "indigenization"), the political strategies and conflicts of local business interests, regime ties to local and foreign capital, and state policy toward the automobile industry. On the basis of these four case studies, Doner challenges two dominant theoretical conclusions about the transnational corporate role in the developing world: (1) that transnational corporate "structural power" in terms of economic resources and almost inevitable alliances with groups in the host country greatly biases bargaining in their favor; and (2) that states that are strong and autonomous from their societies offer the best opportunity for counteracting transnational corporate structural power.

Rather, Doner advocates a "product cycle" approach. The international product cycle, according to this perspective, produces oligopolistic competition among transnational corporations and the diffusion of technologies that provide opportunities for Third World industrialization and the upgrading of production. The considerable differences among the automobile industries of the seemingly similarly situated ASEAN nations, then, demonstrate that these opportunities are certainly not equally utilized. Doner argues that neither national resource possession nor state characteristics (autonomy and nationalistic policies) explain these variations. Instead, the nature of "growth coalitions" between domestic capital and the state appears to be the key factor in promoting indigenous industrialization. Local businesses provide an insider's knowledge of the industry and a technological sophistication that state technocrats do not possess but that are essential in bargaining with transnational corporations. Conversely, state power is necessary to help local capital expand and pursue its goals vis-à-vis the transnational corporations. Thus, growth coalitions based on "reciprocal consent" between the state and domestic business community are necessary to create the economic and governmental flexibility and strength prerequisite to climbing the international product cycle.

Driving a Bargain, therefore, makes an important contribution to the analysis of the political economy of development by strongly indicating the need to move beyond simplistic images. Regime–society relations emerge as the driving force in development, but they work in complex ways and causal chains. Doner demonstrates convincingly that regime–society relations differ dramatically among even generally authoritarian regimes and that what is important for developmental dynamics is the nature, not the absence, of these links. Thus, political and economic factors are inextricably interwoven, and "development" is an ongoing game of pursuing economic and political self-interest within this matrix. Doner's sophisticated and sensitive relating of case histories to theoretical concerns sets a high standard for understanding this truly political economy of development.

Auburn University CAL CLARK

The Military Revolution and Political Change: Origins of Democracy and Autocracy in Early Modern Europe. By Brian M. Downing. Princeton: Princeton University Press, 1992. 308p. $35.00.

A quarter of a century after European state formation reemerged as a central concern of macroanalysts in both sociology and political science, the controversy goes on. This is no gentle disputation among antiquarians but, rather, a hegemonic struggle among ambitious theorists over the interpretation of our own times. Launched by Barrington Moore, Jr. (*Social Origins of Dictatorship and Democracy*, 1966) as a critique of then-reigning modernization theory, the materialist explanation of regime variation itself stimulated a reaffirmation of the primacy of culture as a determinant of political development (e.g., Bendix's *Kings or People*, 1978). But both sides shared an egregious theoretical flaw, identified as long ago as 1902 by Otto Hintze: because state formation in Europe took place concurrently among many states, interactions among them generated processes that shaped the development of each. Any theory contending that "a people's constitution is in effect shaped by its social structure" was therefore incomplete; and he proposed, as an alternative starting point, the notion that the peculiar character of a given state is "co-determined by its relation to its surroundings" (*The Historical Essays of Otto Hintze* [1975], p. 159). The problem was to ascertain "whether the cases of which this is true are isolated and exceptional, or whether they can be organized in groups and presented as evidence of a typical regular pattern" (p. 160, n. 24).

The theoretical task Hintze delineated is still with us. Although the twentieth century provided plenty of grist for his theoretical mill, by and large, the social sciences remained stubbornly "internalist." In our own discipline, Harold Lasswell's seminal notion of the "garrison state," formulated on the eve of World War II as a "developmental construct" very much in keeping with Hintze's approach, was never incorporated into mainstream theory. However, the internalist hegemony began to wane in the 1970s as a result of two quite disparate challenges. One was "dependency," which argued that a country's economic structures were determined in large part by processes generated at the level of the international political economy. The other stemmed from a congeries of scholars who emphasized war—and strategic interaction among states, more generally—as a major determinant of state formation. Coinciding in time with the exhumation of Hintze, the major work in the latter category was Charles Tilly's compendium, *The Formation of National States in Western Europe* (1975), including not only the editor's own well-known introductory essay but also Samuel Finer's more ambitious attempt to conceptualize military-induced mobilization of human and fiscal resources as a variable and to explore the political consequences of this variation.

Although, in the intervening period, analysis of the war factor has been further elaborated by Tilly and a number of others, Brian Downing lifts us beyond the exploratory phase to a more mature theoretical plane by setting forth a proto-theory of how it contributed to the determination of fundamentally different political outcomes. Although he ignores Finer's contribution, his conceptualization of the factor in terms of resource mobilization is quite similar. But whereas Finer overly elaborated the manifold aspects of the process to the

point of producing a jumble from which it is difficult to extract workable hypotheses, Downing adopts the opposite strategy of drastic simplification for purposes of theory construction. However, unlike many other practitioners of comparative historical analysis, he is clearly aware that it *is* an oversimplification and accordingly goes on to examine the historical circumstances that round out the case. The result is an eminently satisfying marriage of theory and history that makes this book as important from a methodological perspective as from a theoretical and substantive one.

Downing's theoretical objectives are highly ambitious. As indicated in his subtitle, he purports to provide nothing less than an explanation of the "origins of democracy and autocracy." His target is clearly Barrington Moore himself and thereby (as for Hintze in 1902) dialectical materialism in its classic form: "I shall argue that constitutional arrangements predating modernization and military dangers in the newly forming state system were at least as important as agrarian relations in determining political outcomes in Western Europe" (p. 9). Remarkably, the contender has obtained from the champion an acknowledgement of his victory by way of a cover testimonial saying, "This book takes a big step beyond my *Social Origins of Dictatorship and Democracy*."

The first element of Downing's theory refers not to the war factor proper but, rather, to medieval constitutionalism as a baseline condition for the emergence of liberal democracy in Europe. This proposition can hardly be gainsaid, but one must take issue with his insistence that the determinative role of constitutionalism "has been largely ignored by almost all modern social scientists" (p. 5). While Downing himself excepts Randall Collins, Samuel Huntington, and Gianfranco Poggi from the charge of ignorance, many others come to mind, including the late Stein Rokkan; Reinhard Bendix; in the Marxist camp, Barrington Moore himself, who drew from Weber the importance of medieval cities and the bourgeoisie; and Perry Anderson, who emphasized the "detotalization of sovereignty" as a characteristic feature of the West. To these should be added just about everyone writing on the political development of the relevant countries (e.g., the contributors to Raymond Grew's *Crises of Political Development in Europe and the United States*, 1978). Has Magna Carta disappeared from textbooks on British politics?

Downing then proceeds by way of a sort of multivariate analysis, somewhat along the lines attempted by Rokkan, but with a sharper focus on one critical period (the seventeenth century) and six major cases (Brandenburg–Prussia, France, Poland, England, Sweden, and the Dutch Republic). Medieval constitutionalism, as well as the military mobilization factor are treated largely as dichotomous variables, strong or weak. The stronger the constitutionalism, the better its chances of surviving, at least in part; the military mobilization pressures unleashed in the seventeenth century (France and Sweden, as against Prussia). Conversely, constitutionalism's chances of survival are much better if the pressures on it are weak (England) than strong (France). It goes without saying that where medieval constitutionalism is extremely weak or altogether absent (as in Spain, Prussia, and Russia), prospects for the emergence of liberal democracy are practically nil.

But, as noted, Downing shuns reductionism and allows that a number of factors may intervene to break the nexus between war and the destruction of constitutionalism. They include the possibility of mobilizing foreign, rather than domestic, resources (Sweden); lightening of the load by way of alliances (the Netherlands in their war against Spain); the advantages of advanced economy and commercial wealth, if it is in the hands of the merchants (as in Venice or the Netherlands); and, finally, geography and natural topographical features (Switzerland, England, and the Netherlands are easier to defend than France). While there is no gainsaying the significance of these factors, they are brought in on an ad hoc basis and hence lack theoretical import. Although Downing adumbrates possible patterns (e.g., commercial–maritime states are more likely to engage in alliances), he shies from a more systematic account.

Although Downing's framework does not produce startling revelations, it enables him to construct a coherent, theoretically grounded narrative that covers most of the relevant universe and respects historical accounts. This is a major achievement, which points the way to the next theoretical step, namely, an articulation of state formation with the dynamics of the international system of states into a comprehensive body of theory. That is no mean task, not only because weighty configurational elements such as geopolitics are inherently difficult to model but because of interactivity. The war factor is shaped not merely by a given state's responses to exogenous events but also by its own activities at the international level; and these are themselves determined in good part by the general character of the regime. Spain, after all, *chose* to carry out a protracted war against the Netherlands, and Louis XIV *chose* to strive for European hegemony in the late seventeenth century. Were not these projects and the resulting load they imposed on the societies in question a *consequence* of absolutism?

New School for Social Research ARISTIDE R. ZOLBERG

The Struggle for Democracy in Chile, 1982–1990. Edited by Paul W. Drake and Iván Jaksic. Lincoln: University of Nebraska Press, 1992. 321p. $45.00 cloth, $19.95 paper.

Political Structures and Democracy in Uruguay. By Luis E. González. Notre Dame: University of Notre Dame Press, 1992. 224p. $26.95.

These two books make an important contribution to the literature on democratization by focusing on the two countries with the strongest democratic traditions in Latin America prior to their 1973 coups. They are concerned, however, with two different questions. The book edited by Drake and Jaksic focuses on the problem of transitions from authoritarian rule, while González's book is primarily about the prospects of democratic consolidation in Uruguay since 1985. In spite of this difference, there is an interesting convergence between the theoretical approach advanced in both books.

The Struggle for Democracy in Chile, 1982–1990 picks up the story of Pinochet's Chile where J. Samuel Valenzuela and Arturo Valenzuela's edited volume *Military Rule in Chile* (1986) left off. It consists of 10 articles, written by an outstanding international group of country specialists, that concentrate on the changes both within the authoritarian regime and in society in the wake of the first serious demonstrations against the Pinochet-led regime

in 1983. The accounts go through the inauguration of a democratic administration in March 1990. The introduction by the editors outlines in broad strokes the transformations between 1982 and 1990. A superb article by Arturo Valenzuela describes the institutional structure of the regime, highlighting the personalistic control exercised by Pinochet. Varas's essay, focusing on tensions within the regime, supplements this account. Two more essays round out the study of the authoritarian regime. Campero's focuses on the role of a firm group of supporters of the regime, that is, the entrepreneurs. Silva's focuses on the government's response to the 1981–83 economic crisis. The next set of three essays studies different aspects of the opposition to Pinochet, including the role of women (María Valenzuela), trade unions (Angell), and political parties (Garretón). The volume closes with a look at external factors (Portales) and the economic legacies inherited by the new democratic government (Larráin).

In *Political Structures and Democracy in Uruguay*, González, a Yale-trained Uruguayan political scientist, provides an analysis of the effects of politicostructural factors on democratic stability. He briefly describes the historical evolution of the party system and the executive branch and explains the 1933 and 1973 breakdowns of democracy. Thereafter, the bulk of the book analyzes the prospects of democratic consolidation in the new democracy restored in 1985. Central to the elaboration of this analysis are two surveys, which constitute the book's main empirical contribution. One is a 1985 public opinion survey, the other, a 1986 survey of legislators, which allows González to describe the complex nature of party factions and the attitudes of political elites. The book's central argument is that the 1973 breakdown was influenced by the political structures that had been in place for over a half a century, namely, by the fractionalization of the major parties and the "quasi-presidential" system. Concerning the prospects of democratic stability after the 1984 election, which brought an end to military rule, González argues that the factors leading to the 1973 breakdown have, for the most part, remained in place through 1990. Because the 1984 election saw a true restoration of the party system, there was a resurgence of many of the problems that facilitated the 1973 coup. The analysis, in short, leads to a negative reading of Uruguay's democratic future.

Theoretically, these two books exemplify what could be called a "political-institutional" approach to politics. The need to stress purely political factors is most explicitly elaborated in González' book, which resembles Arturo Valenzuela's *Breakdown of Democratic Regimes: Chile* (1978) in advancing a classically Linzian analysis through a case study on democratic stability. The nature of the electoral system and the party system, along with the nature of the executive power, are thus viewed as political structures that actors strategize in and that have a crucial impact on the viability of democracy. Furthermore, searching for a solution for Uruguay's uncertain democracy, González presents an argument similar to Linz's concerning the problematic effects of presidential systems from the perspective of democratic stability. The best remedy, in short, is seen as a shift to a parliamentary system. The problem, then, is that the necessary institutional changes face impressive obstacles. The issue of democratic agency, thus, is seen as going beyond the commonsense notion that political actors do things that undermine democracies, to include the broader question of how to transform political structures in such a way that they should ensure the apparently contradictory goal of democratic stability. The democratic challenge is to shape a political arrangement that allows and invites political participation while securing its stability against corrupting tendencies. In focusing on the actions of political elites and considering the strategic options they face, the book's conclusions are quite pessimistic in the end.

González's contribution, in short, is to have provided a detailed study of an understudied case and framed it in terms of the ongoing debate among comparativists, spearheaded by Linz and Lijphart, on the political institutions of democracy. The book's main shortcoming is that the explicit focus on political elites is not balanced through the study of actors in civil society. As in much of the "political-institutional" literature, questions about the type of linkage between political parties and social actors (e.g., business, labor, and various social movements) are never addressed. An issue as basic to democratic theory as whether there are actors with well-defined interests that can then be represented, therefore, simply does not emerge.

If González's main concern is with democratic stability, the volume edited by Drake and Jaksic focuses on the other side of the democratic question. Rather than considering how to stabilize a new democracy, they study the antecedent process of moving away from authoritarian rule. Their concern is thus with factors that lead to the destabilization and removal of an authoritarian regime. Along the lines of O'Donnell and Schmitter's classic *Transitions from Authoritarian Rule* (1986), this book shows that in transitional periods, actors seem to enjoy a greater scope for choice than in periods of "normal" politics. Also consistent with most of the transitions literature, the number and type of actors considered in Drake and Jaksic's book suggests that in fluctuating scenarios, a broad range of actors engage in political action. The plurality of actors, nonetheless, converge into neatly divided camps, which imprint, to a greater extent than in democratic systems, a peculiar state–opposition dialectic upon the process of transition.

In contrast to O'Donnell and Schmitter, however, action and reaction are viewed as framed by institutional factors or political structures. Thus, in a manner consistent with González' approach, Arturo Valenzuela and Garretón, in particular, among the contributors, show how the strategies of various actors cannot be understood outside of the institutional characteristics of the regime. It follows from this that transitions are in part determined by the peculiar institutional features of the preceding authoritarian regime. Again contra O'Donnell and Schmitter, who sought to find the common elements shared by all transitions, this approach leads to a highlighting of variations in the process of transition. This emphasis on strategic actors operating within political structures, in short, contributes to a key aim of comparativists, namely, to distinguish between different "modes of transition." The next question in comparative analysis, however—the possible links between variations in the transitional process and the problems and prospects of democratic consolidation—is beyond the scope of the Drake and Jaksic volume. The research of this question, moreover, would necessitate a move beyond these two works. It would require an explicitly comparative study, and it would demand consideration of cases both in their phase of transition from authori-

tarian rule and as new democracies. These two books, nonetheless, are essential readings for the advancement of such a research agenda.

University of Illinois, Urbana–Champaign GERARDO L. MUNCK

Revolutions of the Late Twentieth Century. Edited by Jack A. Goldstone, Ted Robert Gurr, and Farrokh Moshiri. Boulder: Westview, 1992. 395p. $55.00 cloth, $19.95 paper.

Autocracy, Modernization, and Revolution in Russia and Iran. By Tim McDaniel. Princeton: Princeton University Press, 1991. 239p. $29.95.

Under the impact of the world's enormously varied violent transfers of state power since 1945, political analysts are finally relinquishing an old illusion, namely, that revolutions are singular phenomena for which it is possible to state a single invariant set of necessary conditions, sufficient conditions, and internal sequences. As with the abandonment of many old, cherished beliefs, the new apostates are not always aware of how heterodox they have become; and those who do know often only whisper their unbelief. So it is with Tim McDaniel, Jack Goldstone, Ted Gurr, and Farrokh Moshiri. They broadcast many other messages but mute their apostasy. In vivifying a view of revolution as contingent and various, they have nevertheless mortified the classic sociology of revolution.

At first glance, Tim McDaniel looks like one of the old believers, arguing (as he does) the common properties of revolutionary processes in Russia and Iran. In earlier work, he presented the Russian Revolution as an outcome of contradictions between state-initiated capitalist industrialization and the maintenance of czarist autocracy. Now he claims that the revolutions of 1917 and of 1979 both originated in autocratic modernization, a path to the modern world Barrington Moore's "conservative modernization" did not quite capture. Unlike Germany's Junker-based authoritarianism, says McDaniel, the prerevolutionary Russian–Iranian drive toward industrial prosperity rested on the arbitrary intervention of a divinely sanctioned or self-sanctioned ruler in a country's economic affairs. Thus, the conditions for revolution in those two countries differed deeply from those that prevailed in other kinds of states, where either old elites or the new classes formed by capitalism made their weight felt.

In a dualism resembling Samuel Huntington's portrayal of Western revolutions, that intervention simultaneously undermined the old bases of stability and inhibited the creation of new ones. Autocratic modernization hollowed out society, leaving a space into which disorder rushed. To present this view, McDaniel makes an unfortunate choice, writing a series of essays to emphasize differences and (chiefly) similarities between Russia and Iran, rather than providing two separate sketches, each linked internally by a coherent causal analysis. The result is a provocative set of comparisons, rather than a compelling parallel analysis of revolutionary processes.

In order to make his analysis plausible, McDaniel offers some dubious historical judgments. He greatly exaggerates, for example, the "hallowed tradition" and "divine sanction" of Russian autocracy, forgetting that 14 serious pretenders gained popular support during the seventeenth century, that great families attempted to place their own kinsmen on the throne repeatedly when a disputable succession or a weak ruler arrived, and that a half-dozen czars died at the hands of assassins. In the case of Iran, he minimizes the importance of *bazaaris* and mullahs as connected social elites before and during the revolution of 1979, treating the bazaar's tightly linked merchants as residues of the past, rather than constituents of modern Iranian social life.

McDaniel likewise shows surprising indifference to geopolitical, fiscal, and military effects, to which the works of Theda Skocpol should have sensitized him. On the Russian side, for example, the Russo–Japanese war and World War I disappear as effective causes of revolution. On the Iranian side, we read almost nothing about the shah's close alliance with the United States, except that Jimmy Carter's promotion of human rights made the monarch more vulnerable to claims from dissidents within Iran. Skepticism in these regards does not automatically vitiate an analysis of the Russian or Iranian revolutions. Still, others have made strong enough cases for the importance of such external links in these, and many other, revolutions to warrant the skeptic's ruling them out explicitly, rather than simply ignoring them. Despite the blind spot, McDaniel's interesting version of Alexis de Tocqueville, Emile Durkheim, and Samuel Huntington offers an important challenge to the next round of revolutionary analysts.

When they are speaking most generally, Gurr, Goldstone, Moshiri, and their contributors voice ambivalence about the universality of revolutionary processes. They argue that since 1945 the international situation has changed so much that revolutions occur in different kinds of countries, with different mobilizing claims, and under different circumstances. Yet they also argue that "all revolutions proceed through three phases, beginning with a state crisis, followed by a struggle for power, and efforts by the eventual victors to reconstruct the state. Moreover, no revolutionary crisis occurs until there is a conjunction of three necessary conditions: a crisis in the old state, alienation of a significant segment of the elite, and mass mobilization" (p. 325). The apparent contradiction disappears, however, when we recognize that the universals follow tautologically from the definition of revolution as "the forcible overthrow of a government followed by the reconsolidation of authority by new groups, ruling through new political (and sometimes social) institutions" (p. 37). In fact, the bulk of the Gurr–Goldstone–Moshiri book faithfully documents the diversity of revolutionary conditions, processes, and outcomes in the contemporary world, as well as their difference from revolutions of the past.

The compendium of cases will serve students of twentieth-century revolutions well: Vietnam (H. John LeVan), Nicaragua (Dévora Grynspan), Iran (Farrokh Moshiri), Poland (Jaroslave Piekalkiewicz), Afghanistan (Anwar-ul-Haq Ahady), the Philippines (Richard J. Kessler), Cambodia (Barbara Harff), Zimbabwe (James R. Scarritt), South Africa (C. R. D. Halisi, Patrick O'Meara, and N. Brian Winchester), the Palestine Liberation Organization and Intifada (Joshua Teitelbaum and Joseph Kostiner). All are reflective, informative, and connected somehow with the Goldstonian scheme presented at the book's outset. Introductory and concluding essays by various combinations of the editors neatly stitch the book and its cases together.

The book disappoints, nevertheless, precisely where

its conceptual preferences should have made it strong, namely, in specifying how the international system changed the conditions affecting revolutions after 1945—for example, the links (however indirect) between the Cold War and the rise of nationalist revolutions. Those links inevitably appear, disguised, in the accounts of individual countries, as in Richard Kessler's treatment of Ferdinand Marcos's connections to Ronald Reagan and George Bush. Jaroslaw Piekalkiewicz' analysis of Poland likewise inevitably takes up Soviet intervention. Yet despite acknowledging that in all cases "international factors have contributed directly or indirectly to state crisis" (p. 331), the Gurr–Goldstone–Moshiri "comparisons and policy implications" deal almost entirely with domestic politics. These authors provide no reasoning that leads to inferences concerning the impact of the Soviet Union's disintegration (or the European Community's integration) on the prospects for revolution in Europe or elsewhere. The theoretical revolution, it seems, has not quite completed its work.

New School for Social Research CHARLES TILLY

Red Vienna: Experiment in Working-Class Culture, 1919–1934. By Helmut Gruber. Oxford: Oxford University Press, 1991. 270p. $29.95.

In this work, Gruber has chosen to study a fascinating experiment in which politics and culture were linked in an effort to create social change. Picking up where Carl Schorske's *Fin-de-Siecle Vienna* (1980) leaves off, the book begins with an insightful description of Vienna as a socialist laboratory—a thriving metropolis ready for social, political, and cultural experiments. Gruber then proceeds to discuss the policies and programs implemented by the socialists between 1919 and 1934. These programs involved everything from reforms in public housing and social services to dramatic attempts at redefinition of family life, community spirit, artistic taste, and use of leisure time. He examines the real accomplishments of these programs, as well as the cultural and political obstacles which ultimately undermined the Viennese experiment.

The core of the book is Gruber's analysis of the Viennese socialist cultural programs. Here he brilliantly discusses the elite culture and cultural theory, the power of the written and spoken word, the attempts to enrich and ennoble the workers' artistic taste, and the inevitible conflicts arising from the desires of the party leaders versus those of their working-class supporters. His interpretation is balanced and insightful, acknowledging that although serious problems existed in the conception and implementation of policies, red Vienna actually was quite successful in implementing several innovative and positive social reforms.

Gruber's book is exhaustively researched and well written. Taking almost five years to complete the manuscript, Gruber has managed to assemble a wealth of material from a variety of sources, including new documentation from Viennese archives and university institutes. Of particular interest is his research regarding women workers. Even though women made up approximately 40% of the labor force at the time, few treatments of this period have dealt with the recruitment of women to the socialist party and the party's attempt to address women's issues as part of their cultural programs. Therefore, Gruber's research and analysis are a very welcome addition to our understanding of this subject.

Overall, this work skillfully integrates political, social, and cultural history. The book represents an important contribution to the study of Austrian social democracy and provides a useful case study for students interested in cultural politics, urban renewal, and social reform.

Hamline University KAREN J. VOGEL

Authority without Power: Law and the Japanese Paradox. By John Owen Haley. New York: Oxford University Press, 1991. 258p. $37.50.

Political Life in Japan: Democracy in a Reversible World. By Takako Kishima. Princeton: Princeton University Press, 1991. 142p. $27.95.

Few studies of Japanese politics have yet penetrated into the mainstream of comparative politics. Too frequently driven by the desire to explain yesterday's headlines using a Japanese terminology completely incomprehensible to non–Japan specialists, most works on Japanese politics remain theoretically barren and therefore, for most comparativists, are consigned to those small compartments of their brains reserved for "exotica." This has begun to change, fortunately, in the area of political economy. There, for at least a decade, both Japan specialists and non–Japan specialists have been engaged in a theoretically rich and highly informative set of exchanges. It remains to be seen whether that experience will be carried forward into other dark corners of Japanese politics; but clearly the burden is on the Japan specialist to create works that become empirically and theoretically indispensable to nonspecialists.

The two books under review endeavor to reach such a wider audience, albeit with varying results. On the surface, they would appear to have little in common aside from the fact that both deal with Japanese politics. Yet essentially, both argue that the surface-level order that seems so pervasive throughout Japan is ultimately quite precarious. Haley, drawing on a range of secondary literature, offers a lengthy, historically rich examination of the development of Japanese law. Japan, he argues, appears to have a centralized state with a high level of authority that enables it to intervene in all areas of life. Yet, in reality, the government's de facto capacity to coerce and compel is remarkably weak (p. 14). Consequently, Japanese governments from prefeudal time to the present have continually been forced to rely on "extralegal, informal mechanisms of social control as a means for maintaining social order with a concomitant transfer of effective control over the rules and norms that govern society to those who are able to manipulate these informal instruments of enforcement" (ibid.). In short, the real power of social control rests with families, villages, firms, and other small social units.

Kishima is also concerned with the power of those below. But in contrast to Haley, she is far more concerned with demonstrating the applicability of poststructuralist theories to Japanese political life than with exploring inherently problematic aspects of the Japanese experience. Drawing heavily on Foucault and the vocabulary of deconstruction, her short book offers mostly firsthand examinations of what she calls "marginal beings and liminal states" in contemporary Japanese politics (p. 22). Doing so, she contends, allows her to

illustrate the "human, non-reified interactions among political players." In practical terms, this translates into an examination of several cases: a parliamentary Finance Committee chairman's subtle and informal human relations massaging of the committee's members; an examination of the "orgiastic atmosphere" of an election campaign headquarters; nonsensical dancing and play acting by politicians at the dinner party; a tearful presentation by former prime minister Nakasone before the ruling Liberal Democratic party's executive board; and portraits of three of that party's officials seen as outside "tricksters," defiant of established political rules. This combination of microstudies, she contends, illustrates how Japanese politics can take on subjective meaning to its participants and, more importantly, how such activities provide a basis for "dynamic, progressive, and irreversible social change" in the country at large (p. 14).

Haley's book is by far the more explicitly comparative. Indeed he uses the comparative method to take direct aim at a number of longstanding cliches about Japan. Perhaps his most important target is the notion that the United States offers some logical comparative standard by which to measure Japan. In law, as in most other areas, he notes, the two countries have far less in common than Japan has with most parliamentary systems of Western Europe. In this context he also makes a good case against the notion that Japanese are culturally nonlitigious. Rather, he shows, the country's limited number of lawyers, judges, and procurators make the Japanese court system one of Japan's least efficient organizations. Why bring a case when it may be a decade before it is decided?

In addition, Japanese crime rates are applaudably low and declining in comparative terms; but little of this, he demonstrates, is the result of tough cops, tight laws, and merciless judges. Instead, almost infinite discretion by police, procurators, and judges is the rule in Japan; and this is aimed at "correction" of criminal behavior, rather than its punishment. Japanese-style correction involves engaging the criminal, the victim, and their respective social organizations (usually family or firm) in the process of creating remorse and restitution by the criminal, which in turn reduces recidivism in Japan to admirably low levels.

Still another major thread in Haley's study involves the demonstration that while their formal powers appear to be great, contemporary Japanese bureaucrats derive most of their formal powers from rules put in place by the U.S. Occupation, not from any Japanese tradition of statism. Moreover, he argues that bureaucrats rarely have the power simply to give orders to the objects of their alleged control: "The formal regulatory powers that Japanese administrations do exercise are . . . notably weaker than those of comparable governmental agencies in other industrial democracies" (p. 159). Instead, he argues (following Richard Samuels), most typically bureaucrats must bargain to ensure compliance with their goals.

Finally, in a very compelling interpretation, Haley contends that the power of Japan's organized crime families reflects the general weakness of authority in Japan, demonstrating instead the importance of autonomy and self-policing by powerful organizations. Ultimately, therefore, he contends that Japanese law "serves as a means for legitimating norms while it remains relatively ineffective as an instrument of coercive control" (p. 200). And the norms (at least in the many marginally legal areas of Japan where organized crime families are influential) are those that favor allowing the criminals to police themselves.

Haley's book is by far the easier of the two to follow in its central argument. With its one central thesis, it musters a series of easily comprehended examples as illustration and thus resonates well with existing standards of social science. The empirical examples in Kishima's book, on the other hand, are more sparse. While generally interesting and informative about the day-to-day human aspects of Japanese politics, most of her examples are presented principally to illustrate a much more ponderous theoretical architecture that in the end remains far from compelling. Delightfully humane instances of human interaction are freighted with the heavy deconstructionist burden of proving far more. Perhaps, for those so inclined, the cases offer major new insights into human self-fulfillment and social change; to others they will be far more valuable as portraits that show in convincing human terms that Japanese politicians are not terribly different from their counterparts in other political venues.

Commitment, or noncommitment, to the deconstructionist paradigm will be the ultimate test of how Kishima's book is received. Poststructuralists will surely find it a fine example of the cross-national applicability of their theory. Doubters are less likely to be convinced. Haley's book, on the other hand, has by far the greater chance to achieve broad appeal, because it engages a series of issues that both are currently very salient to a broad spectrum of scholars and at the same time involve central puzzles that are applicable in any industrialized country. It is in that direction, surely, that the future integration of Japanese studies into comparative politics must lie.

University of Colorado, Boulder T. J. PEMPEL

Behind the Intifada: Labor and Women's Movements in the Occupied Territories. By Joost R. Hiltermann. Princeton: Princeton University Press, 1991. 265p. $29.95.

The Palestinian Uprising: A War by Other Means. By F. Robert Hunter. Berkeley: University of California Press, 1991. 292p. $24.95.

Both of these studies examine the origins and staying power of the Palestinian Uprising that broke out suddenly in December 1987 and continues in various forms today. Both authors contradict two commonly held but mistaken views of the Intifada, namely, that it was a spontaneous outburst of the masses lacking both organization and leadership and that it was the work of local agents of the Palestine Liberation Organization following orders from outside the occupied territories. These explanations assumed that the revolt lacked strong roots in Palestinian society and that it would exhaust itself after an initial burst of unguided fury. Instead, Hiltermann and Hunter persuasively portray the Uprising as the culmination of a decade-long process of association formation and political mobilization that transformed the character of the West Bank (and to a lesser extent the Gaza Strip), despite repeated crackdowns by Israeli military authorities.

Both authors highlight the years of institution building that preceded the Intifada, carefully tracing the

emergence of a diversified network of voluntary groups among workers, students, women, merchants, professionals, civil servants, villagers, Muslims, and Christians. Although these groups did not initiate the uprising, they all joined in quickly providing it with organization, leadership, and coordination. In turn, the Intifada gave many struggling and fragmented groups an infusion of new members and a common set of goals. Before long, the art of association became so closely intertwined with nationalist protest that they were virtually indistinguishable.

Hiltermann and Hunter agree that the growing vigor and resourcefulness of Palestinians' associational life has had far-reaching consequences in and beyond the Occupied Territories. The Uprising was sustained and periodically renewed despite recurrent pronouncements that it had run its course. A whole new tier of "inside" leadership emerged alongside the old guard of the PLO, strengthening Yasser Arafat's claims of preeminence while pushing him toward the more urgent pursuit of a negotiated settlement and a two-state solution. Israeli public opinion was forced to reconsider the costs of continued occupation and the temptations of creeping annexation. American diplomacy was given yet another opportunity to take up the unfinished business of the Camp David accords.

Most important, both writers interpret this florescence of associational activity as convincing proof that Palestinians in the Occupied Territories are capable of representing themselves. Hiltermann and Hunter share remarkably similar views of the historical significance of this change. Their conclusions are nearly identical on four major issues.

First, rapid group development signifies at least the partial resuscitation of civil society in the Palestinian heartland. After experiencing multiple traumas, the sinew and tissue of community have grown back to the point where they can support a stronger awareness of common sentiments and a more complex division of labor. Although neither author uses Durkheimian vocabulary, they furnish vivid descriptions of a society where both "mechanical solidarity" and "organic solidarity" are increasing at the same time.

Second, voluntary groups have become the nucleus of the Intifada and the base of a more powerful national liberation movement. Most groups are allied with one of the competing factions that have fragmented Palestinian politics for decades, particularly with Fatah, the Democratic Front for the Liberation of Palestine, the Popular Front for the Liberation of Palestine, the Communist party, and various Islamic groups. At times, this linkage encouraged the proliferation of weak associations, squabbling over the same limited membership. On the other hand, these same connections also invested interest group leaders with important leverage in fashioning new alliances between the factions. Association leaders in the Occupied Territories not only took up key positions on the steering committee of the Intifada, they also pressured the factions to cooperate with one another on the "inside" and the "outside." The result was a more unified PLO with a wider mass following but also with greater responsiveness to constituencies it formerly took for granted or sought to control.

Third, the group network is frequently described as the blossoming infrastructure of a future Palestinian state. This may be the boldest and most debatable conclusion of both books. It is one thing to argue that the "institutionalization" of the Uprising symbolizes a maturation of Palestinian society and a deepening of its nationalist consciousness. It is quite another thing to suggest that these organizations comprise a state-in-the-making.

Hiltermann and Hunter leave little doubt that the people of the Occupied Territories have cultivated the art of association well enough to represent themselves in several arenas at the same time—locally, regionally, and internationally. Indeed, their achievements have far surpassed the expectations of the PLO and Arab patrons who financed community organizations with every confidence that they would serve their sponsors' interests more than their own. Nevertheless, the authors engage in wishful thinking when they suggest that a vigorous associational life is a way station on the road to an independent state. Yet even this exaggeration has merit if it alerts us to the possibility that a Palestinian state might be politically pluralistic and competitive and thus not easily manipulated by either its leaders or its neighbors.

Fourth, both writers agree that the Intifada is not a social revolution within the Palestinian community. Rather, they note that pressures to sustain a united front during the uprising have postponed demands for social justice accumulating for years among workers, women, youths, and the poor. Hiltermann is much more critical and partisan than Hunter on this issue. Hiltermann frequently disparages the conservatism of Fatah and the "bigotry" of Islamic groups while praising the progressivism of the PLO leftists and the communists. He is concerned that the uprising may actually reinforce some of the most repressive aspects of Palestinian culture. Hunter, on the other hand, notes a more benign desire to preserve a wide range of popular traditions, including customary law, arts, and religion. Hunter gives an informed and subtle account of the contribution of the Islamic movement to the Intifada that is refreshingly free of the usual polemics and stereotypes.

Both books are valuable supplements to earlier studies of West Bank and Gaza politics by writers such as Emile Sahliyeh and Ann Lesch, who focused more on elites and officeholders than on associations. Hiltermann is a sociologist who concentrates on the details of the labor and women's movements whereas Hunter is a historian who tries to situate social change in grand contexts, temporal and international. Neither study contains much in the way of social theory. Both are highly readable, mainly journalistic accounts that wisely allow many interviewed participants to speak for themselves. Hiltermann's voices are mostly left-wing organizers, whereas Hunter's are mostly fellow university professors and intellectuals. I strongly recommend listening to both choruses.

University of Chicago ROBERT BIANCHI

The Political Economy of National Security: A Global Perspective. By Ethan Barnaby Kapstein. Columbia: University of South Carolina Press, 1991. 252p. $39.95.
The Political Economy of Defense: Issues and Perspectives. Edited by Andrew L. Ross. Westport, CT: Greenwood Press, 1991. 240p. $47.95.

The combination of the Reagan military buildup and its consequences and the ending of the Cold War has inspired a recent burst of publications on the political

economy of national security. *Political economy* is a fashionable term these days and, as such, has become difficult to define. It encompasses quite a broad range of research in both approach and substance. At times, it includes far more than it excludes. Within the broad and somewhat nebulous category of political economy, what is the political economy of national security? To judge from these two volumes, the political economy of defense includes any analysis of the interactive relationship between politics and economics in the area of national security. For Ethan Kapstein, at the core is the fundamental and enduring question of nations' simultaneous pursuit of security and prosperity. According to Andrew Ross, who edited *The Political Economy of Defense*, it encompasses three areas of research: "(1) the political dimensions of economic actions in the defense realm; (2) the economic dimensions of political actions in the defense realm; and (3) the security dimensions of political economic actions" (p. 8). Despite the apparent parity among the three dimensions, the fundamental and dominant concerns are largely economic, including the economic consequences of defense spending, the economics of international arms trade, defense industries, fiscal constraints, and defense planning.

These two books share more than an emphasis on political economy. Both volumes are comparative and international in approach. The Kapstein text explicitly argues that the political economy of national security can only be understood as an interaction of national and international forces and that this is increasingly the case in an interdependent world economy. It also uses comparative cases throughout, from both developed and developing countries. The chapters in the Ross volume, while sometimes focused on one country, tend toward comparative and international analysis, as well.

While the books begin from common concerns and subject matter, they diverge in their merit as scholarship and in their utility for the classroom; that is, the Kapstein volume is a relatively succinct textbook, while the Ross volume is an edited reader, and each enjoys the benefits and drawbacks of their respective formats. The Kapstein book is comprehensive in scope but of rather shallow depth. The Ross volume has sufficient depth in many chapters but is like many other edited volumes a somewhat eclectic selection of topics and contrasting approaches.

As a textbook, the Kapstein volume is a solid piece of work. It is nearly up to date (the best that can be expected of anything published today in international relations), comprehensive in its coverage, and strengthened by the comparative and international perspective. In good textbook fashion, it touches on most of the major issues on the economic side of national security, including chapters on defense spending, defense industries and procurement, the arms trade, economic relations among military allies, and the effect of a global economy on national security. Most of the discussion is well done and informative. Kapstein's emphasis on the effects of interdependence, including the internationalization of defense production, is especially interesting. But two drawbacks are evident.

First, the breadth of coverage is not matched by sufficient depth. Several of the chapters left me (imagining myself as a student) wanting more—much more in some cases. Only a few of the chapters (e.g., that on the arms trade) seemed to provide enough substance and detail. Good students, I think, will be left unsatisfied. One chapter on mobilization, war, and conversion, for example, was weighted more toward mobilization than conversion, although the latter, I think, is the more controversial and more interesting problem these days.

Second, the author's initial analytic framework and subsequent claims about frameworks are handled somewhat loosely. The introduction offers the obligatory tour of the major paradigms in International Political Economy (liberalism, realism or neomercantilism, and marxism); but there is a lack of clarity about whether students are to use them as explanations of, or prescriptions for, state behavior. Marxism is described as an explanation (it is not, of course, prescriptive). But Kapstein ends the introduction by saying the best policy prescription is a combination of liberalism and realism. From there on, the IPE trinity is not used systematically. And much of what Kapstein talks about does not necessarily fit those models anyway—such as weapons procurement and defense spending, where national politics and bureaucratic forces are important. This makes the IPE models look rather obligatory and pro forma. Kapstein's claim that he wants to provide a framework for analysis in each chapter only underscores this problem. Frameworks rarely emerge clearly (the chapter on alliances is an exception). Most of the time, one has been given a good introduction to the topic and provided with some concepts and examples.

The Ross volume seeks to transcend the traditionally segregated political and economic approaches to the study of national defense. In his introduction, Ross makes a tentative case for the *political economy of defense* as a nascent and potentially coherent subfield; but this attempt seems strained. Just as political economy is a collection of theoretical approaches and substantive concerns, so is the political economy of defense. Consequently, the merit of the volume is in the individual chapters, not in its effort to define an emerging endeavor. As the title and author note, it is an assemblage of issues and perspectives. Moreover, the kind of work in the Ross book has been going on for quite some time. Somewhat uneven in their quality, most of the chapters are interesting and informative; yet all in all, they offer no surprises or innovative approaches and insights. Most of the findings are familiar or self-consciously inconclusive.

Although the editor argues that the political economy of defense transcends a narrow focus on the economic consequences of military spending, 5 of the 10 chapters address that problem directly; and it is an indirect concern in 1 or 2 others. For example, Steve Chan provides a brief literature review on the relationship between military spending and economic performance; and Daniel Nelson examines the impact of Warsaw Pact defense spending on member nations. Much of this work is substantial and well-done research, such as Judith Reppy's comparative piece on the relationship between military research and development funding and international trade performance and David R. Davis and Steve Chan's longitudinal analysis of security and welfare in Taiwan. The most political analysis is the chapter on electoral cycles and defense spending in Israel, by Alex Mintz and Michael D. Ward. Politics is also important in Glenn Fong's very good chapter on Pentagon–industry collaboration in high technology.

While one doubts that the political economy of defense is more than a broad set of issues and perspectives, that is not an argument against its importance as

an area of research. I do have a concern about the apparent increase in economy-centered research in the area of defense and national security now that the Cold War is ending. My concern is that the strong tilt toward economic analysis and economic issues could crowd out real politics and political analysis of military policy, in much the same way that the dominance of strategic logics (e.g., deterrence theory) in international relations theory tended to preclude the insights of political analysis in our understanding of national security policy during the Cold War.

University of California, Santa Cruz DANIEL WIRLS

From Revolutionary Cadres to Party Technocrats in Socialist China. By Hong Yung Lee. Berkeley: University of California Press, 1991. 437p. $48.50.

Hong Yung Lee's new book is an extremely broad study of "the structure, personnel, and historical formation of the cadre system in China" (p. 1). It covers the entire history of the Chinese Communist party (CCP) from its founding in 1921 to the present although the bulk of the material deals with the post-Mao reform era. An immense amount of data, including 56 tables, are presented on all levels of the Party and state from the highest officials in the land to ordinary cadres and Party members. The substantive issues covered include recruitment patterns, bureaucratic methods of personnel management, the politics of cadre staffing, and speculative argument concerning the characteristics of the political system emerging during the reform period. While readers will undoubtedly find something of interest in this wide-ranging study, it has significant limitations in terms of both empirical accuracy and conceptual clarity.

Although the book's central theme that there has been an elite transformation from "revolutionary cadres" to "Party technocrats" is clearly stated in the title and is, within limits, noncontroversial, it is more difficult to determine its basic argument in terms of how elite composition relates to political outcomes. Perhaps the most representative statement of the author's position is that "the less institutionalized a political system [like China's], the more likely political elites will bring the ideology, experiences, and outlooks of the social classes from which they came into the political process" (p. 387). There are several problems with this which can be seen in Lee's treatment of the revolutionary cadres dominant in the Maoist era. First, Lee ascribes an alleged "continuing rural orientation of the revolutionary elite" after 1949—itself a questionable notion—to the initial recruitment of the ruling elite "largely from poor peasants with a low level of education" (pp. 47, 387). This is simply not so if we regard the "ruling elite" as most concretely represented by the Central Committee elected in 1956. As Donald Klein's work 30 years ago demonstrated, these leaders were far from the bottom of the traditional social scale in their origins and they were quite well educated with substantial numbers having tertiary, and even foreign, training. But perhaps *ruling elite* is meant to refer to the Party as a whole, where the characterization would be more (but hardly fully) adequate. Such an approach is suggested by various assertions that political outlooks, such as the "rural orientation," came not so much from the CCP's top leadership but from the mass membership. For example, Lee argues that "the root of the Maoist ultraleftist tendency should be traced back to the cadre corps. . . . In this sense Mao's peasant mentality represented rather than shaped those of the majority of cadres" (pp. 73–74). But given all we know about the leader-dominated Chinese system, the suggestion that fundamental regime policies largely *reflected* the social characteristics of ordinary CCP members, which were in fact quite different from those of the top leadership, is very hard to credit.

There is, however, more to credit the notion, also advanced by Lee, that much political conflict can be explained by the concept of "situational groups," that is, groups forged by the impact of particular state policies on their members. Here he usefully adopts the well-established distinction among the victims, survivors, and beneficiaries of the Cultural Revolution in explaining developments in both the last days of the Maoist period and the early post-Mao era. But apart from some errors of fact (e.g., the treatment of Chen Yun, perhaps *the* major architect of the reform of the cadre system, as essentially having little to do with it), shortcomings to the approach are reflected in the contradictions within Lee's account. Lee astutely ascribes the impetus for reform to the rehabilitated victims of the Cultural Revolution, who, having suffered Maoist excesses and had the opportunity to reflect upon the shortcomings of the system during their period in the wilderness, became "born-again reformers" once returned to power (p. 168). Yet key members of this group, including Chen Yun, Peng Zhen, and Bo Yibo are elsewhere pictured as conservative opponents of reform (see, e.g., pp. 285–86). While there undoubtedly is something in this, too, the book lacks a systematic analysis of the paradox. It offers neither a careful empirical examination of the politics of the "old revolutionaries" nor a theoretical explanation of their presumed shift of position.

Finally, the discussion of the background of the new generation of Party technocrats provides some rich detail on their well-known characteristics but leaves unsettled major questions concerning the politics of this generation. Indeed, quite sensibly Lee cautiously canvasses alternative theories concerning the tendencies of technocratic leaders and, on more than one occasion, notes that no definitive conclusion is possible. Nevertheless, he does have a preferred view that the technocratic concern with results, rather than ideology, will result in a greater tendency to bargain and compromise, the likelihood of a collective style of leadership, and fewer power struggles and purges than marked the rule of the old revolutionaries (see p. 407). Still, as Lee acknowledges, the relative lack of close personal ties among the technocrats could result in division or stalemate; and a detailed examination of elite politics during the reform era (only spasmodically undertaken in the book) might show the restraining hand of the surviving old revolutionaries on their younger colleagues, who would possibly have been much more prone to open conflict if left to their own devices. Most of all, a careful reconstruction of reform politics would arguably show that the likelihood of polarized, as opposed to consensual, politics has more to do with the problems and stresses facing the leadership than with the characteristics of particular elite groups.

University of Sydney FREDERICK C. TEIWES

Self-Interest and Public Interest in Western Politics. By Leif Lewin. New York: Oxford University Press, 1991. 146p. $48.00 cloth, $16.95 paper.

Leif Lewin sets for himself a very imposing task with the first line of this book: "Is it self-interest or public interest that predominates in Western democracies?" It is Rousseau's question, Are people guided by their individual wills (democracy being but the sum of these individuals wills) or by some general will that is distinct (or at least distinguishable) from individual will? This is a question that has concerned political scientists of all stripes and colors, Lewin argues; but because they make different assumptions and work at different levels of abstraction, they are often talking past one another, rather than to each other. Thus, the field as a whole has reached no consensus on how best to structure government to foster the common good. The aim of this book is not to add to the debate in any one of the areas of political science that have addressed the self-interest/public interest question but, rather, to encourage cross-fertilization between the different schools of thought.

To achieve this goal, Lewin begins by sketching the nature of the debate in the rational choice, political philosophy, and electoral behavior subfields. Following Bentham, Condorcet, Hobbes, Mill, and Smith, the "public choice" rationalists assume that people are guided by their self-interest, while the "social choice" rationalists ask whether what is best for society is but the sum of what is best for its individual members. Philosophers concerned with this problem (Lewin relies mostly on Sen and Harsanyi) doubt that anyone can know what is in a person's self-interest (even that person) or, consequently, debate how best to determine what is the general interest. Electoral researchers have explored whether the general public lives up to the classic democratic ideals of the rational, informed voter guided by a disinterested view of the common good, as opposed to being the irrational, unthinking, socially determined or passion-driven voter that Madison feared.

The bulk of the book, however, is devoted to a review of the evidence for the basic self-interest assumption of the public choice school. We hear from Downs (*Economic Theory of Democracy*, 1957 and *Inside Bureaucracies*, 1967), Buchanan and Tullock (*Calculus of Consent*, 1962), and Niskansen (*Bureaucracy and Representative Government*, 1971) that voters are narrowly self-interested; that parties and politicians are spineless, programless vote maximizers; and that bureaucrats care only about increasing their own budgets. These classics of the public choice literature structure the remainder of the book, as Lewin explores whether voters, politicians, and bureaucrats are as self-interested in their actions as the rational choice literature says they are.

The evidence turns out to be a devastating attack on the self-interest assumption. The public rarely vote their own pocketbooks; there is little or no evidence that politicians try to create "electoral business cycles" or that parties feel free to adopt any position they believe the majority of the public favors. Moreover (while the evidence is much less complete here), there is also little or no support for the assumption that bureaucrats are budget-maximizers.

The strengths of this fine, concise book are the diversity of areas covered and the author's awareness of the North American, European, and Japanese literatures on all of these topics. I would strongly recommend it to the participants in any of these debates. As a scientist, I appreciate Lewin's insistence on *evidence* as he attempts to adjudicate between the advocates of the self-interested and public-interested views. Paradoxically, the major limitation of this book is that same focus on evidence, or (more precisely) the limited nature of the available data. Although I cannot claim to be an unbiased observer of this debate, to my mind, the evidence against self-interest as a major motivation for political action is overwhelming.

The case for the public interest alternative is not nearly as strong, however. To begin with, we are shortchanged on any discussion of the philosophical underpinnings of the public interest viewpoint. Rousseau and Locke are not seriously mentioned until the end of the final chapter. And, as Lewin acknowledges, the evidence for some sort of public interest motivation is generally lacking—limited primarily to voting behavior research. This is not a criticism of the book so much as a criticism of those of us practitioners who develop the theories and provide the evidence to address this question. The fact still remains, however, that if we can reject the hypothesis that self-interest guides most political activity in Western democracies, we, as a field, are nowhere near being able adequately to test the alternative hypothesis that public interest is a major motivator of that same behavior.

Rutgers University RICHARD R. LAU

The Religious Challenge to the State. Edited by Matthew C. Moen and Lowell S. Gustafson. Philadelphia: Temple University Press, 1991. 320p. $44.95.

This volume is the product of a conference on religion and politics held at the University of Maine in 1989. Although the title accurately describes the general thematic focus of the collection, many approaches to various aspects of the problem are represented in the essays. The editors' introductory essay provides an abbreviated historical contextual background to the problem and thumbnail summaries of the individual essays. However, it provides neither a conceptual framework within which these essays could be integrated nor an analytical elaboration of their theoretical significance.

Although the editors state in the preface that the book is primarily intended for scholars, a number of shortcomings diminish this collection's significance as a scholarly work. Only the essay by Matthew C. Moen appears to be based on original research using primary data (in this case, interviews, documents and publications of the Christian Right in the United States). The others are either primarily or exclusively based on the analysis of secondary sources (mostly the work of other scholars). Unfortunately, few of the contributors bring new perspectives through the application of different theoretical perspectives or original analytical insights to their data. The editors state, "Because the approach of the contributors is normative and historical, the volume is accessible to a wide readership" (p. x). The descriptive nature of many of the essays diminish their scholarly value.

No contributor refers to any of the other contributions in the volume. This is particularly unfortunate because it wastes an opportunity for more comparative analyses

and the potential to refine conceptual frameworks. For example, the suggestive insights of Damian Fernandez regarding the role of popular and political religion in the Cuban Revolution could have profitably referred to Allan Metz's discussion of civil religion in Israel. Fernandez asserts that political religion provided a source of legitimacy for the Cuban Revolution but fails adequately to demonstrate how this was done. To paraphrase Clifford Geertz (quoted by Fernandez), he fails to show how the symbols symbolized. The use, in reference to religion, of terms like *popular, political,* and *civil* without conceptually defining and refining them is a missed opportunity of the whole volume.

Metz (a specialist on Latin America) attempts to cover too much ground in his survey of the highly complex nature of religion and politics in Israel. Borrowing heavily on the pioneering work of Charles Liebman and Eliezer Don-Yehiya, he patches together what he terms a "theoretical framework." Unfortunately, this framework is no more than a few generalizations about the nature of the relationship between religion and the state. The work of Liebman and Don-Yehiya is important for the originality of their primary research but not for its theoretical sophistication. They use Robert Bellah's notion of civil religion as a focus for their most important book, a concept Metz does not even utilize in his analysis. His two case studies add no new insights, either conceptual or substantive, to this rather unfocused and unoriginal essay.

Another even more glaring example of the lack of communication between the contributors is the fact that neither Ann Lesch, who writes knowledgeably about the Muslim Brotherhood in Egypt, nor John Voll, who examines the same subject as one of the four cases of Islamic fundamentalism in Africa, refer to each other's analysis. Voll's case analysis is much more successful than Metz's.

Metz introduces the notion of corporatism in his essay on church–state relations in Mexico but fails to utilize it as a conceptual framework for his analysis and does not even mention the term in his conclusions. Nor does he refer to Gustafson's use of corporatism in his analysis of church and state in Argentina, a compliment Gustafson returns by ignoring Metz's essay. Since Gustafson is one of the editors, his omission is even more glaring. Aside from the introductory essay by the editors, only James Warhola, in his lucid and interesting discussion of religion and modernization in Gorbachev's Soviet Union, makes a single, general reference to the volume as a whole.

A very brief essay by Donald Bain, an expert on strategic studies, on the papal challenge to Soviet regimes in Eastern Europe is most noteworthy for the memorable line, "When the pope speaks, people listen" (p. 138). Mark Bartholomew's short analysis of the relationships between the church, the peace movement, and the Social Democratic party of Germany is both knowledgeable and informative.

The volume lacks both an index and a general reference list. Both omissions undermine the utility of the volume for scholars. It appears as if this volume was rushed to press before rapidly changing events could make the book appear outdated. The editors would have done better to have encouraged the contributors to refine their essays and to refer to one another's contributions and used that extra time to write a concluding essay that would bring out the general implications of the findings of the individual authors. Unfortunately, in its present form, this volume makes a very modest contribution to the scholarly literature.

Rutgers University MYRON J. ARONOFF

Political Participation and Democracy in Britain. By Geraint Parry, George Moyser, and Neil Day. New York: Cambridge University Press, 1992. 509p. $84.95 cloth, $34.95 paper.

British specialists are not the only likely readers of this impressive research work. It is a major contribution to our discipline's understanding of political participation in democratic societies generally. The large array of data is British, but the authors present and analyze their findings in an intellectual context that is hardly parochial and often explicitly comparative. Along with the exploration of hypotheses of their own, they test many findings from earlier scholarly studies of political participation in several nations, referring most frequently to *Participation in America* by Verba and Nie (1972).

The book's data are from a substantial project that Parry and Moyser directed, as principal investigators, with funding from Britain's Economic and Social Research Council. Research, in 1984 and 1985, involved not only a national sample survey of nearly sixteen hundred people but also a survey of about the same number in six diverse localities, plus interviews with three hundred selected leaders in those same localities. Questions were designed to learn the extent and nature of political participation, usefully categorized as voting; party campaigning; collective action (through meetings, petitions, and other group pressures); contacting public officials; and direct action (marches, blocking traffic, political strikes, and boycotts). Attention concentrates on the minority of citizens whose participation goes beyond voting. Rightly, as the authors say, that kind of British participation has been much less fully studied than British voting behavior. They fill a large portion of the gap in our scholarly knowledge by telling us about the numbers active in each category, the social status of these participants, the resources or other factors that help account for participation, the impact of participation on policy outcomes, and the satisfaction that participants derive from their activities.

So general a description does not do justice to the wealth of information and analysis that the authors provide. An idea of the book's richness, as well as of its quantitative character, may be inferred from the presence of 49 figures, 7 charts and maps, and 111 tables. The expository prose is ample and clear. Moreover, the authors put their study in a coherent theoretical, even philosophical, framework, reflecting a concern with the significance of participation in a democratic society. Especially enlightening is the discussion of the strongly local emphasis of most participant activities. It was first discovered in the national survey and then confirmed and illuminated in the several local studies that activists are more often motivated to influence policies close to home than they are to take up the broad issues that command the attention of the national news media. Local, not central, government is the usual target. Issues beyond the everyday concerns of citizens are to a larger extent left to officeholding representatives between elections. The authors evaluate the consequences for stable

and effective democratic government with great sophistication, taking into account the contrasting traditions, on the one side linked to Rousseau and on the other, to Schumpeter.

Citing a few of the book's many other findings will illustrate its significant lines of inquiry. Americans, accustomed to unfavorable comparisons of voting turnouts in the United States with those in Britain and other nations, will be interested in data showing British participation in certain political activities to be at a somewhat lower level than American participation in similar categories. A more generally intriguing matter is the special place that party campaigning occupies in the responses to questions about whether the various participatory activities led to increased knowledge. Whereas most activities were not seen by participant–respondents to have added to their knowledge, the major exceptions were two forms of party campaigning. Canvassing for votes and behind-the-scenes party clerical work were reported as educative in their effect. But the absence of such learning in nonparty political activities did not mean that the relevant participants failed to find satisfaction in their experience. A high degree of satisfaction characterized most participants. Again, the local surveys fully confirmed the national survey. "Participants," the authors conclude, "displayed remarkable degrees of contentment with the outcomes of their dealings with local authorities" (p. 403). Their sense of individual efficacy was at a respectable level, even for some who expressed considerable political cynicism. Here, as elsewhere, the relationship is complex. Indeed, the complexity, along with multidimensionality, is a principal theme of the authors' careful analytical study.

University of Wisconsin, Madison LEON D. EPSTEIN

The Apartheid State in Crisis: Political Transformation in South Africa, 1975–1990. By Robert M. Price. New York: Oxford University Press, 1991. 309p. $55.00 cloth, $16.95 paper.

In this study of recent South African politics, Robert Price distinguishes processes of transition (changes in a society's formal political order, consisting of rules and institutions) from processes of transformation (changes in a society's substructure of domination, consisting of social interactions, cultural norms, economic activities, and informal power relationships). Price argues that changes in the latter make possible change in the former. In the case of South Africa, a two-decades-long process of economic and social transformation have led to the current talks on changing the formal rules that govern South African politics.

Price observes that apartheid was established in the aftermath of World War II in order to bring Afrikaners up to socioeconomic parity with South Africans of British descent, to reverse gains made by blacks in South Africa during the war years, and to respond to an international system slowly developing norms of decolonization. While never accomplishing this last goal, by the early 1970s, apartheid had transformed the interests and demographics of Afrikaners to the point where their elites saw apartheid as a drag on the economic growth needed to preserve white domination.

The imperative of reforming apartheid emerged during a period of regional confrontation. The collapse of Portuguese colonialism in Southern Africa created the threat of a region unified against white domination in South Africa. The success of liberation movements in Angola and Mozambique spurred on such movements in Zimbabwe, which in turn emboldened black protest in South Africa itself. A newly found confidence among South African blacks, combined with the economic deprivations of apartheid, created the environment in which the Soweto uprising of 1976 took place. The regime responded to the Soweto riots with brute force and repression, which prompted the international community to increase pressure on South Africa to change. The pressure—economic and cultural isolation—exacerbated the downward economic spin of South Africa and pushed the regime to undertake reform. Thus began a slow, roundabout effort to eliminate apartheid in order to maintain white power and privilege.

Price, drawing from the insights of Tocqueville, describes how the reforms had unanticipated consequences that strengthened internal opposition to white supremacy. Renewed opposition was met with renewed repression; renewed repression was met with new international condemnation and stiffer sanctions; renewed international pressure increased the need for reform and strengthened domestic opposition, which led to more repression. By the mid-1980s a situation that Trotsky had described as "dual power" existed in South Africa. What Price refers to as the "security contradiction" of the white regime could only be broken by the willingness of the regime to negotiate a transition in its formal political rules and constitution.

Price knows South African politics, economics, and society and writes well. He recognizes that developments in South Africa are a product of three levels of analysis—international, regional, and national—which he vividly and impressively weaves together in order to explain how change has come to South Africa.

Weaknesses in Price's analysis come from two sources. First, he tends to see various changes in South Africa as linear and therefore unsusceptible to reversal. For instance, early on, Price states that changes in the substructure of society will necessarily lead to transition, a claim that he subsequently backs away from (pp. 4, 285). Elsewhere in the book, he claims that economic processes irrevocably shattered Afrikaner identity and values (pp. 83–85). Yet if we have learned anything about political identities of cultural groups, it is that they wax and wane and that sentiments of group identity can disappear only to reemerge at a later date.

Second, Price's formulation of the relationship between substructure of domination and structure of formal order suffers from imprecision. Consider, for example, the grab bag that makes up the substructure of domination: social interactions, informal norms, economic activities, and informal power relationships. Should we expect changes in these categories to be synchronous? Do any of the categories take precedence over the others? Here, the two authors that inform Price's conception of society differ. Tocqueville saw revolution as inevitable in France because of the disjuncture between the formal rules of the Old Regime and the informal constitution of French life. In his history of the Russian Revolution, Trotsky saw the key disjuncture as one between the formal political rules that benefitted a particular class and economic changes that had rendered that class obsolete.

In the South African case, there have been important

changes in some of the categories of the substructure. But the transformation that Price describes is an incomplete one; changes coexist with important continuities. Two examples come to mind: economic activities that continue to benefit the white population of South Africa disproportionately and mental legacies of racism. If Price's overall thesis, namely, that formal political rules must rest on the informal substructures of domination, is correct, then any transition to majority rule in South Africa will stand on shaky foundations; the formal constitution will proclaim equality, while the informal one will mock such claims.

Even with its faults, Price's book is a stunning accomplishment. It towers over recent analyses of the South African situation and puts to shame recent historical sociological treatments of revolution that amount to histories written to illustrate a theoretical assertion like the autonomy of the state or the nature of class relations between lord and peasant. Price, like Trotsky and Tocqueville, constantly ranges back and forth between historical facts, patterns, and relationships on the one hand and broader theoretical concerns on the other. And like Trotsky and Tocqueville, Price's keen insights come not from some overarching theoretical perspective that by insisting that it explains everything, explains nothing, but from familiarity with historical detail and sociological method.

Johns Hopkins University STEPHEN J. STEDMAN

Democracy and the Market: Political and Economic Reforms in Eastern Europe and Latin America. By Adam Przeworski. New York: Cambridge University Press, 1991. 210p. $39.50 cloth, $12.95 paper.

This book deals with two of the most important and least understood phenomena occurring in the world today: democratization and the transition to more market-oriented economic systems. Przeworski is a wise and insightful analyst unusually well placed to observe the transitions in both Eastern Europe and Latin America closely and thus, perhaps, uniquely qualified to theorize about them. The strength of the book lies in the effort to create general models of processes that are usually treated descriptively.

The stylized political world that underlies these essays is populated "not by freely acting individuals but by collective organizations that are capable of coercing those whose interests they represent," latent groups that might become organized, and individual voters who can periodically reward or punish political leaders but seem not to affect the strategies of the groups whose interactions with each other determine political outcomes (see pp. 12, 39). This is not the only possible stylization of a democratic political system, and it occasionally runs into inconvenient events that require some interpretative surgery to make them fit. But an imperfect model (and all are imperfect) is always better than no model; and this model gives Przeworski the leverage he needs to theorize in areas that have been notorious as analytic swamps.

I shall concentrate on three of the numerous ideas in this wide-ranging book that seem to me to form the logical core: the requirements for stable democracy, the choice of new democratic institutions during transitions, and how the transition to market-based economies affects the probability that stable democracies will arise from the breakdown of authoritarianism.

Przeworski's treatment of stability begins with the observation that democracy survives when groups that lose out in the policy process continue to comply with the democratic rules of the game. He therefore focuses on losers, developing an abstract model of their preferences as a way of exploring the conditions that would affect their willingness to remain in the democratic game. In the model, losers' compliance depends on the stakes of the game, the probability of winning in the future, and their discount rate (all affected by, among other things, the institutional structure of a particular democracy), as well as the probability of successful subversion.

Once it has been established that institutions affect the probability that democracies will consolidate themselves, it becomes important to understand how different concrete institutions affect both policy losers' calculations and how political actors choose new institutions during transitions to democracy. Przeworski says little about the former but devotes considerable attention to the latter. In the Przeworski world, institutions emerge as the result of bargaining among political "forces." Where the "relation of forces" is known and uneven, institutions will reflect the interests of "a particular person, party, or alliance" (pp. 81–82). Where the relation of forces is known and balanced, the outcome is indeterminate. Where the relation of forces is unknown, institutions can be expected to emerge that contain checks and balances, reduce the stakes of competition, and provide guarantees to potential minorities. Anarchic and uncertain transitional situations may thus, counterintuitively, be most conducive to the emergence of stable democracy in the long run.

In the final essay of the book, Przeworski moves from the general issue of democratic consolidation to the particular threat to consolidation posed by concurrent transitions to market-based economics. In this essay, he has many sensible and insightful things to say about the uncertainties and short-term costs of economic transition; and he develops a series of perceptive arguments to explain the choice of radical, over gradualist, adjustment strategies.

A risk-averse person would not have ventured into this subject so early on in the process, when outcomes were still so uncertain. But strong theory gives scholars the courage to get up and do what needs to be done. With the advantage of more experience of reform than was available to Przeworski, however, it now seems that his conclusions about the political consequences of economic reform are too pessimistic.

As Przeworski notes, the costs of economic transition for organized labor are likely to be high. Because he sees the political process as dominated by the struggle for advantage among organized groups, Przeworski concludes that governments will only be able to pursue adjustment through one of two strategies: consultation and negotiation with groups, especially unions, that will be hurt by reforms (concertation), which he sees as unlikely in developing countries for several sound reasons; or else, insulation of policymakers from affected groups, which he sees as undermining democracy. In fact, however, several governments (most notably, Argentina, Turkey, and Thailand) have carried out substantial reforms (without concertation) while maintaining political support through at least one election.

By the clever sequencing of reforms or dramatic reductions in inflation, they have made reforms palatable to the average voter while imposing heavy costs on much of the unionized working class. They have, in other words, found support for themselves and their policies among the relatively unorganized masses of voters while politically marginalizing better-organized groups. Although relatively unimportant in the book as a whole, this seems to me a good example of the limitations of the Przeworski stylization. In democracies, outcomes sometimes depend on interactions between political leaders pursuing their own interests and freely acting individual voters, not organizations "capable of coercing those whose interests they represent."

To return to the whole, this is an important book on important topics. Its vivid imagery, lively language, and unexpected but apt metaphors make it highly enjoyable to read. Like other work by Przeworski, it will help structure the thinking of other scholars. It will also frustrate anyone who attempts to use the arguments in it as the basis for building a sturdier accumulation of knowledge. The empirical referents of concepts central to the models are so underspecified that it is hard to imagine how one would go about testing them. How, for example, could you tell whether the "relation of forces" was balanced or not? How could you tell whether an unknown balance of forces resulted in institutions that limit the stakes of the political game without any information about what concrete political institutions have that effect? Nevertheless, the book is thoughtful, imaginative, and provocative. It will cause its readers—scholars, political activists, graduate students, and even some undergraduates—to think interesting thoughts.

University of California, Los Angeles BARBARA GEDDES

Politics within the State: Elite Bureaucrats and Industrial Policy in Authoritarian Brazil. By Ben Ross Schneider. Pittsburgh: University of Pittsburgh Press, 1992. 337p. $49.50.

This lively and important book is an account of the triumphs and failures at the vanguard of Brazilian policy-making represented by the steel and mining sectors. The coverage is broader than the subtitle indicates; for the analysis ranges over the decades of developmentalism, democratic as well as authoritarian, from the postwar years to the present. One of the implications of Schneider's research is that changes in political regimes do not matter much for the way the Brazilian *sistema*—the deep structure of power—operates.

The paradoxical idea behind the analysis is that the distinctive trajectory of bureaucratic careers in Brazil, dominated by agency-hopping and political, rather than civil service–style, appointment, undercuts institutional predictability at the same time that it permits and even facilitates a creative mix of interorganizational competition and coalition building. Although he does not resort to the jargon, the evidence assembled by Schneider can be understood as documenting the vagaries of the labor market internal to the Brazilian public sector and its consequences for getting the job done.

Politics within the State is worth reading and rereading, first, simply as an insightful phenomenology of the sprawl and folkways of the Brazilian state. The organizational guerrillas who are the subjects of story will have no trouble recognizing themselves and the arcane recesses of their environment. In going through its pages I was reminded of the vulnerable artfulness of Brazilian soccer—almost all offense, given to *o lance genial* (the stroke of genius), only to collapse the next moment—for all one knows, out of boredom with the game. The book is fun to read. Schneider writes suspiciously well.

Schneider also raises important questions about the links between reform-mongering, organizational design, and economic strategy. The challenge involved in dealing intelligibly with bureaucratic politics—and with the Brazilian public sector, in particular—comes not only from the size of the thing itself but also from the simultaneous vastness and inadequacy of the literature on the subject. Like many others before him, Schneider attends to the hard-chargers, the managerial zealots bent on compressing "fifty years of development in five," as a Brazilian president once declared. As Delfim Netto, the *primus inter pares* of the barons of grandeur, put it with still greater hyperbole, "Development is the moral equivalent of war." Schneider himself suggests that this focus ignores the agencies charged with services, the cultivation of human capital, and, more seriously, the larger bureaucratic context (less flashy, more inertial, closer to the backwaters of Brazilian politics) above which the superstars of capital accumulation and futuristic technology move and shake. While attention is given to the coziness between ministerial officials and their counterparts in private industry and banking, the lack of contact between development hotshots and those engaged in mop-up and peripheral chores like health and education—the clubbiness of winners and their Darwinian exclusion of the timid, the luckless, and the inept—seems crucial to understanding the cumulative inequities that defy democratization. In this light, the pathologies of the American entertainment industry would be a better metaphor for Brazilian development than a balletic soccer match. The analogy breaks down not in the magnitude of the profits or the style of the subculture but in the costs to losers.

Schneider's criticism of the assumption that bureaucratic fragmentation regularly leads to inefficiencies is reasonable. Yet his contention that development projects in Brazil succeed more frequently than expected is problematic. If the baseline for this claim is that fragmentation inevitably gets in the way of intended results, then the hypothesis is bound to be correct. However, a more sensible standard for evaluating implementation might start with a statistically normal distribution of outcomes. The idea would be to estimate what organizational formats produce better (or worse) results than they might do randomly. The logistics of testing this null hypothesis (hardly a model but better than no criterion at all) would involve a larger number of cases than the four examined by Schneider, who is careful to list a number of conditions and factors that contribute to success and failure in variably coordinated and chaotic settings. Nevertheless, baldly stating criteria for success and failure or else giving reasons for not doing so would clarify the particular outcomes.

But a significant implication of *Politics within the State* is that using a random walk as a point of departure for understanding policy implementation in Brazil—while more realistic than condemning the process to failure by fragmentation—is historically inaccurate. Over time, and at an accelerating rate over the past decade, the

chances of success seem to have deteriorated. This may be the nightmare effect of the administrative turnover that boosted innovation earlier in the public sector. The circulation of officials almost certainly created incentives for each of them to embellish his curriculum vitae by launching new ventures without incentives to complete the projects during their brief tenures. Such a proliferation of commitments helps explain how aggregate bottlenecks and irreversible paths build up from tactical improvization. It also lends a certain pathos to the cycle of enthusiasm and exhaustion—the manic-depressive syndrome—common among the talented individuals responsible for the fortunes of the Brazilian state.

Adept at cutting deals and finding shortcuts, energetic in resolving problems on the spot, Brazilian bureaucrats and politicians have seen their institutions veer out of control and their developmental visions thwarted. Next to the distinctive combination of patrimonialism and piecemeal empiricism, the anomaly of vaunting ambition and shortsightedness may be as close to a fixture of Brazilian political culture as one is likely to encounter in so fluid a spectacle. Expedience as an ideology is not pragmatism. This ethos emerged in the ramshackle constitution of the New Republic; and it produces the sort of scenario that periodically sets the military tapping its fingers at yet another demonstration that democratic politics may be, if not quite catastrophic, less than the sum of its parts.

If development in Brazil has become a race against worsening odds, for reasons having to do both with institutional perversities and structural distortions embodied in the concentration of wealth, it may be instructive to set the Brazilian impasse against the less-discouraging experience of countries like Spain and South Korea, with comparable histories of authoritarian and democratic episodes—not to mention cases closer to home, such as Mexico and Argentina. In conveying the drama and the stakes of Brazilian politics and in offering a provisional interpretation, *Politics within the State* brings us closer to placing the terrible enigma of Brazil in comparative perspective.

Arizona State University PETER MCDONOUGH

Disorder under Heaven: Collective Violence in the Ming Dynasty. By James W. Tong. Stanford: Stanford University Press, 1991. 325p. $39.50.

In this book James Tong offers a quantitative, diachronic, rational choice, state-centric study of collective violence during the Ming Dynasty. Quantitatively, he marshals data on 630 violent events and a variety of data on the economic and political context surrounding them. Temporally, his study spans the 277 years of the Ming era. His approach is rational choice in that he hypothesizes that the decision to join in collective violence was the product of individual calculations of the odds of survival in time of dearth and hardship and the odds of survival as an outlaw in the face of government repression. His approach is statist in that most of his measures of the odds of surviving either hardship (e.g., provision of government relief) or as an outlaw (e.g., proximity of government forces) are measures of state performance and capability.

Tong's study is exemplarily straightforward and parsimonious. In an introductory chapter he poses his research tasks, namely, to describe and explain the major forms of collective violence during the era; sketches out his model; and describes the major patterns visible in his data, namely, a preponderance of collective violence in the South, in the latter half of the dynasty, in rugged and remote regions, and in the form of banditry. Chapter 2 elaborates the model—a choice model based on the interplay of individual motives (primarily survival), the resources available to the people, and the opportunities for contention open to them under the political status quo—and the data, with terms defined and operationalized using the county–year (of which there are 303,869, given 277 years and 1,097 counties) as the unit of analysis.

Chapters 3 and 4 are the centerpiece of the book. Chapter 3 presents in descriptive detail the different types of collective violence: and in chapter 4 two scales (composites of four measures of survivability and six measures of survivability-as-an-outlaw) are constructed and posed against the collective violence data for each unit of analysis. Contention across county–years varied directly with survivability-as-an-outlaw and inversely with the survivability of hardship; together, they powerfully indicate that (as hypothesized) banditry as a response to hardship was by far most likely in regions and at times in which the survivability of hardship was lowest *and* survivability-as-an-outlaw was highest.

Chapter 5 goes into detail as to why the observed relationship should hold, focusing primarily on regime capabilities and how they shaped the opportunity structure of collective violence both in toto and as they varied across time and space. Chapters 6 and 7 consider two alternative explanations of collective violence: that it results from rapid social and/or economic change and that class conflict is the force behind it. The data from Ming China provide little support for either. Motives, resources, and opportunities appear to be unquestionably a superior explanatory constellation.

The special strengths of this book are its parsimony, persuasiveness, and comprehensiveness. Without falling into simplistic number crunching, Tong presents his model and his data thoughtfully and with appropriate caveats, uses data both illustratively and analytically, and winds up his analysis (including relevant historical background and consideration of the two alternative models) in only 214 pages of text. Moreover (perhaps because it is so simple and straightforward), his argument is persuasive, at least to me. The components of Tong's scales seem to be valid indicators of the concepts in question, and his operational measures are valid measures of the component variables. Finally, Tong's study is comprehensive. The genre of historical political sociology includes few studies of collective violence covering any society so intensively over such a long period. Although equivalent, the quantitative analysis in Charles Tilly's *Contentious French* is neither so systematic nor so permeating. In Tong's words, "For no other premodern system are annual, local-level data on collective violence as available as they are for the Ming" (p. 9); and, as he points out in his conclusion, data collection efforts presently underway in China hold out the promise of extending this study by several centuries in both directions from the Ming.

This said, I still have three reservations regarding this study: a worry, a caveat, and a disagreement. The worry is that there may be more data-underreporting than Tong argues. A similar data set for a similar period

(1590–1877) for another preindustrial society (Tokugawa Japan, with one-fifth the population of Ming China) includes 7,664 incidents of collective contention, of which 1,258 involved aggressive or violent action and 811, outright violence. By any criterion of comparison, China seems extraordinarily quiescent.

The caveat is that this study is not quite comparable to many of those (by, e.g., Tilly, Gurr, and Hibbs) that come quickly to mind, because the collective violence it focuses on is overwhelmingly *collective, armed crime*, not just political conflict or protest or violence, or social contention more broadly construed. Thus, the model is more likely to fit Jesse James than Emiliano Zapata. Tong gives good reasons for this focus, but it may still restrict the comparability of the study.

Finally, I disagree with some of the assertions explicated in Tong's model. In the first place, I do not agree that "in premodern China, as in other agricultural societies, subsistence was the central objective of the peasantry." (p. 78), unless *subsistence* is defined to include *status* subsistence, that is, keeping one's family and community economically, politically, and socially viable. I think that the sorts of economistic calculating studied in Samuel Popkin's *Rational Peasant* were always at work and could lead to collective violence of various types. Nor do I agree with the corollary that free-riding was not a problem in collective violence because participants sought only private goods and that "the desire to seek public goods" is not a "plausible explanation of motivation for premodern collective violence" (p. 199). It *is* true for *this type* of collective violence—banditry—but not for the sorts of political and social violence studied by others, in which public goods could play a powerful role, as the example of Zapata suggests.

In other words, this is not a disagreement with what Tong has found in his data but a disagreement with the generality of his findings. Certainly, on its own terms, Tong's study is solid, persuasive, and informative; and it deserves, and rewards, reading by anyone in the field of contentious collective behavior.

University of North Carolina JAMES W. WHITE

Race to the Swift: State and Finance in Korean Industrialization. Jung-en Woo. New York: Columbia University Press, 1991. 280p. $35.00.

It is increasingly clear that the role of finance in the extraordinary growth and structural transformation of the East Asian economies is a crucial topic. Nowhere was financial policy used more aggressively than in Korea. It is also clear that the preferential allocation of capital played the crucial role in the emergence of Korea's giant business groups, or *chaebol*.

Jung-en Woo construes finance broadly and approaches it historically. She locates the origins of Korea's strong state primarily in foreign influences, particularly the security concerns of the United States, and the regional economic concerns of Japan as mediated by the United States. Thus, her approach builds on dependency theory but is more determined by global security politics than the logic of international capitalism and allows for considerable room for maneuvering on the part of the peripheral state. A second major theme is the creation of a capitalist class and of "Korea, Inc" through a credit-based financial system. Subsidiary arguments include the repeated subversion of orthodox financial and trade reforms urged on Korea by the United States, the International Monetary Fund, and the World Bank and, in contrast, successful economic intervention by the Korean government.

An initial theoretical chapter is followed by a discussion of colonial Korea. The experience of Japanese colonialism, including precocious development of an activist financial system led first by the colonial state and then by the Japanese *zaibatsu* (but never by local entrepreneurs), became a template for postwar development, she argues. Import substitution and aid grubbing under Syngman Rhee in the 1950s were not simply corrupt and ineffective but laid an economic base for the 1960s and delayed domination by Japan. Takeoff in the 1960s was made possible in good part by Japanese investment and support for the ruling party. High-interest-rate policies adopted under American pressure were incomplete, and led to a huge foreign debt, since interest rates abroad were lower; debt, in turn, led to instability and authoritarianism and, finally, a return to low real interest rates and bureaucratic allocation of capital.

Chapter 5 covers the "Big Push" toward industrial deepening of the 1970s (which the author sees as motivated by the Nixon doctrine and global economic shocks) and provides thumbnail sketches of development in five key industries. Chapter 6 is the heart of the book: "A credit-based financial system, mediated by an interventionist authoritarian state, became the basis of Korea, Inc." (p. 149). The state shoveled money to exporters and heavy industry despite their lower profitability and used control over access to capital to ensure economic performance. The state socialized the risk of the *chaebol*, while small firms and households were forced to rely on the unregulated curb market. By the 1980s, she argues, changes in the security environment and global economy and the skyrocketing of nonperforming loans convinced Korean technocrats, now overwhelmingly "transnational elites" trained in the United States (p. 190), to liberalize the financial system; while by no means complete, this liberalization and the growth of the stock market and of nonbank financial intermediaries has marked a decisive change in Korea, Inc.

The strength of the book is clearly in history, particularly on the influence of the Japanese period (a controversial issue) as a model. The in-depth treatment of American and Japanese policies toward Korea is unusually well weighted. The discussion of the implications of regional economic integration for Korea is interesting and ironic, in light of recent developments: dual hegemony in Asia with a predominant economic role for Japan is not new and was, in fact, designed by the United States (pp. 88–92, 184–87).

The book does suffer from some important omissions. It provides no sustained account of politics, particularly for the period since the fall of Rhee. There is surprisingly little analysis of the actual details of financial policy and virtually none on financial and other bureaucratic institutions (see pp. 84, 103, 159). The Ministry of Finance manages only 3 citations in the index, while Nixon receives 15. Similarly, little attention is paid to the actual organization of the *chaebol*; and the unusual definition conflates group structure and market power (p. 149). Moreover, while Woo concludes that the flip side of the development state was a "racketeering state," questions about rent seeking are dismissed in two sentences (pp. 69, 200).

Looking comparatively, it is impossible to explain from Woo's externally dominated account why Taiwan's financial system was rigidly constrained, rather than mirroring Korea's, and why high real interest rates there led to increased domestic savings and very little foreign debt. Similarly, despite her characterization of Korea as a "clone" of Japan (p. 176), the *chaebol* operate very differently from Japan's *keiretsu*. The Korean government dominated businesses and, on occasion, compelled them to merge in ways that have no precedent in Japan. The lack of a concluding chapter also weakens the analytic and comparative aspects of the book.

Finally, while the bibliography is extensive, the author implies that no one has ever recognized the importance of finance in Korea before; and that is simply not true. Important recent works by Alice Amsden, Tun-jen Cheng, and others receive at most glancing recognition. Had the publication of *Race to the Swift* not been delayed, its arguments would have been more novel, and its emphasis on dependence, less dated. It is best seen as a broad introduction to the history and international context of Korean industrialization, rather than as a cutting-edge analysis of Korean finance.

University of California, Berkeley GREGORY W. NOBLE

INTERNATIONAL RELATIONS

The World Trading System at Risk. By Jagdish Bhagwati. Princeton: Princeton University Press, 1991. 156p. $16.95.

Losing Time: The Industrial Policy Debate. By Otis L. Graham, Jr. Cambridge: Harvard University Press, 1992. 370p. $29.95.

Parallel Politics: Economic Policymaking in Japan and the United States. Edited by Samuel Kernell. Washington: Brookings Institution, 1991. 390p. $36.95 cloth, $15.95 paper.

The grouping of these three volumes reflects the interconnections between the politics of the domestic economy and the international politics of trade and security. *Parallel Politics* is a valuable collection of Japanese and American essays comparing the two nations' political management of the domestic economy. Because the essays make good reading assignments that present a clear position and sketch the historical and institutional texture of the two cases, the book will be useful in graduate and undergraduate classes.

The American essays tells a consistent and convincing story. "Political Institutions and the American Economy" by John E. Chubb and Paul E. Peterson is an excellent overview that traces the historical development of the American system of political economy. It examines how the collision of new issues and established institutions has stymied the management of the U.S. economy. Historically, the federal government found mechanisms to contain their rivalries while still providing coherent leadership; but those mechanisms have now failed. The presidential/congressional struggle now overlaps the party struggles and blocks effective policy. Matthew McCubbins reinforces this message, arguing effectively in "Party Politics, Divided Government, and Budget Deficits" that the enormous American national budget deficits are the product neither of the Reagan policies nor of congressional abdications. Rather, they are consequences of a divided government that has stalemated. Allen Schick argues in "The Surprising Enactment of Tax Reform in the United States" that astute political management permitted sharp changes in the tax structure, which suggests that sharp changes in policy are possible. If so, then a crucial question falls in between chapters: Why could we find new directions in tax policy but not effectively address the budget problem of a gap between spending and revenue? It would have been helpful if one of the authors had addressed what was different about these two problems.

It is in the Japanese case that we find controversy. Both popular discussion and much academic literature emphasize the capacities of a directive Japanese developmental state to provide industrial leadership. Consequently, a balancing emphasis on local politics is very valuable. The controversial assertion woven into Samuel Kernell's overview essay is that in Japan, appearances and earlier literature to the contrary, politicians, not bureaucrats, run the country. Do we require a revision or a marginal corrective in our understanding of Japan?

This volume provides several competing answers to the question of politicians, party, and bureaucrats in Japan. The conventional view is that there is a substantial difference between the U.S. and Japan in the policy priorities and capacities for direction and policy implementation. Haruo Shimada's essay, "Structural Policies in Japan" supports this traditional view of bureaucratic authority and directed strategies. Michio Muramatsu and Masaru Mabuchi present a story, in "Introducing a New Tax in Japan," in which the single ruling party provides—or comes over time to provide—the capacity to shape policy directions. It is very helpful to our understanding of how the Liberal Democratic party (LDP) influences policy and the capacity for governance. Yukio Nguchi's discussion of budget making is certainly consistent with the increased power of the LDP in conflicts within ministries. But critically, the Ministry of Finance remains the principal player in settling conflicts between ministries. Thus, in this essay, the power is balanced between bureaucracy and party, each having a separate domain. However, Eisuke Sakakiabara's overview of the Japanese political economy, "The Japanese Politico–Economic System and the Public Sector," presents a Japan in which the politicians whose centrally local concerns are grouped in policy cliques and political factions dominate. Neither the Ministry of Finance nor the dominant party, the LDP, play much of a role. Indeed, in his vision, the government stands aside from the market and often competes with it. The worlds of the development state (Chalmers Johnson), policy compacts (Richard Samuels), and the semicorporatist doorkeeper (T. J. Pempel) are no where to be found. This debate is neither resolved nor, really, confronted in the book; and the notion that developmental policy is simply the other face of the "politics of particularism" simply sidesteps the problem and buries the most interesting questions. For example, "collusive solutions to market dilemmas" in most countries simply retard growth. In Japan such arrangements are often elements of "controlled competition" (Murakami and Yammamura, Tyson and Zysman). True, as Sakakiabara argues, the (public) postal savings banks and the private banks compete; but the overall pattern of financial flows is one that has both sustained industrial development and created a particular pattern of competition. Are these financial flows and patterns accidental or created?

The most effective section of the volume is the comparison of structural policy in the United States and Japan. Roger Noll and Haruo Shimada present a common essay that develops a set of premises and hypotheses that guide their individual analyses. They argue together that structural policy is easier to formulate in Japan and more likely to be long-term, even though both countries will have difficulty scaling down inefficient sectors. Noll's lucid essay argues that the United States does not have a coherent and directed policy; nonetheless, he manages to outline the pattern of policy that does influence the industrial structure. Noll's overall argument is not new, but he presents it exceptionally well. The specific discussions of individual policy domains are quite rich and very interesting. I regret, for example, that the section on deregulation could not have been a chapter in itself, so that the supposed success in transportation, energy, and telecommunications in the

1980s could be set more fully against the overt failures in finance and the new problems in telecommunications and transportation. Shimada's essay usefully focuses on the Japanese industrial sector policies that have attracted American attention because of their international dimensions. Importantly, he also considers the Japanese effort to set right distortions in the economy, with primarily domestic consequences—a list that includes the gap between the high nominal income and low standards of living, increased wealth differentials in terms of real estate and financial assets, and differential allocation of economic resources between Tokyo and other regions (p. 285). In sharp contrast to Sakakiabara's presentation, Shimada asserts the critical and coherent role of the state in industrial development. Shimada is not set the task of explaining how a system made up of particularistic politicians can produce a coherent outcome. But the volume, already very good, would have been even better had this question and the difference in views between Shimada and Sakakiabara been exploited.

The editor, Samuel Kernell, proposes as his integrating theme that politics in both countries is centrally about local politicians satisfying the demands of constituencies, particularly those who have suffered in the marketplace. Local politics is certainly aggregated differently in each country because of differences in bureaucracy and party. Nonetheless, the common feature, claims Kernell, is that "rather than implementing some comprehensive strategy of growth, [policy in each country] offers protective cover to ailing and favored sectors of the economy through trade protection, sympathetic commercial regulation, and direct subsidy" (p. 328). Kernell provides an interesting and helpful analytic framework for examining the place of local politics and the mechanisms of political aggregation in the two countries. Kernell's position is not "tested" by the essays but, rather, is an interpretation of, and approach to, the material discussed. One problem with Kernell's approach is that the two particularisms, Japanese and American, produce quite different economic results. A complementary theme, not an alternate one, would be to identify the distinct policy results and ask why the common particularisms of local politics produce such distinct outcomes.

Losing Time, by Otis Graham, is a contemporary history of the industrial policy debate. In a sense, it depicts the story of the economic option that was not adopted in the 1980s, an alternate path to that chosen by the successive Republican administrations. It traces the intellectual and political debate. The detail will be a useful resource for those constructing the story of the politics of American economic decline. Although a carefully constructed history, the book would have benefited from an analytic framework that set the contemporary story in a historical context and provided an interpretation of the domestic politics of the period and an independent interpretation of the competitive and trade problem.

By contrast, Jagdish Bhagwati's *World Trading System at Risk* is a sharply focused defense of the multilateral trading system organized around the General Agreement on Tariffs and Trade (GATT). It is articulate, well crafted, and very frustrating. Bhagwati has an important case to make; and at the core, it is one I would largely agree with. The multilateral trading system has made great contributions to our economic welfare and is now at risk from demands for protection and strategies for advantage. A number of his proposals to reform the GATT system are useful and interesting places to begin discussion. The difficulty is that he dismisses outright the concerns underlying the demands on governments to protect or promote in ways that might threaten the GATT. Those concerns emerge from a vision of trade and an understanding of the terms and consequences of competition that differ from his. The form and language of his arguments preclude debate about the dynamics of trade, what the exact risks to (not just sources of strain on) the trading system are, or what solutions might be appropriate. He presents his arguments as if they were theoretically, analytically, and empirically dispositive. Those who would disagree with his characterization of the problems or seek other lines of solution are, in his view, apostates, holders of false beliefs. Bhagwati argues that "facile views of Gatt's impotence" are given fuel by "ignorant prattle by academic economists . . . unburdened by historical knowledge, scientific expertise, and the sustained scholarly reflect[ion] that these matters require." The list of those criticized directly in the book for various sins including theoretical fallacies (and presumably therefore spewing such ignorant prattle) include such journalists as Robert Kuttner and Jim Fallows, as well as political scientists. The economic credentials of the political scientists and journalists may be easily challenged. But also on the list are such outstanding economists as his Massachusetts Institute of Technology colleagues R. Dornbusch and Paul Krugman; Harvard economist (presently vice-president for economics at the World Bank) Lawrence Sommers; and my colleague, Laura Tyson. Many of us would share his basic concerns. His analysis and his policy prescriptions would gain greater weight by truly engaging the issues, rather than disposing of them with rhetoric. Some examples are helpful.

Consider the place of Japan in the trading system. Is Japan—the world's second largest economy with advanced and sophisticated technology and some dominant industries—continuing to follow developmental strategies that insulate its market from foreigners and promote domestic firms to create advantage in global markets? If so, competitor governments will attempt to defend their firms and competitive position with protection. The GATT will be strained. But in Bhagwati's eyes Japan either is an open, liberal economy or at least is distinct in ways that are not crucial to the trade debate. Consequently, the problem for Bhagwati are Japan-bashers, not Japan. Maybe he is right and Japan is fundamentally an open liberal economy, or at least an economy whose distinct domestic approaches are not crucial to trade issues. But the notion that Japan's protected markets were crucial to its success in the past and remain important to creating advantage and managing declining sectors is not so easily dismissed. Certainly, the debate about nontariff barriers makes argument and evidence complex. Perhaps the allegations are as hard to disprove as they are easy to make, but that does not make them wrong. Bhagwati's Japan is close to that of Sakakiabara in *Parallel Politics*. Shimada's Japan in the same volume fits the model of the Japan-bashers. Arguments by Murakami and Yammamura that Japanese business structure and policy drive a pattern of excess competition, domestic market saturation, and resultant policy intervention and dumping are never confronted and are hardly the fantasies of American Japan-bashers. The technical debate over econometric studies of Japan's trade, some of which suggest not only

discrimination but a very unique pattern of trade, is swept aside by the assertion that "the best crafted studies" favor the view that Japan's trade patterns are not distinct. Perhaps; but others, such as Bela Belassa and Edward Lincoln (former director of the Japan Economic Institute and hardly a first-line basher) would disagree about substance and methodology. They offer very convincing alternate interpretations of the data that Bhagwati introduces. There are strong and legitimate alternate positions, and Bhagwati cannot simply wave them away.

Or consider the debate on strategic trade theory. Bhagwati is concerned that these arguments, if misused, can justify protection. I would agree and argue that the marriage of strategic trade theory and arguments about technological externality can produce a potent ideological brew justifying first-strike economic strategies with protectionism as a core. But that does not make the arguments wrong. To ignore the technological externalities gained by winning a position in sectors that use large volumes of emerging new technologies (a gain that Bhagwati either avoids or denies) requires dismissing the conclusions of a range of economists including Christopher Freeman, Richard Nelson, Paul Winter, Giovanni Dosi, and many others. There are issues here that the trade regime must confront. The best way would be through an agreement on multilateral rules for investment and intellectual property (as Bhagwati and Sylvia Ostry argue); but it may be tough to reach agreement and even tougher to agree that the deals are being kept.

It is unfortunate that Bhagwati's case is not made in the language and style that permits sensible discussion and debate. The debate is an important one, extensive common ground is possible, and the remaining issues for debate are important and fascinating. For the reader intrigued by Bhagwati's views, I would commend his earlier book *Protectionism*. My substantive disagreements remain; but in this previous volume, Bhagwati makes his central case in a more sustained and reasoned way.

University of California, Berkeley JOHN ZYSMAN

In the Name of Democracy: U.S. Policy toward Latin America in the Reagan Years. By Thomas Carothers. Berkeley: University of California Press, 1991. 309p. $29.95.

The major question Thomas Carothers addresses is, Did Reagan administration policy promote or hinder the development of democracy in Latin America? Based on more and less detailed chronological reviews of administration policy toward over a dozen countries, his answer is a "qualified no" (p. 249). By Carothers' accounts, the qualifications are indeed minimal. In El Salvador, for example, administration policy, which strengthened the corrupt and "profoundly anti-democratic" military, had "adverse effects with respect to promoting democracy" (pp. 44–46, 251). Even the administration policies that for Carothers represent the "farthest point of evolution with respect to promoting democracy" do not receive high marks (p. 150). For example, U.S. assistance was only a "minor factor" in the success of the 1988 Chilean plebiscite campaign; and in Paraguay, the U.S. role in ousting Stroessner was "small" (pp. 163, 166).

Carothers addresses two subsidiary questions in less detail. First, why did the Reagan administration adopt such a strong stance toward promoting democracy in Latin America? Most of Carothers' answers are not especially surprising. Among other things, the administration promoted democracy to persuade reluctant members of Congress to approve requests for military aid, to garner support for its anticommunist policies from a public wary of such crusades, and to alter the conditions giving rise to leftist movements and social instability. Carothers also contends that the administration's stance on democracy resulted from a tendency in U.S. foreign policy making for "rhetoric to influence reality": Once the administration publicly linked its policies to democracy, it came under pressure to match its pronouncements with substance (p. 244).

What did the Reagan administration actually meant by democracy in Latin America? is the second subsidiary question of concern to Carothers. He concludes—again, not surprisingly—that the administration's definition was a hyperformalistic, electorally focused, top–down version of what conventional, Western political science understands by the term. The conception was so narrow that it allowed governments exhibiting any measure of corruption, disdain for human rights, and control by military and economic elites to be classified as democracies.

People moderately knowledgeable about U.S. relations with Latin America will not learn much new from Carothers' country-by-country summaries of U.S. policy. Nonetheless, the book is useful in providing an accessibly written, one-stop description of U.S. policy toward many nations over an entire decade. Throughout these discussions, Carothers highlights divisions within the administration, thereby enlivening his accounts considerably and helping him explain subtle policy shifts over time.

The book's strongest and most original chapter deals with political development assistance programs, with which Carothers has intimate knowledge because he worked with Agency for International Development beginning around 1985. This chapter discusses both the National Endowment for Democracy and the spinoffs of Reagan's Project Democracy initiative; and it makes it even more clear why Carothers' assessment of the administration's contribution to democracy in Latin America is largely negative. Taken together, the National Endowment for Democracy and the Project Democracy spinoffs constituted the heart of the democratic development component of administration policy. Yet these programs were minuscule compared to administration spending on military programs; and major portions of them were directed toward such activities as police training and counterterrorism. Finally, Carothers shows why the Reagan administration per se deserved less credit for these democratic centerpiece programs than might seem to be the case. For one thing, many of the Project Democracy spinoffs succeeded in accomplishing their modest goals only because of the informal efforts of individual career officers in the Agency for International Development and other agencies, many of whom had served in their capacities before Reagan's election and remained after he had departed.

Unfortunately, Carothers made a series of research choices that limit his book's value. First, though much of his discussion is based on interviews with past and current U.S. officials, Carothers shares little about these

interactions or his respondents (e.g., when the conversations took place, how many people he talked to, or how or why he selected them). As a consequence—and especially since most of those he interviewed declined to speak on the record—readers are forced to remain particularly skeptical of many of Carothers' assertions.

Second, the author chose not to deal with U.S. policy toward Cuba, despite the country's relevance to his topic. For one thing, the administration played relentlessly on the democracy theme vis-à-vis its Cuba policy. For another, Carothers notes, time and again, how the perceived threat from that country was foremost on the minds of administration officials as they sought to develop policies toward many of the countries he does discuss. The author's decision to ignore U.S. policy toward Cuba, even while including treatments of U.S. policy toward countries of inarguably less significance, produced a noticeable and unnecessary gap in the book.

Third, Carothers made some unfortunate and poorly justified choices in his use and treatment of some key concepts. The first was to ground his critique of Reagan's policies in a conventional, Western political science definition of democracy, rather than in a broader, more inclusive understanding of the term that would link politics and policies to social and economic considerations. Second, he failed to take seriously enough his own goal of disentangling the rhetoric and reality of administration policy. Rather, his tendency is often is to confound the two.

Carothers' choices on these two matters are related to the book's most serious shortcomings. Often, the author's critiques fall flat and his analyses lack sharpness. There seems to be an odd mismatch between his forceful descriptions of policy development and implementation and his bland evaluations of these processes. At times, it is almost as if one were reading two different books. Finally, Carothers is left with an inadequate stock of tools with which to make sense of his data, forcing him, time and again, to retreat into additional, more detailed presentations of facts and events. The upshot is a book that, in my view, is too long on description and too short on insightful explanation. It is primarily at the moments Carothers' allows himself to distinguish clearly between rhetoric and reality and to make connections between politics and the social and economic contexts in which they are embedded that his book is at its best.

University of Oregon LINDA FULLER

Exploring Realpolitik: Probing International Relations Theory with Computer Simulation. By Thomas R. Cusack and Richard J. Stoll. Boulder: Lynne Rienner Publishers, 1990. 209p. $32.00.

Cosmologists and international relations theorists are faced with the "$N = 1$" problem: while they can study subsystems, only one example of the overall system exists. Computer simulations provide one route out of this conundrum. Just as cosmologists can simulate the aggregation of scattered atoms into stars and galaxies, so computer models of political consolidation can show the development of states from smaller territorial units through conflictual interaction.

Cusack and Stoll present a systematic and detailed analysis of one of the best known of these models, based on Stuart Bremer and Michael Mihalka's "Machiavelli in Machina: Or Politics Among Hexagons" (1977). The idea of simulating political aggregation through territorial conflict is a fairly general one. It had been anticipated by Michael Dacey in 1970, and a similar model for urban segregation was developed simultaneously by Thomas Schelling in *Micromotives and Macrobehavior* (1978).

Cusack and Stoll use the model to explore a number of questions raised by realist theory, in particular, how far power-oriented self-interest creates an "invisible hand" that prevents a multistate system from degenerating into a single "empire." The book includes a chapter-length, amply footnoted discussion of realism, including many of its classical, neo-, and post- variants. The authors find realism to rest on a "fractured" and disparate foundation, though this may be due in part to their casting a net so wide as to include as "realist" virtually all theories that posit the use of "power."

The model contains a variety of parameters such as the accuracy of power estimation, the cost and destructiveness of war, and the variance of power growth rates, as well as several different decision-making rules. This project, though spun off from the GLOBUS simulation, (an elaborate and highly parameterized model), is relatively parsimonious. The analysis is further focused by concentrating almost solely on the question of multistate maintenance. The impact of the parameters is analyzed using statistical analysis on the simulation outcomes following a sophisticated experimental design involving tens of thousands of simulation runs.

The general behavior of the simulation proves remarkably insensitive with respect to most of its parameter values. While each has some influence, by far the most important is the function relating the ratio of the power of two antagonists to the likelihood of victory. In the basic model, multistate systems are reliably sustained only when victory requires a very high ratio; otherwise, about 50% of the simulations coalesce into a single empire.

The experiments with the decision rules, on the other hand, produce a variety of behaviors. Collective security systems and, to a much lesser extent, balancer systems are more likely to preserve states than simple power seeking. While this is a plausible result, the authors note that some theories have argued against collective security efforts as needlessly exposing states to risks. The model thus usefully distinguishes among theories. Expected utility maximization, alliances, and internal revolt are some of the other behavioral variations explored with the simulation.

How realistic is this model? To the extent that one is interested in territorial aggregation determined by relatively simple rules involving comparison, maximization, and occasional expected utility calculations using power, the model is probably quite robust. While there is more to realism than just these calculations, the model provides a systematic exploration of behaviors that are all too often simply assumed in realist theory. Granted, the model says nothing about the Rio Summit or the Uruguay Round; but it probably says a lot about Yugoslavia, Armenia and Ethiopia.

Even the tendency of the model to result in empires may be consistent with observed behavior. The units in the simulation need not be Westphalian states; they require only power, territory, and autonomous decision making. The evolution of a decentralized system into a "universal empire" was observed in subsystems such as Tudor England, fifteenth-century Spain, Tokugawa Ja-

pan, and China in 1945–50. The apparent stability of the multistate system we see today may be due in part to prior periods of "imperial" aggregation within its subsystems.

Most of this work was done prior to 1989 and focuses almost entirely on aggregation. While one cannot expect prescience of the authors, it would be very interesting to see greater attention devoted to the issue of dissolution of units through internal instability. Fortunately, since the computer code for the simulation is available from the authors and can be run on any reasonably powerful personal computer, such experimentation is possible for other researchers.

In summary, Cusack and Stoll provide a thorough analysis of a basic model of political power, decision making, and territory. It is neither the first nor the last word on the subject but provides a useful baseline for further experimentation and is to be recommended to individuals interested in formal models of international behavior.

University of Kansas PHILIP A. SCHRODT

U.S. Foreign Policy and the Shah: Building a Client State in Iran. By Mark J. Gasiorowski. Ithaca: Cornell University Press, 1991. 242p. $35.00.

American Presidents and the Middle East. By George Lenczowski. Durham: Duke University Press, 1990. 321p. $40.00 cloth, $17.95 paper.

It is becoming increasingly commonplace among political scientists to recognize the impact of domestic politics on international politics, and vice versa. The two books under review represent the intricacies of this dialectical relationship. The volume by Lenczowski looks at how American presidents from Truman to Reagan have articulated their foreign policy toward the Middle East in light of domestic considerations at home. The book concentrates on the eight presidents' perceptions, priorities, and policies vis-à-vis the Middle East. However, as the author informs us early on, this is not "a psychopolitical analysis of the presidents' personalities and motivations" (p. 5).

Drawing on over 40 published memoirs by presidents, secretaries of state, national security advisors, and ambassadors, as well as Israeli, Egyptian, Iranian, and British statesmen, Lenczowski provides a lucid amount of how post–World War II American presidents viewed the Middle East. He identifies containment of the Soviet threat, survival of Israel, and accessibility of oil supplies as the three constant concerns of American policymakers in the region (p. 281). Lenczowski maintains that the first was considered to be the most important, while the priority of the other two fluctuated depending on the administration in power.

Although *American Presidents and the Middle East* deals with a host of international crises originating in the Middle East (Suez, Mosaddeq's oil nationalization, the civil war in Yemen, Cyprus, and the Soviet invasion of Afghanistan), the central focus of the book is the Arab–Israeli conflict and U.S. involvement in it. The author pays particular attention to the pressures put on successive U.S. administrations by Israel and the American Jewish community. We are told of Truman's "misgivings and caveats" about supporting Zionist objectives (p. 26); Eisenhower's standing up to Israeli pressures; and Johnson's unequivocal support for Israel, which he regarded as a "strategic asset." Furthermore, Lenczowski insists that Nixon and Reagan's desire to appease strong constituencies at home and their Cold War view of the Soviet Union led them to seek closer ties between Israel and the United States. Overall, he considers Johnson as the most vulnerable, Eisenhower the most impervious, president when it came to domestic pressures (p. 280).

Lenczowski's work suffers from three weaknesses. Despite his treatment of certain domestic considerations and constituencies, many important factors and details are left out. The reader is not adequately informed of the compass of political impediments, interest calculations, domestic preoccupations, and partisan congressional agendas each respective president had to grapple with. The most conspicuous omission of the book, however, is any discussion of how presidents tried to build public support for their foreign policies at home in the midst of all these crises. Second, by relying exclusively on memoirs and certain official documents from the executive and the legislative branches, the volume suffers from its sparse source material. Considering the fact that political memoirs are more often than not marred by a sense of self-flattering and self-conceit, the author could have enhanced the value of his work by qualifying and corroborating his sources with other competent accounts that are available. Finally, Lenczowski does not refrain from interjecting his personal value judgements in the book. For example, he brands the Iranian Prime Minister Mosaddeq as "thirsty for power," his oil policy as "reckless" and his sense of nationalism as "misguided" (pp. 36, 64).

In contrast to Lenczowski's book, *U.S. Foreign Policy and the Shah* by Mark Gasiorowski deals with the exogenous determinants of domestic policy. In this original and highly competent book, Gasiorowski examines the nature and progression of the "cliency" relationship that emerged between the United States and Iran in the aftermath of the 1953 coup overthrowing Premier Mosaddeq. After defining *cliency* as "a mutually beneficial, security-oriented relationship between the governments of two countries that differ greatly in size, wealth, and power" (p. 2), the author provides a promising theoretical framework for how international cliency relationships can influence the domestic politics of client countries.

Gasiorowski's main argument is that economic aid and security assistance provided by patron powers to their client states empower the latter to seek autonomy from civil society. Lacking any societal constraints, the state can begin to defy all demands for political participation, thereby leading to serious unrest, instability, and even revolution. Henceforth, the initial intention of patron powers to maintain political stability in their often strategically located client states may inadvertently produce the opposite results.

In seven detailed chapters, Gasiorowski examines the nature of state formation in Iran over the last four decades. He maintains that the formation of the cliency relationship between the United States and the Shah and the rapidly augmenting oil revenues in the 1960s and 1970s helped to initiate and sustain a highly autonomous Iranian state in the prerevolutionary period. Gasiorowski contends that a mere 10 years after the overthrow of Mosaddeq, an autonomous Iranian state was firmly in place (p. 187). This state's premeditated use of repression as its most important autonomy-

enhancing mechanism contributed to the advent of the Islamic revolution.

In his analysis of the 1953 coup, which can quite properly be regarded as a turning point in modern Iranian history, Gasiorowski has left no stone unturned. Utilizing a wealth of recently declassified documents from the British Public Records Office, the U.S. National Archives and the U.S. Department of State, in addition to over 70 interviews with British, American, and Iranian officials and activists involved in the event, Gasiorowski has produced by far the leading study on the subject. Through his meticulous use of primary and secondary sources, Gasiorowski has authoritatively augmented our knowledge of that tempestuous event and its tragic aftermath.

This book, however, is not without its shortcomings. Many political economists would argue with Gasiorowski's assertion that cliency was more important than oil as a determinant of state autonomy. It is highly doubtful whether the state could have sustained its autonomy from civil society to such a degree and for such an extended period of time without the petrodollars. One should not lose sight of the fact that the financial largess of the state, provided to it by oil exports, enabled it to coopt an already-subdued polity. Second, the author's identification of the National Front (led by Mossadeq) as the main political organization of the modern middle class (p. 84) may be somewhat of an oversimplification, considering the eclectic constituency base of that party, which drew support from disgruntled *bazaaris* and the Qashqai tribe, in addition to the urban secular middle class.

Lenczowski and Gasiorowski respectively demonstrate that a great power can be pressured by a small ally and its vocal domestic supporters and that patron states should beware of enabling client states to become highly autonomous. Their virtue resides in the fact that by making the confluence of domestic and international policies problematic, they can provide all students of foreign policy with much food for thought.

Syracuse University MEHRZAD BOROUJERDI

Pathways from the Periphery: The Politics of Growth in Newly Industrializing Countries. By Stephan Haggard. Ithaca: Cornell University Press, 1990. 276p. $12.95 paper.

Stephan Haggard seeks to compare the development policies of the East Asian "tigers" (Korea, Taiwan, Hong Kong, Singapore) with those of two Latin American countries (Brazil and Mexico) and to account for the differences. More than that, however, he wants to explain the policy changes adopted in each of the countries during the postwar era.

The East Asian strategy of export-led industrialization has been much more successful than the import substitution strategy (ISI) adopted throughout Latin America. But Haggard points up an interesting paradox. Among the reasons ISI was instituted in Latin America was the belief—shared by technocrats and representatives of the organized working and middle classes—that ISI in a protected national market would allow for higher average wages and, consequently, greater distributive equity and less dependence on forces beyond national control. Export-led industrialization, in contrast, was thought to entail severe wage restraint and dependency. Yet the outcomes have been the diametric opposite of these expectations: the data cited by Haggard show clearly that all but one of the "tigers" score higher than the Latin Americans on Gini measures of distributional equity and on quality-of-life indexes such as average level of education, longevity, and infant mortality rates.

Haggard, in seeking to unravel this paradox, is mindful of the impact of international forces and pressures. He notes, for instance, how both Korea and Taiwan actually began their industrial development along ISI lines but then shifted to export-led industrialization as declining U.S. economic assistance made the former policy less viable. However, his explanatory framework prioritizes domestic factors, since no theory of the international system can explain policy differences between developing countries whose situations are otherwise similar. For Haggard, the key domestic factor is the structure and capabilities of the state: in some countries, "political elites have inherited or built organizational structures that significantly constrain the ability of societal actors to achieve their political and economic objectives" (p. 3). This is important because successful development requires more or less drastic policy changes to adapt to changing world conditions or redress demonstrated failures. But any strategy of development creates social forces with a stake in it; and if those forces are well organized and politically mobilized, their tendency is to use their political power to block changes that would adversely affect their interests.

Consequently, the study of development policy becomes the study of efforts by ruling elites to build supporting coalitions in the context of a set of state institutions "that provides differential incentives for groups to organize. Because of variations in institutional structure, political elites differ in their organizational capabilities and the instruments they have at their disposal for pursuing their goals. . . . It is at the intersection of choice and institutional constraint that political explanations of economic growth must be constructed" (p. 4). Hence, Haggard eschews rational choice approaches to explaining policy output, although he admits that they can shed light on the actions of the contending societal groups.

This position reduces to restating development policy choice as a collective action problem along the lines discussed by Mancur Olson: when societal groups can organize to influence policy, they form coalitions to secure benefits for themselves and thereby restrict both the state's ability to choose among available policy options and the coherence of policy itself. Haggard thus returns us to the argument of the 1960s (which much of the subsequent literature has been at pains to refute) that development may be less than fully compatible with an early broadening of democratic norms and institutions. To be sure, Haggard is not an advocate of all political authoritarianism, since he knows that such regimes may be penetrated by dominant groups with partial interests. In Latin America, where this penetration has been frequent, "the association between industrial strategy and authoritarian rule . . . appears to be weak" (p. 255). Rather, the authoritarianism he favors is the sort labeled "state autonomy." The most effective development policies, he avers, are products of "those regimes which limit autonomous political organization and public contestation," whether through the direct use of state power or via corporatist controls (pp. 44–45).

It seems to me that Haggard is tacitly assuming that the authoritarian order is led by people who are both aware of, and devoted to, the public good—or at least have an interest in a form of development that will benefit a wide range of social sectors. It also seems that he has presumed a high degree of administrative expertise at the disposal of these leaders. (Although the cases he has chosen give him no reason to make the point explicitly, he plainly excludes personalist authoritarian regimes like, say, Mobutu's in Zaïre or Somoza's in Nicaragua.) But what about communism, which would seem to meet all the requirements of a "desirable" form of authoritarian control? To say that communism failed owing to its rejection of markets and fear of the international economy begs the question; for that answer implies the significance of leaders' *beliefs* about the nature of the world and the kind of society they want to build, and Haggard does not discuss such beliefs, how they are formed, or how they are affected by international influences. While he makes mention of "economic ideologies" (pp. 46–47), these are professional understandings of economics and policy, transmitted through international professional networks and institutions. They are not the worldviews and moral interpretations of social reality that we usually mean when we employ the term.

There are, of course, many other reasons why committed democrats will not rush to embrace Haggard's claims. One is the present coincidence between the restoration or strengthening of Latin American democracy and the most serious effort yet undertaken in the region to undo the excesses of statism-cum-ISI. A second is the suspicion that the political practice of subordinate social groups may be informed by stronger democratic impulses and a better sense of the general welfare than we have assumed. Richard L. Sklar is attempting to build a theory on just this basis ("Developmental Democracy," *Comparative Studies in Society and History* 29[1987]). His efforts remain at a preliminary stage of exposition; but, in my opinion, they are already fruitful enough to give us pause.

Haggard's book should serve well as a text for graduate and advanced undergraduate courses in development policy. It may not be the last word on the subject. Apart from its uncertain view of political democracy in developing countries, its relative inattention to class forces deprives it of explanatory power in regard to underlying processes of social and political change. But students will learn the truth about the industrialization of East Asia—that is, that this process depended and still depends on heavy state intervention—and will profit from the comparisons between East Asia and Latin America. They will also be stimulated to reflect on the thought that the shortest distance between the objective of equitable development and the result may not be a straight policy line.

Dartmouth College DAVID G. BECKER

Peace and War: Armed Conflicts and International Order 1648–1989. By Kalevi J. Holsti. New York: Cambridge University Press, 1991. 379p. $54.50 cloth, $15.95 paper.

Focusing upon foreign policy issues, particularly those over which states fight violent conflicts, Holsti attempts a systematic treatment across three-and-a-half centuries. This in itself is a stupendous exercise. Link the size of this empirical effort with the amount of knowledge required to make the analysis meaningful politically and the conclusion must be that this is a major work of scholarship indeed.

The book combines empirical research on international relations with diplomatic history; assessment of foreign policy treaties and their aftermath; and the strategic outlook of statesmen, that is, political realism with an admixture of idealism. Further, study of the assimilation of defeated hegemonic states is an important dimension of this work. Holsti carries the examination of the Peace of Westphalia (1648) and the Treaties of Utrecht (1713), each of which ended and began a new system, through the more familiar Congress of Vienna (1815), Peace of Versailles (1919), and the San Francisco treaties that led to the establishment of the post-1945 bipolar system. Contained within these portentous treaty constellations are the issues over which the prior system collapsed and a new system was born.

A great attraction of the book is how it unearths the visions of the architects of statecraft like Stanhope and Talleyrand, Metternich and Castlereagh, as well as those of the rogues like Hitler and Stalin. By staying close to the individuals who made foreign policy, this assessment of international relations possesses authenticity.

Perhaps the highpoint of Holsti's writing comes with his evaluation of the thought of Woodrow Wilson. Wilson was enigmatic. He was magnanimous and punitive. He was interventionary (in Latin America) yet the opponent of power politics at Versailles. He was a dreamer yet ruthlessly disregarded the concerns of Lloyd George and Clemenceau. He was a great proponent of public opinion yet remained ignorant of the opinion at home and in the Senate that would kill his grand idea. Wilson was the hero of self-determination and democracy at a time when neither could be fully effected and when the greatest danger still lay with old-fashioned nationalism and dictatorship. Still today, it is Wilson, more than perhaps any other diplomatist, who has left an imprint on the theory of international relations, showing that a world of democracies is less likely to be war-torn than its contrary.

According to the descriptive statistical findings Holsti reports, the place of territory in the disputes of governments has declined. Territory as an issue has gone from a high of about one-quarter of the total disputes in the seventeenth and eighteenth centuries to less than one-tenth in the late twentieth century. This finding could lend support to the notion of the demise of the territorial state. But if one combines all of the other categories of issues involving territory—namely, strategic territory, territory (boundary), national liberation as state creation, national unification and consolidation, secession as state creation, and territory per se—the inference is quite different. Together, territory in this larger sense accounts for about a third or more of all issues throughout the period. Territorial dispute remains, in its many forms, the most problematic issue facing states today.

Despite the great conceptual and empirical difficulty of determining how many foreign policy issues pertain and which issues are more central in any given dispute, Holsti possesses a data set that, when further analyzed, may produce other important insights. For example, the relationship between the size and seriousness of an armed dispute and the issue or issues at stake is of great interest. It is to be expected that certain core issues

regarding role and status, as well as security, will predominate in the most violent and major of confrontations. Holsti has not yet tested this hypothesis, though he might.

Combined with its astute investigation of the attitudes of the statesman, the empirical findings of this pioneering work make this book rich ground for subsequent theory development.

Elaborated in the final chapters is the difference between the system of the advanced industrial states (virtually the central system, or the great-power system, in other parlance) and the system of states in the Third World. Surely, Holsti is correct that governments within each of these systems or subsystems pursue some divergent foreign policy aims. His assertion that the former is preoccupied with avoidance of nuclear war while the latter is more focused upon the problem of the security of the state and the survival of the governmental regime is undoubtedly correct. Though nuclear proliferation may yet blend these heretofore divergent subsystemic anxieties, nevertheless, to the extent that the types of conflict differ for each subsystem, so may the reasons for war.

A full incorporation of other approaches to world politics with the splendid empirical assessments of this volume promises many further insights into the cause and results of aspirations to world order.

Johns Hopkins University CHARLES F. DORAN

Dominoes and Bandwagons. Edited by Robert Jervis and Jack Snyder. New York: Oxford University Press, 1991. 299p. $37.50.

During the Cold War, the domino theory and a series of related metaphors (e.g., "rotten apples in a barrel," "weakest links in a chain," etc.) lay at the heart of American foreign policy in small countries around the world and largely accounted for the pattern of U.S. intervention in Third World revolutions, especially in Vietnam and Central America. The domino theory held that decisive action had to be taken to prevent the fall even of small countries of little or no intrinsic political, economic, or strategic significance lest the fall of such countries set in motion a chain of events inexorably leading to communist takeovers in country after country throughout entire regions. Indeed, the consequences might even be global, for falling dominoes might undermine the credibility of U.S. commitments elsewhere, leading to a "bandwagoning" effect as neutral states and even allies rushed to accomodate the apparently inexorable communist wave.

Despite its critical significance to American foreign policy in the Cold War—and indeed (as Snyder and Jervis show) to the policies of great powers throughout history—there have been remarkably few systematic, scholarly analyses of the domino theory or empirical/historical evaluations of its validity. This excellent book has gone a long way to fill the gap, though there still is a need for detailed historical case studies (e.g., for Vietnam.

In their introductory and concluding chapters, Jervis and Snyder provide historical and theoretical overviews of the domino theory, bandwagoning, and related metaphors. The other essays, all of them first-rate, include analyses by Stephen Walt and Deborah Welch Larson of bandwagoning, balancing, and alliance formation; Douglas J. Macdonald's discussion of the domino theory during the Truman administration; Ted Hopf's assessment of the Soviet Union's reaction to the domino theory; and other analyses of Soviet policy in the Third World by Douglas Blum, Richard Herrmann, and Milan Hauner.

While there are some differences among the contributors, most are very skeptical of the domino theory. The most important overall conclusion is that while, historically, there have been some circumstances where dominos have fallen or states have jumped on bandwagons (e.g., Eastern Europe, 1989–91), much more typically, the theory fails to predict the behavior of states. There are a variety of reasons for this, including the inapplicability of events in one country or region to very different circumstances in other countries or regions, as well as the power of nationalism in creating obstacles to expansionist states or ideologies. Beyond that, one of the most important arguments of the authors (especially Jervis, Snyder, and Walt) is that the domino theory is more often a self-denying, than a self-fulfilling, prophecy. When the first in a feared chain of dominoes fall (as happened in China in 1949, in Cuba in 1959, and in Vietnam and Angola in 1975), neighboring states are much more likely to balance than to bandwagon—that is, they shore up their alliances, rebuild their military power, take preventive action to avert revolution, and so on. Moreover, as Hopf demonstrates, after Vietnam, Soviet behavior in the strategically significant areas of the world was more cautious, rather than more aggressive; for Moscow correctly anticipated that the West would redouble its efforts to prevent any significant shift in the global balance of power.

For these reasons, then, the fall of Vietnam in 1975 had little lasting effect on the Cold War conflict. Some neighboring small states of no intrinsic significance quickly succumbed to communism (Laos, Cambodia); and there was a brief surge of Soviet support for a handful of Third World revolutions elsewhere, especially in Africa; but there was no widespread collapse of dominoes. Even in Asia, none of the large or truly important states, like Japan or India, were affected; and there was certainly no loss of U.S. credibility or rush to accomodate elsewhere, as in Europe or the Middle East. Jervis persuasively explains why: "The domino theory exaggerates the role of reputational interests in determining the outcomes of confrontations and the inferences observers draw and commensurately underestimates the importance of intrinsic interest. . . . States seem able to separate peripheral from vital interests" (p. 42).

There is little reason to think that assessments of the domino and bandwagoning theories have lost their relevance in the post–Cold War era. As the authors demonstrate, in all historical eras, there has been a tendency for dominant powers to adopt domino theories in order to justify interventionist behavior. Precisely because of the collapse of the Soviet Union, the United States is militarily even more dominant than ever. Therefore, it is still vulnerable to the imperial temptations that ultimately account for the domino theory.

State University of New York, Buffalo JEROME SLATER

The New European Community: Decisionmaking and Institutional Change. Edited by Robert O. Keohane and Stanley Hoffmann. Boulder: Westview, 1991. 208p. $49.95 cloth, $14.95 paper.

Euro-politics: Institutions and Policymaking in the "New" European Community. Edited by Alberta M. Sbragia. Washington: Brookings Institution, 1992. 303p. $32.95 cloth, $14.95 paper.

The New European Economy: The Politics and Economics of Integration. By Loukas Tsoukalis. New York: Oxford University Press, 1991. 333p. $52.00.

The titles of these three volumes have in common the word *new,* in recognition of the momentous changes that have taken place in the European Community (EC) since the mid-1980s. The writings found in them may be characterized as representing "new thinking" on the institutions, policymaking and economics of the EC as of the early 1990s. With the exception of Stanley Hoffmann, the writers are also "new" to the debates regarding the nature of the beast, debates that raged in the 1960s and then exhausted themselves during the period of Eurosclerosis of the 1970s and early 1980s.

The volume edited by Hoffmann and Robert Keohane contains essays written in 1989 and 1990 that sought to gain better theoretical purchase regarding the genesis and political meaning of the 1986 Single European Act (SEA). The other two works were produced during the period of the intergovernmental conferences (1990–91) that led up to the Maastricht summit of December 1991.

Although the discussion brings fresh perspectives to bear on new phenomena, the books edited by Keohane and Hoffmann and by Sbragia, written mostly by political scientists and addressing essentially political topics, are haunted by the specter of the earlier debates, particularly in their frequent uses of the terms *neofunctionalism, neorealism, intergovernmental,* and *supranational.* The best job of sorting these terms out is done in the Sbragia collection by David Cameron in his essay "The 1992 Initiative: Causes and Consequences." The first two terms are treated by Cameron as labels for theoretical perspectives, while the latter two are applied to EC institutions. Basically *neofunctionalism* highlights the role of "supranational institutions," particularly the EC Commission, in making decisions that in Ernst Haas's terms, "upgrade the common interest" among member states. *Neorealism* emphasizes the role of "intergovernmental institutions" (the Council of Ministers and, esp., the European Council) in arriving at decisions that express the "lowest common denominator" of agreement among members.

In their introductory essay, Keohane and Hoffmann argue that none of these terms exactly applies to how decisions are made in the EC today. Neofunctionalism produced the "spillover hypothesis" that predicted a strengthening of supranational institutions as a result of the consequences of EC policies pursued in one economic sector for the performance of the member countries' economies in other sectors. Spillover and the even more economically deterministic "international political economy" hypothesis would have predicted a strengthening of supranational institutions during the economic crises of the 1970s to address the dislocations produced by the energy crises and national macroeconomic policy failures. They acknowledge that development of the European Monetary System (EMS) in the late 1970s provided the hypotheses partial support. However, swifter progress toward the internal market and a full Economic and Monetary Union (EMU) should have occurred but did not. They do not make a similar effort to weaken a third hypothesis, which they label "preference convergence," namely, the prediction that decisions will be reached only if the preferences of member states converge. This stems from neorealist premises and stresses intergovernmental institutions. Nor do they suggest that the convergence which occurred in 1984–85, making the SEA possible, was brought about by factors common to the principal actors. This sets the stage for the next essay, by Andrew Moravcsik, which gains attention from Cameron and other writers in the Sbragia collection.

Moravcsik makes a strong case for the preference convergence hypothesis, couched in the sense that member states can be expected to reach only the lowest common denominator of agreement. In the case of the SEA, the principal actors on which a neorealist would focus are West Germany, France, and Britain. Moravcsik details the preferences of the Mitterrand administration and the Thatcher government that converged on the SEA, a package agreement that combined the elimination of barriers to the EC internal market with relaxation of the unanimity convention regarding Council of Ministers voting on most pieces of internal market legislation. Prior to the agreement, France and West Germany had acquiesced in a budget agreement that went a long way toward satisfying Britain's complaints about EC funding burdens. While there was substantial agreement among the three as to the desirability of the market-freeing aspects of the SEA, Moravcsik's analysis makes it clear that France and Germany gave ground on the budget issues (esp. France, given the decision to lessen the Common Agricultural Policy share of the budget), while Britain gave ground on the voting procedure issue. This suggests more an upgrading of the common interest than the arrival at the lowest common denominator of agreement among the three. However, this is not to disagree with Moravcsik's more general contention that member governments acting within intergovernmental institutions played the decisive role in the process.

Other writers in the two volumes are more reserved on that score, since they pay more attention to the role played in the formulation of the SEA by the European Commission, as the leading supranational institution. Cameron argues that the commission was laying the groundwork for the SEA with specific internal market proposals during the early 1980s when the member governments were confining themselves to broad statements of intent, rather than to the concrete actions that began with the Stuttgart summit in 1983. This was before Jacques Delors became commission president at the beginning of 1985. Cameron appears to agree with Moravcsik that Delors's role could not have been decisive before the June 1985 Milan summit. But that was the point at which the package deal began to take shape, and Cameron believes that Delors played an important strategic role in bringing the deal about. In more general terms, Peter Ludlow, in his balanced essay "The European Commission" points to the "educative" role of commissioners in helping governmental leaders to "learn" of the necessity for common action to be taken to deal with common problems, a perspective that has neofunctionalist overtones. Cases in point, in addition to Delors in the latter 1980s, were the roles played by

Commission President Roy Jenkins in helping to bring about consensus in support of the EMS in the late 1970s and by Commissioner Etienne Davignon in fostering, in the early 1980s, industrial policy cooperation among major European companies that later supplied influential support for the SEA.

Efforts are made by other writers to transcend, or at least escape, the intergovernmental/supranational polarities as conceptual tools. In his essay "The EC Council," Wolfgang Wessels makes a distinction between the constitutional decisions and package deals made at the European Council summit meetings and the ordinary work of the Council of Ministers, involving "iron triangles" familiar to students of the American political system. In the Sbragia collection, *Europolitics*, B. Guy Peters makes a similar distinction in his essay "Bureaucratic Politics and Institutions of the European Community." According to Peters, there is "the level of very visible *haute politique*, in which the leaders of the member nations meet as the European Council to negotiate among themselves over the future shape of Europe," and a second level, that of "the bureaucracy functioning within the Commission" (p. 76). Wessels subscribes to the idea that innovative acts of the European Council, which others take as evidence of intergovernmentalism, are responses to economic imperatives that the heads of state and government take in common. Hence, the European Council has become a supranational body that upgrades the common interests. For his part, Peters resuscitates Martin Heisler's 1873 European Polity model as one appropriate for how intergovernmental bodies make decisions in the new era of qualified majority rule; that is, the motivation to join in common decisions and avoid isolation is stronger than under the unanimity rule. Thus, both the European Council and the Council of Ministers are more apt to reach decisions promotive of integration than to protect narrowly defined national interests.

More transcendent is the attempt by Alberta Sbragia in her concluding chapter to expand the concept of federalism to characterize what the EC is becoming in political/constitutional terms. According to Sbragia, the idea of federalism that has prevailed in the debates about the EC has been too narrowly influenced by the essentially unique brand of federalism found in the United States. Germany, where the member states are more powerful than those in the U.S. federal system, would provide a better model for understanding the EC today and for projecting it into the future. When theorists friendly to European integration have thought about an end-state, they have found it difficult to escape something like the U.S. version of federalism. As the EC moved further and further away from that model, especially as its intergovernmental features became more pronounced, such theorists came to despair of their desired goals' ever being achieved. Yet a looser, more flexible, concept might have served them better, in Sbragia's opinion.

Whether these imaginative efforts to go beyond the old debates will take on adherents would be impossible to predict at present. The rejection of the Maastricht Treaty by Danish referendum has highlighted the problem of the "democratic deficit," which Shirley Williams discusses with considerable insight in "Sovereignty and Accountability" in the Keohane and Hoffmann collection. Perhaps the problem of how to associate the mass public in what has been until now an essentially elitist set of institutions will come to preoccupy more students of the EC in the next decade; or perhaps it will be the problem of reconciling the internal market with the need to protect the "losing" regions, economic sectors, and social classes—a problem addressed in essays by Gary Marks and Peter Lange in the Sbragia volume. Such problems will likely be exacerbated with EC expansion, about which little of substance is said in either volume. Other supranational institutions are also considered: the European Court of Justice by G. Federico Mancini, a judge on the court (in Keohane and Hoffmann) and Martin Shapiro (in Sbragia) and the EMS in a sharply analytical essay by John Woolley (in Sbragia). Unfortunately, only a mention of these fine essays is possible here. In short, both volumes provide for political scientists a wealth of useful interpretations of recent complex and largely unanticipated developments.

For the political scientist who has difficulty mastering the complexities of EC economic policies, *The New European Economy* by Tsoukalis will be indispensable. While essays in the Sbragia volume by Cameron, Woolley, Marks, and Lange are written in a political economy mode by political scientists, they stress the political, more than the economic, factors operating in EC economic policymaking. Tsoukalis, an economist, provides a clearly written account of the economic factors at play while continually calling attention to political antecedents and consequences. As economists are inclined to do, he opposes the "market" and the "state," assigning efficiency to a free market and stability and equity to politics. His own preferences are by no means exclusively on the side of economic efficiency. Whereas writers in the Keohane–Hoffmann and Sbragia books concentrate for the most part on the issue of whether power is becoming more centralized in EC institutions, Tsoukalis addresses what may well be the more fundamental issue: Will the EC remain simultaneously a market-liberating "economic giant" and an economy-shaping "political dwarf"? He interprets the SEA and the steps toward the EMU as means by which the member states have been depriving themselves of the instruments of economic control without allowing the institutions of the EC to acquire compensating control measures of its own.

A "spillover hypothesis" that could be derived from Tsoukalis' analysis would suggest (1) that as the distributive consequences of the SEA reveal themselves, at least in times of recession, to be negative for competitively weaker firms, economic sectors, regions, and member countries, interest groups and governments affected will press for the acquisition of redistributive powers by the EC and (2) that as the EMU shows itself to be an effective means of maintaining price stability while removing the capacity of exchange rate fluctuation and the countercyclical uses of fiscal policy by member states to maintain stable economic growth without high unemployment levels, affected interests will seek a transfer of fiscal capacity from the national to the EC level. In both scenarios, EC powers would expand accordingly.

Tsoukalis' analysis shows that such neofunctionalist expectations remain dubious in the present context. Neither the social policy nor the revenue-gaining powers of the EC are as yet very impressive. Member governments are retaining their holds on both, even though their capacity to wield these policy instruments for purposes of redistribution and economic stabilization have diminished substantially since the 1960s. In other words, a reading of Tsoukalis leaves one skeptical about

whether the SEA and EMU are good examples of neofunctionalist spillover. More likely they represent a "spilling out" of political capacity into a void. If we follow Tsoukalis, then, without a strong Eurostate there can be no effective regional or industrial policy, no "Fortress Europe," no social corporatism, and (very likely) no public acceptance.

University of Missouri, Columbia DAVID M. WOOD

Adjusting to Reality: Beyond "State Versus Market" in Economic Development. By Robert Klitgaard. San Francisco: International Center for Economic Growth, 1991. 303p. $22.95 cloth, $11.95 paper.

Robert Klitgaard possesses a genuine talent for communicating with broad audiences about serious matters of public policy. As with his previous, highly successful *Tropical Gangsters* (1990), *Adjusting to Reality* explores themes of great interest to scholars and policymakers in an engaging and accessible way. In the case of *Tropical Gangsters*, practical problems of development assistance motivated the work; in this case, it is the problems associated with structural adjustment.

There is a serious analytic core to this book. The argument is grounded on the theory of market failure, in particular, the problems arising from imperfect information. Given market failure, Klitgaard emphasizes, it makes little sense to talk of a return to the market or of an abandonment of the state. In making this point, Klitgaard goes further than have others to offer specific steps that governments can take so that "government failure" does not merely replace "market failure" in the making of public policy. In making his case, Klitgaard draws from a variety of his own experiences. Most centrally, he works with materials from Bolivia and various parts of Africa. The skill with which he employs case materials imparts an immediacy and freshness to this work.

While there is thus much to admire in this book, there is also much to criticize. While engagingly written, well grounded analytically, and communicated with a sense of immediacy, the book comes across as superficial. There are several reasons. One is endemic to the development field—the tendency to advance single-factor explanations. Once it was population; at another time, the terms of trade; most recently, parasitic governments. In *Adjusting to Reality*, it is imperfect information. One can legitimately ask, Even if a government did all that Klitgaard says it should, would the economy grow? The answer is that we do not know, because so many other factors, besides the absence of high-quality information, are at work. Klitgaard's broader point—that there remains a broad role for governments, even in market-based economies—is, of course, a good one and well worth making. But by narrowing the argument to a single source of market failure, he threatens to make costly information the flavor-of-the-week in a field already too given to faddism.

A second reason the work will not ring true to many is that it is dangerously apolitical. In this book, there are decision makers but not politicians; there are policymakers but not presidents or generals who seek to use their control over office to build coalitions, win elections, or undertake those other steps that must be taken in order to stay in power. Suppose it true that if a government followed the recipes offered in this book, development would occur. One might still wonder whether all decision makers would be equally motivated to follow them. The factors that would influence the eagerness with which they would do so would be precisely those factors of interest to political scientists. They have been studied by such scholars as Ronald Rogowski, Jeff Frieden, Joan Nelson and company, Robert Kauffman, Steph Haggard, and many others—scholars who have focused on the *politics* of structural adjustment. They are not analyzed here.

The two criticisms can perhaps be more persuasively registered if they are combined. Suppose we focus on other sources of market failure—public goods, externalities, or imperfect competition resulting from the large size of "special interests." Then it would be inconceivable for politics not to become the central focus of the book. The policy counsel and technocratic solutions provided in this volume would be revealed for what they are—well-meaning, often clever, occasionally astute, but far too often politically unsophisticated.

I would use this book with undergraduates. It might be useful, as well, in public policy programs. It would not work well in graduate courses.

Duke University ROBERT H. BATES

France after Hegemony: International Change and Financial Reform. By Michael Loriaux. Ithaca: Cornell University Press, 1992. 314p. $36.50.

Even a dabbler in French political economy knows that before you can properly claim to know anything at all, you must first master the details, all of them, eccentric and obscure though they are and pointless though at first they seem. Michael Loriaux has mastered the details. He has also mastered their historical and political context and framed the whole with a credible theory.

Loriaux sets out to map the Labyrinth—that maze of relations among state, finance, and economy that marked the development of France after 1945—and not only to map it but also to show how it developed; why it could succeed for a time; what pressures it came under as the world economy unraveled in the 1970s; and why, ultimately, in an irony of ironies, a Socialist government was compelled to adopt liberalizing reforms beginning in 1983. He succeeds brilliantly at these tasks, providing a historical narrative that is a dense, sustained synthesis covering perhaps two-thirds of this work.

Ariadne's thread for the whole enterprise is a single concept, that of an "overdraft economy," characterized by assured access to credit for major economic agents. Loriaux argues that such an economy arose under the "hegemonic protection" of the United States, survived for a while, became sclerotic, and eventually crumbled as American protection was removed.

Whereas others, including myself, have emphasized the modernizing role of French state mechanisms from Monnet through Barre, Loriaux compels attention to a darker side. His focus is on the "insurance functions" of the state and on the "moral hazard" that arose as the state lost its way in its own net of implicit guarantees of economic and social position. In a country that after 1945, allowed state-influenced institutional credit sources to dominate it and then devolved social security

functions onto firms, the presumption of assured access to credit invited stasis. There were modernizing institutions, to be sure; but Loriaux argues that on balance, the system inhibited economic and technological adjustment and made France more vulnerable to the shocks of 1973 and after than it otherwise would have been.

As at the level of the firm, so at the level of the state. France itself was able to institutionalize its internal overdraft economy only because the United States was prepared to grant analogous privileges to the whole nation. This we did, at first, out of fear of the French Communist party. From the Marshall Plan through 1968, France was able to close the gaps between the expected profitability and competitiveness of its industries (and other ventures, such as the First Indochina War) by relying on the United States. But when the United States turned inward, adopting a policy of "predatory hegemony" under the strains of Vietnam, France was cut loose. It then had to begin to face the hard budget constraints imposed by oil shocks, then European monetary coordination, and also, presumably, by the unwillingness of the Germans to extend credit on quite such easy terms.

Thus we come to the paradox of the 1980s. A Conservative government, that of Barre, had tried and failed to restore France's competitiveness through massive use of the traditional techniques of industrial and financial intervention. A Socialist government took two years to figure out just how hard the budget constraint had become. Then came the crisis. What to do? The only solution was to establish a capitalist financial market and an asset-based banking system in France, for the first time since the 1930s, so as to reestablish a buffer between the demands of society for insurance and the true capabilities of the state to provide it. This the Socialists did. It was an elegant (if not wholly effective) exercise in the politics of necessity, and Loriaux's write-up is an elegant exercise in the analysis of political choice.

Loriaux is highly critical of the overdraft economy from its inception to its end. In this, it may be that he stretches his point. He evidently believes that a restoration of free financial markets, to have begun with Mendes-France's proposed, but spurned, monetary stabilization of January 1945 and followed thereafter with a program of monetary orthodoxy à la Jacques Rueff, would have yielded a more competitive and resilient French economy in the long run. How can we be sure? After all, the French economy did grow reasonably strongly through the 1950s and 1960s, while maintaining at least a simulacrum of social stability. It had not achieved this when strong and free financial markets existed, in the 1930s. Perhaps moral hazard, overinsurance, and inflation were prices that had to be paid to achieve growth, prosperity, and some modernization in the delicate political environment of postwar France. The politics of necessity is a tricky business, and it is a little unfair to celebrate the outcomes when they conform to orthodoxy while condemning them when they do not.

Nor is it certain that the overdraft economy per se was a recipe for failure. Other overdraft economies existed—notably in Japan, Korea, and Finland—and (as Loriaux acknowledges) at least the first two of these showed capacity for internal reform in advance of crisis. Perhaps to create such an economy was a rational response to the opportunities provided by American hegemony, a chance to get ahead while the getting was good. And perhaps France's problem was mainly a matter of misplaced investment choices. In particular, where Japan and Korea zeroed in on the American market, France chose to adventure into the supply of weapons, capital goods, and engineering projects (notably, subways, airports, ports, and mines) to the developing world. It might have been a reasonable bet that expanding U.S. hegemony would have sustained the market for such exports; but it happened, instead, that American hegemony contracted. France, having made choices that were not so much wrong as unlucky, found itself and its opportunistic system among the first victims of a changing world.

These, however, are issues of interpretation. The pleasure of Michael Loriaux's achievement is that he makes possible a discussion of such issues on an exceptionally solid foundation, both of historical fact and reasoned theoretical judgment. *France after Hegemony* is an admirable book and a satisfying read.

University of Texas, Austin JAMES K. GALBRAITH

Inadvertent Escalation: Conventional War and Nuclear Risks. By Barry R. Posen. Ithaca: Cornell University Press, 1992. 280p. $36.50.

With the end of the Cold War, many policy-relevant books in security studies became of purely historical interest. Barry Posen's study of the process by which conventional conflicts can escalate to nuclear war avoids that fate, at least in part, because of its explicit theoretical focus. The body of the book is a very detailed examination of projected military operations in a major war between the Warsaw Pact and NATO, a potential conflict that has joined the communist regimes of Eastern Europe and the Warsaw Pact in the proverbial ashcan of history. Fortunately, the analytical tools that Posen develops and exercises in the study can be applied to a variety of regional conflicts in the post–Cold War world.

Posen's central argument is that a large-scale conventional war between two nuclear-armed adversaries is likely to escalate to the use of nuclear weapons despite the initial intentions of both sides to avoid such escalation. Three factors cause such tendencies. First, Posen argues, the "security dilemma" in international relations (in which actions taken by a state to defend itself unintentionally threaten others) also applies to the conduct of conventional war. Political leaders may issue orders to execute conventional military operations for defensive purposes, unaware that such operations threaten or damage the nuclear forces and command-and-control of its adversary. If that adversary believed that it was in danger of losing its nuclear capability, it might choose to launch a preemptive or limited nuclear attack in a desperate effort to avoid the effects of such vulnerability. Second, the organizational interests of the military exacerbate this dilemma: militaries often favor offensive doctrines and plans, since offensive operations tend to increase organizational power, simplify planning problems, and enhance prestige. In addition, military organizations, like most organizations, greatly value their autonomy from outside influence, which can seriously inhibit the sharing of details about operational plans with civilian authorities. Third, Posen borrows the concept of the "fog of war" from Clausewitz: the inevi-

table difficulty in understanding communications and intelligence in the midst of combat encourages inadvertent escalation both by making detailed central control over operations problematic and by increasing the fear that an adversary might take advantage of such confusion by launching a surprise attack.

The rest of the book presents Posen's assessment of the likely conduct of conventional military operations in a major war in Europe in the mid-1980s. There are four case studies. First, Posen analyzes a large-scale air war between the two alliances and argues that NATO conventional attacks on Warsaw Pact air defenses and command-and-control systems in Eastern Europe would have eventually increased the vulnerability of the strategic command-and-control systems in the Soviet Union. Second, an assessment of the ground forces campaign leads to the conclusion that NATO armored forces could have defended the central front, thereby avoiding deliberate escalation to nuclear war. Third, Posen examines the potential for escalation if NATO naval forces, following the U.S. maritime strategy, had attacked Soviet ballistic missile submarines and naval bases in a conventional war. Finally, Posen compares offensive and defensive strategies for NATO protection of sea lanes in the Atlantic, arguing that defensive operations could have been successful. In these scenarios, leaders in Moscow might have felt pressures either to initiate the use of nuclear weapons first or, at a minimum, to delegate effective control of nuclear weapons to lower-level military officers, thereby increasing the danger of accidental or unauthorized use of such weapons.

Posen's assessments are certainly plausible, although (as is usually the case in counterfactual analyses) reasonable people can disagree about the likelihood of various contingencies. Future research can therefore further illuminate this issue by examining prenuclear historical cases of inadvertent escalation and war termination. For example, a thorough investigation of the reasons why some conflicts escalated to gas warfare while others did not would at least provided analogical evidence on the likelihood of different nuclear scenarios. Similarly, studies of war termination may provide insights into the conditions under which states facing defeat terminate hostilities (the U.S. government's rationale for the maritime strategy) or make desperate decisions to escalate combat to more dangerous levels (Posen's argument).

In a *very* brief (two page) section of the concluding chapter Posen presents two reasons to suspect that medium and small nuclear powers in the future face even more serious dangers of inadvertent escalation in any conventional war: (1) "they are unlikely to be able to afford the numbers, basing modes, or early-warning capabilities that helped the superpowers to develop survivable retaliatory forces" and (2) "the conventional forces with which these countries have conducted their competition have all regularly engaged in offensive military operations" (p. 201). I would add a third reason to anticipate problems, namely, that many developing states have lower degrees of civilian control over the military, which would compound the organizational causes of escalation identified by Posen. These are strong reasons to be concerned. Posen does not, however, analyze factors which might *reduce* the vulnerability of nuclear weapons and command-and-control between small nuclear powers, such as the absence of stealthy aircraft or sophisticated antisubmarine warfare capabilities in these countries. Yet although Posen does not adequately address the similarities and differences between his superpower cases and the circumstances likely to exist between future proliferators, this was not his main task. Other scholars will be indebted to Posen for giving them a very valuable set of analytical tools to do this important work.

Stanford University SCOTT D. SAGAN

Winning the Next War: Innovation and the Modern Military. By Stephen Peter Rosen. Ithaca: Cornell University Press, 1991. 275p. $33.00.

In recent decades, research and debate on the determinants of military strategy and doctrine have advanced in two main waves, which we may call the *professionalist* and *institutionalist* approaches. The professionalist approach, defined by Samuel Huntington in the late 1950s, focuses on the status of military officership as a distinct profession with its own specialized knowledge and corporate identity. Motivated by technical love of their craft and by their sense of social responsibility, officers seek to enhance the resources and autonomy of the military in order to maximize its capability to carry out its function of securing the state. Civilian intervention in military decision making is unhelpful, because officers know their craft better than outsiders, and often damaging, because it politicizes and deprofessionalizes the officer corps.

The institutionalist approach, advanced by Barry Posen, Jack Snyder, Stephen Van Evera, and Matthew Evangelista in the mid-1980s, starts from slightly different premises but reaches quite different conclusions. Institutionalists recognize the military as a large, specialized, hierarchical organization whose behavior can be understood using the tools of organization theory. Large organizations, including the military, pursue institutional interests in maximizing their budget, prestige, and independence from outside control and in preserving the organization's self-concept, or "essence." Unless monitored from the outside, strategic and doctrinal choices may sometimes respond more to the military's institutional self-interests than to state requirements for military effectiveness. However, civilian monitoring can be difficult, because the military's near monopoly on relevant expertise makes it difficult for outsiders to criticize service arguments effectively (although the immense U.S. postwar civilian defense establishment has ameliorated this somewhat).

Innovation is often resisted, because it threatens the budget, independence, or essence of services or branches that may be deemphasized in favor of others, forced to coordinate more closely, or even lose their traditional missions. Successful innovation will be more likely when the existing leadership and doctrine has been delegitimated by defeat; when civilians intervene to help innovators overcome resistance; or when the innovation supports, rather than challenges, the organization's autonomy and essence.

Stephen Rosen seeks to revive the professionalist approach, arguing that innovation often originates within the military and is rarely instigated or facilitated by civilians. His study, an expansion of his "Theories of Victory" (*International Security* 13[1988]), provides explanations of what he sees as three distinct classes of

military innovations: peacetime, wartime, and technological. First, he argues that peacetime innovation is a function of military communities' vision of the future of war and of their criteria for promoting senior officers. Successful innovation depends on insightful imagination, as well as creation of promotion paths that will allow junior officers to advance their careers while pursuing the innovation. This, in turn, requires the support of senior officers. Civilian intervention in the promotion process would be seen as illegitimate and would factionalize the officer corps (pp. 20–21).

Second, innovation in wartime is far more difficult than in peacetime, because time is short, the lessons of combat are often not obvious, and disruption of ongoing operations is risky. Successful wartime innovation, when it occurs, usually requires new measures of strategic effectiveness. Innovators must rethink the connection between operational missions and strategic goals, since success may require not improved execution of existing missions but, rather, their abandonment in favor of new missions and capabilities (p. 110). This process is carried out primarily inside the military.

Finally, for military-technological innovation, Rosen rejects both the institutionalist and "technology-push" explanations in favor of a modified "demand-pull" approach. Technologies are developed based on intelligence estimates of requirements to counter the enemy's current, and expected future, capability, both in terms of technology and doctrine. Successful innovation depends on managing the inherent uncertainty in intelligence. At the research-and-development level, civilian scientist can contribute; but military management is preferred, because officers better understand the combat applications of technology (p. 227). (As a sidelight, Rosen claims that U.S. military research-and-development has been much more cost-effective than most observers realize [p. 46]; this is a question worth further investigation.)

Rosen makes significant contributions, especially in pointing out the importance of promotion paths and strategic evaluation, but does not overturn the institutionalist argument. Internal innovators do often generate new doctrines (and sometimes new technologies), and judicious use of promotion and other internal bureaucratic levers can build constituencies within the service; but actual implementation in creation of new force types or war plans is often blocked by entrenched interests that the innovation threatens. Before World War I, numerous military reformers in all the major European armies pointed out the need to adjust tactics to a battlefield on which the defense had been strengthened by machine guns and barbed wire; but they could not prevail against institutional interests that required offensive doctrines.

Rosen's three peacetime cases illustrate this. The 1930s renovation of U.S. Marine Corps amphibious doctrine, which fitted perfectly with the corps's essence as the spearhead of overseas land operations and enhanced its resource and autonomy claims, was carried though, in part, by promoting amphibious-minded junior officers and without civilian assistance.

Also in the interwar years, Rear Admiral William Moffett's strategy of coopting senior nonaviators to support promotion of junior aviators made all the difference between the advance of American aircraft carrier doctrine and the stagnation of the British Fleet Air Arm at the same time. However, Rosen is wrong to play down the importance of the Pearl Harbor attack. While an effective combat doctrine was developed before the war, it did not replace the Navy's battleship-centered war plan until Pearl Harbor eliminated the battleships.

Finally, in the 1950s, airmobility doctrine began to make progress within the army when pilots were no longer looked down on as glorified taxi drivers and were, instead, considered eligible for promotion to combat commands. However, sizable airmobile combat units were not formed until Defense Secretary Robert McNamara demanded their creation in 1962. Rosen minimizes the importance of this intervention by suggesting that McNamara may have been primed by aviation advocates within the army, but the crucial point is that they could not prevail without outside assistance.

In one sense, Rosen's promotion path argument is worth more weight than he gives it. One of the most important motivations for military resistance to innovations is that they create requirements for new professional skills and devalue the skills of officers trained in the old doctrine, damaging their prospects for promotion.

So how do we win the next war? Rosen suggests that in an environment in which we cannot predict the likely scenarios that we may face or the missions that will be required and where limited budgets will not permit large-scale acquisition of new weapons or creation of new force types, we must rely instead on flexibility and innovation. In particular, he recommends relying on analysis and simulation of as many different contingencies as possible and researching a variety of technologies, rather than investing heavily in production of any one.

This policy advice is eminently sensible (and rapidly becoming conventional wisdom), although it is not really dependent on the rest of the book. More interesting would have been recommendations directly tied to the author's theoretical arguments. If successful peacetime innovation requires new promotion paths, can we design ways to institutionalize opportunities for younger officers to make their careers pursuing innovation doctrines or technologies? If wartime innovation depends on improved measures of strategic effectiveness, can we improve the odds that focused and timely analysis will be conducted, disseminated, and acted upon in future conflicts? Attention to these questions might help us design useful reforms.

University of Michigan CHAIM KAUFMANN

History and Strategy. By Marc Trachtenberg. Princeton: Princeton University Press, 1991. 292p. $44.50 cloth, $14.95 paper.

History and Strategy is a collection of seven essays by Marc Trachtenberg, all but one of which have been published elsewhere. Their subjects are diverse: the early evolution of American strategic thought, the origins of World War I, American nuclear strategy in the early 1950s, the nuclearization of NATO, the Berlin crisis, the influence of nuclear weapons in the Cuban Missile Crisis, and the relevance of historical analysis. Many of the essays are excellent, all provocative; but they do not cohere as a book.

The chapter on American strategic thought sets out to answer a puzzle, Why did the "golden age" of strategic

analysis "hit something of a dead end in the 1960s"? It never really provides an answer but develops an insightful critique of the literature. Trachtenberg faults these strategy analysts, especially Thomas Schelling, for narrowly conceptualizing war, peace, and strategy as problems of crisis management and bargaining. By focusing almost exclusively on the manipulation of risk and "the purely military side of war causation," American strategists reduced strategy to tactics and analyzed these tactics outside of any political and historical context. To reinvigorate the study of strategy, it is necessary to ground deductive, abstract analysis in a thorough understanding of the processes that have led to the development of nuclear arsenals and the conflicts to which they are relevant.

The second essay in the book is the author's controversial attack on the thesis that war broke out in 1914 because of the rigidity of military plans and the loss of control they engendered. The gist of his argument is that the Schlieffen Plan was based on the accurate political judgment that France would not remain neutral in a Russo–German war; that German political leaders understood and approved the Schlieffen Plan; and that they, not the military, made the decision for war. In Russia, too, Trachtenberg maintains, political authorities understood the meaning and consequences of mobilization.

From World War I, the book shifts back to nuclear strategy and how Americans adjusted to the loss of their monopoly on nuclear weapons. The author argues that anticipated shifts in the military balance played a critical role in shaping grand strategy and specific policies, especially with regard to Korea. He documents how seriously preemption and other aggressive strategies were taken at the highest levels of the government in the early 1950s. No final decision was taken against preemption until 1954. The chapter as a whole illustrates the myopia of the Pentagon and the Eisenhower administration. For both, the military balance was the only reference necessary to explain the twists and turns of Soviet foreign policy.

A follow-on chapter examines U.S.–West European military relations during the 1950s. Trachtenberg challenges the conventional wisdom that the nuclearization of NATO was an attempt to bridge the gap between European defense requirements and the political and economic constraints that ruled out large conventional forces. He insists that a nuclear-oriented strategy made good military sense in the early 1950s. The real debate in Washington was between the army's conception of a "forward defense" and the air force's preference for a "peripheral strategy" that contemplated withdrawing from the continent. By 1953, technical breakthroughs and the growth of the nuclear stockpile made a nuclearized forward defense feasible. Such a strategy was preferable to the Eisenhower administration and the Europeans for strategic and political reasons.

The next two chapters look at two of the tensest confrontations of the Cold War: Berlin in 1959–62 and Cuba in 1962. Trachtenberg makes a compelling case that the Soviet challenge to the Western position in Berlin was motivated primarily by Soviet fears that West Germany would soon acquire nuclear weapons. From the Soviet perspective, Konrad Adenauer and the United States were intent on changing the status quo, Moscow, on maintaining it. In the end, the Unites States came to accept the division of Germany and even to view it as a positive development. The crisis also indicated just how difficult nuclear weapons made it for either side to threaten war credibly.

The Cuba chapter is now out of date because of the new information that has since become available, especially on the Soviet side. The question it poses is still timely: What role did nuclear weapons play in shaping American and Soviet policies in the crisis? Trachtenberg argues that the lopsided strategic balance led to asymmetrical nuclear anxieties and (here I strongly disagree) to a "balance of resolve" that favored the United States and was reflected in the outcome of the crisis. He is on firmer ground in arguing that President Kennedy and Secretary of Defense McNamara were constrained by their fear of loss of control from exploiting their strategic advantage. Soviet leaders, who did not see this, exaggerated the threat of an American nuclear attack.

The final chapter, "Making Sense of the Nuclear Age," argues that historical scholarship can facilitate a reconceptualization of the fundamental problems of foreign and military policy in the nuclear age. This essay should have introduced the volume and gone on to identify the themes that would be examined in the subsequent chapters. With a sharp introduction, the individual chapters could have been more focused and the interesting but irrelevant essay on World War I dropped from the collection. The conclusions should have offered the author's reconceptualization of nuclear strategy, thereby validating his initial claim and enhancing the volume's value and interest to scholar and policymaker alike.

University of Pittsburgh RICHARD NED LEBOW

International Theory: The Three Traditions. By Martin Wight. New York: Holmes & Meier, 1992. 286p. $39.95

Martin Wight can be considered the founder of the English school of international relations theory, which emphasizes the cultural and moral presuppostions of international relations, its historical dimensions, and the works of great thinkers in the Western tradition. This book consists of Wight's lectures at the London School of Economics during the 1950s and 1960s, published 20 years after the author's death. In a brilliant introduction, the late Hedley Bull, Wight's most distinguished student, argues that Wight's approach "provides an antidote to the narrow and introverted character of the professional academic debate about International Relations, . . . opening it out to wider intellectual horizons" (p. xix). So it does: indeed, much of the merit of this work lies in its claim that contemporary controversies are extensions of old disputes, carried out in previous centuries by thinkers more deeply imbued with philosophical subtlety and historical understanding than we are. Such an argument allows us to fit ourselves better into a collective tradition, giving us perspective and, one hopes, humility. Publication of *International Theory* is therefore an important contribution to the literature, which should be read by American students and scholars.

International Theory is built around Wight's threefold classification of thought on international relations into Realism (sometimes labelled Machiavellian), and Revolutionism (stylized as Kantian), and rationalism (viewed as Grotian). Realism denies the reality of international

society, sees states' interests as essentially in conflict, and justifies action by necessity or by success. War is inevitable; history is cyclical. Revolutionism is an odd, hybrid category, meant to encompass Kant, Cobden, Lenin, Hitler, and Stalin. It seems to me that generalizations about revolutionists fit the latter better than the former. Kant, indeed, stressed that world peace would be promoted by republican constitutions within states. But he was no utopian about human nature; he sought a loose federation, rather than a world state; and he did not believe in an authoritarian transformation of the world. Wight himself suggests in his conclusion that "soft" revolutionists, from Kant to Nehru, should be distinguished from "hard" revolutionists, such as the Jacobins and Marxists (p. 267). Rationalists—toward whom Wight seems to lean—regard interstate society as real, compromise among interests as usually possible, and moral dilemmas as soluble through a combination of respect for rules and concern about consequences. War is to be avoided when possible and waged as humanely as possible when it occurs.

Wight admits that there is a "confluence" of these traditions; he is aware of the perils of classification. In his introductory essay, Bull argues that "the account of the three traditions will not bear all the weight that Martin Wight sought to place upon it" (p. xviii); and I agree. The hybrid character of his revolutionist category muddies his analysis of revolutionism. However, the categories of realism and rationalism facilitate some of Wight's most striking insights: such as "the Hobbesian character of the United Nations," never more evident than in the war against Iraq (p. 34); realism's expansionary conception of national interest versus rationalism's contraction of interests (p. 126); and realism's ambivalence about our ability to ascertain others' interests versus rationalism's claim for the objectivity of distance (pp. 146–47). Wight's categories work better for interests, international society, and the ethical basis of action than they do for power, balance of power, bargaining, and war. World politics is multidimensional, and views on bargaining or the balance of power are not cogently defined by views on international society or ethics. In spite of Wright, rationalists need not believe in the inviolability of promises, especially if such promises seem to conflict with peaceful change and adaptation to new situations.

Wight is definitely a thinker in the Western tradition, making few references to other cultures' views of international relations. Consistent with his Christian piety, one might fear that he would therefore adopt an uncritical view of the actions of the west toward non-Western peoples. Nothing could be further from the truth. His brilliant chapter 4 on relations with "barbarians" extensively documents Western justifications of savage cruelty and wars of extermination toward these peoples. Indeed, he identifies "extermination to eradicate heresy, to purify society, for scientific social engineering, for the sake of the cause," as "specifically Western." Islam, he says, "may have invented the holy war, but it did not exterminate" (p. 229). For critical insights such as these, from someone *within* the Western tradition, *International Theory* is worth serious reading and reflection.

Wight's greatest oversight, which Bull also mentions, is his neglect of the scientific or behavioral search for laws of action (or contingent generalizations) about world politics. Wight does not defend his view that cumulative knowledge is impossible in world politics. He claims that rationalists do not see international politics in terms of laws of behavior but that most contemporary behavioralists, who seek laws, are also rationalists. Yet American international relations specialists, even the most behavioral and rationalist, should read this book in order to place themselves in the tradition of thought in international relations, appreciate the insights that come from studying particularities, and see how history and philosophy can enrich our study by providing us with insights and even, one dares to say, testable hypotheses.

Harvard University ROBERT O. KEOHANE

The Tet Offensive: Intelligence Failure in War. By James J. Wirtz. Ithaca: Cornell University Press, 1991. 290p. $34.95

Strategic military surprise has been a satisfying genre in security studies. Investigation of a complex puzzle—why states and leaders seem so vulnerable to catastrophic surprise when the resources devoted to avoiding them are substantial, the stakes huge, and their suspicions about antagonistic states very high—has yielded a relatively simple and elegant theoretical explanation. Even better, it seems to hold across cultures, times, and personalities, with later studies providing additional confirmation.

James Wirtz's volume is a case in point, very much in keeping with other case studies of strategic surprise. Thus, it has little in terms of innovation on this subject. However, it is more than satisfactory in recounting the events of the Tet Offensive and is clear and precise in the application and refinement of the available theory. The theory and contributors to its development are insufficiently cited. Readers unfamiliar with the subject may not realize how firmly the book is set within an existing theoretical perspective.

Is vulnerability to strategic surprise due to the attacker's *deception* or the victim's perceptual mistakes? Is defender misperception due mainly to the combination of attacker secrecy, the ambiguity of available data, and the distractions of other information ("noise") or mainly to distortions imposed by defender preconceptions, beliefs, and expectations? Are the key mistakes made by top officials or by organizations and lower-level personnel? Finally, do the misperceptions spring from standard cognitive processes or "motivated biases"? Wirtz's analysis puts considerable emphasis on deception (more precisely, on the interplay between the victims' perceptual predilections and the enemy's elaborate deception effort). Like analysts of other cases, however, he finds that the victims' preconceptions in this instance were crucial. There was enough information available, clear enough in import, for the Americans to have anticipated the Tet Offensive. Their preconceptions (bolstered by the deception) got in the way. He parallels other studies in locating the major mistakes mainly at the top, with leaders and central headquarters analysts. Finally, most of the distorted analysis they displayed appears to have flowed from unmotivated bias. Along the way, he spots instances of all the standard cognitive errors, such as: (1) assuming more action–reaction linkages with the enemy than actually exist; (2) letting preoccupation with one's own strategy override efforts to grasp the enemy's;

(3) crying wolf; and (4) comfortably explaining away discrepant information.

As the emphasis on deception is the most innovative feature of all this, it is unfortunate that it is open to challenge. The massing of enemy forces on the borders, especially near Khe Sanh, is depicted as a deception about the location of the forthcoming attack designed to keep American forces away from the cities. Thus, a Vietcong attack there would face only the (ARVN) and could more readily (it was hoped) provoke a popular uprising. Maybe so, but he admits that the evidence that deception was the purpose is incomplete; and the evidence there is fits the equally plausible explanation that the North Vietnamese wanted to exploit the Americans' presence at the border to inflict heavy casualties and maybe another Dien Bien Phu, while *simultaneously* hitting the ARVN and the government in the cities. This could make Khe Sanh less a deception than part of a two-pronged attack, which would explain why the enemy did not just feign an attack but went into it vigorously, taking heavy casualties. Hanoi may have been seeking the same damaging political–psychological results via *either* of the two attacks. Whether one was simply intended as a deception cannot be definitively determined, and the conventional understanding of the role of deception in achieving a strategic surprise is left unchanged. His analysis is a stimulating view that will have to await further information from Hanoi to be fully confirmed.

Even the claim that Westmoreland overreacted (was deceived) in reinforcing Khe Sanh is problematic. After all, as the author notes, another Dien Bien Phu would have had an immensely damaging impact on the war effort, and Westmoreland was under great pressure in this regard because of McNamara's huge investment in the idea of a defensive barrier across the northern border and President Johnson's intense preoccupation with that area. He would have had to keep very large forces there in any case, deception or not.

For our general reflections on the Vietnam War, the book is valuable in many ways. For instance, Wirtz finds little evidence of willful distortion of information. Most of that which challenged official perceptions (alternative analyses, dissenting intelligence findings, discrepant information) was passed along to very high officials and given at least some consideration. The officials do not, in his account, appear to have aggressively evaded confrontation with criticism and dissent.

He disputes the view that headquarters in Saigon deliberately understated the size of Vietcong forces—the position of the CIA's Samuel Adams at the time and since. Military Assistance Command Vietnam's estimates were largely correct, he says; but Hanoi, accepting the veracity of the same reports Adams used, arrived at the same inflated estimates of their support in the south that he did. American leaders knew that a major Vietcong offensive would be militarily foolish in view of actual Vietcong strength. Hanoi overestimated its strength in deciding to mount what the United States, therefore, considered an improbable kind of attack. This is a nice touch in the author's assessment, although finding that attacker misperceptions played an important role is not original with Wirtz and this case.

He also concludes that those suspicious of the periodic enemy-offered truces (because the enemy exploited them) or of Hanoi's offers to negotiate were, on the whole, correct. The North used such offers to help set up the Tet Offensive; and taking advantage of the Tet ceasefire was only exploitation beyond the norm.

University of California, Irvine PATRICK M. MORGAN

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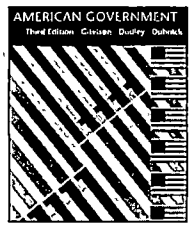
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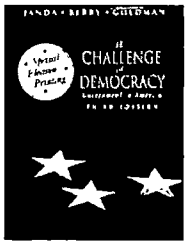


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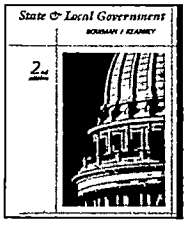
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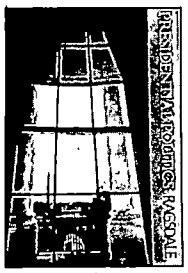


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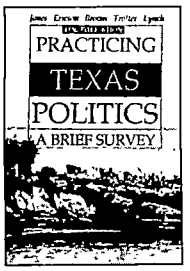


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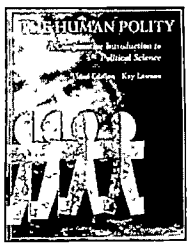


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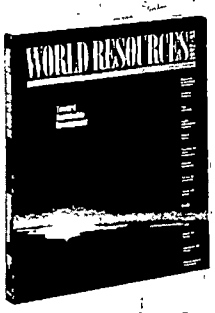
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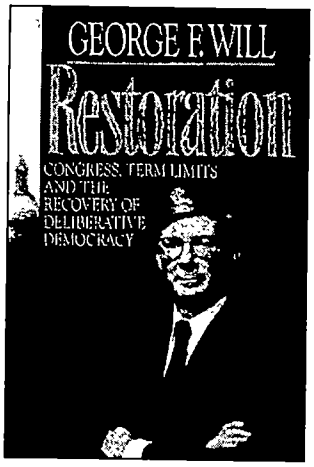
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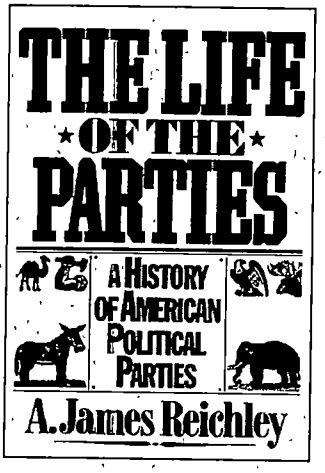
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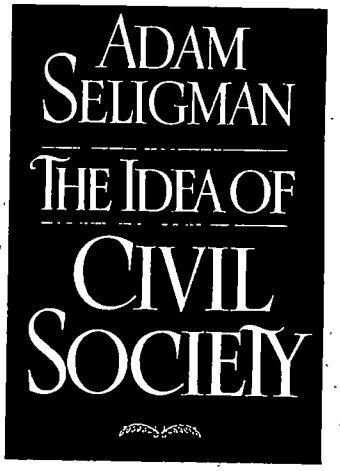
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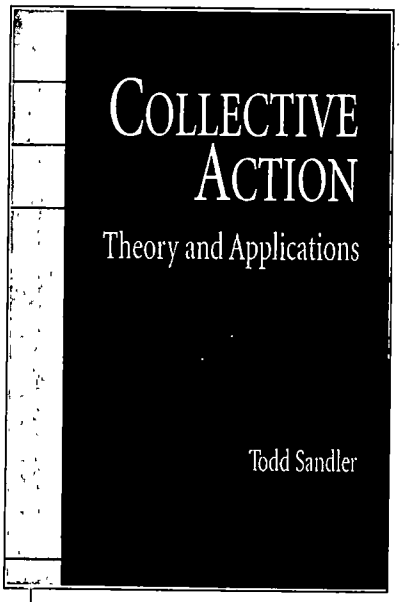
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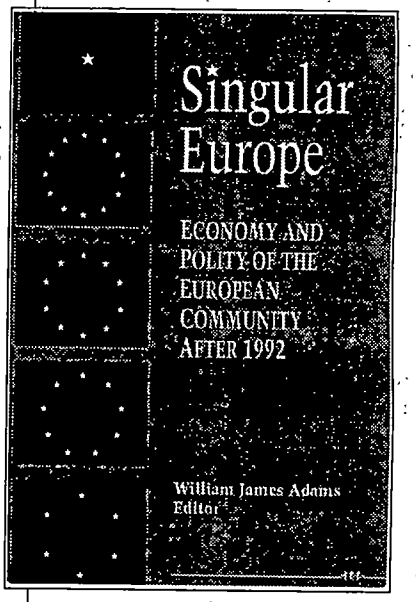
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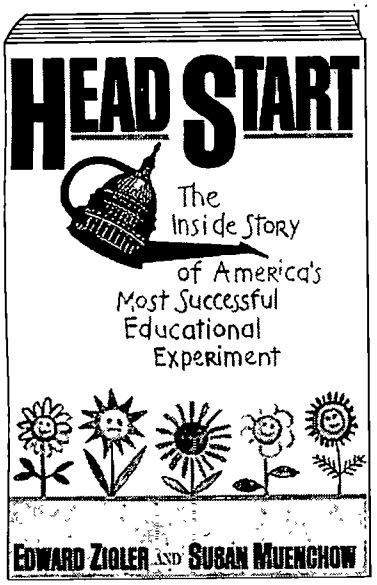
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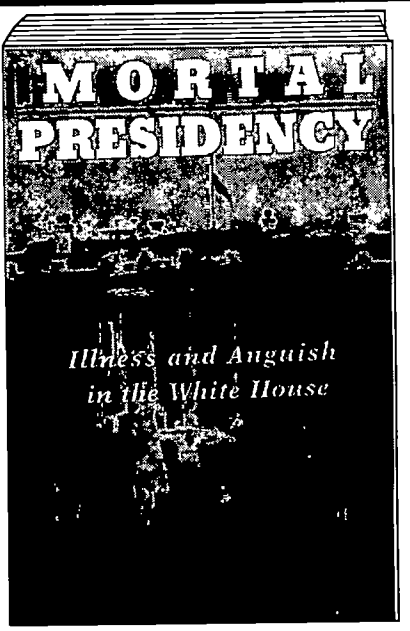
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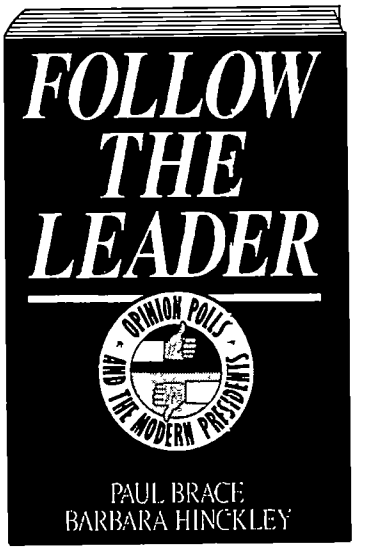
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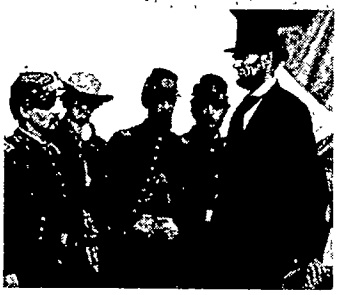
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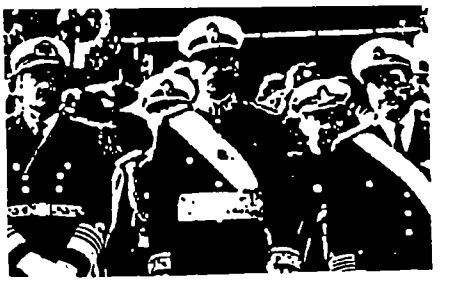
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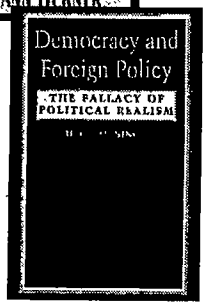
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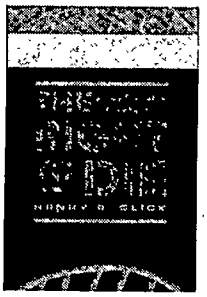
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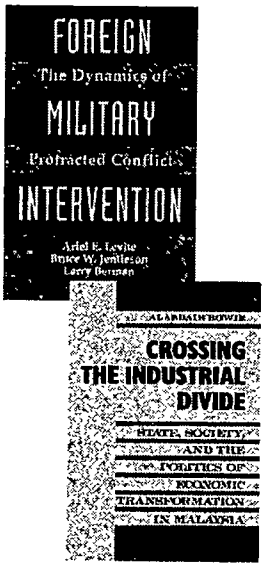
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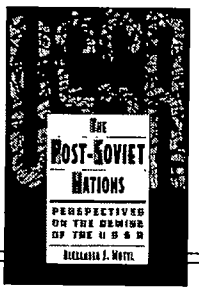
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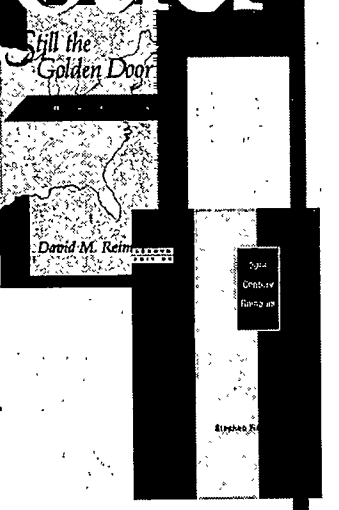
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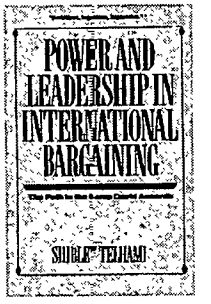
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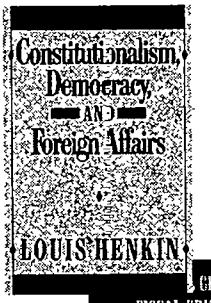
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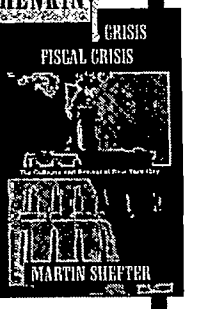
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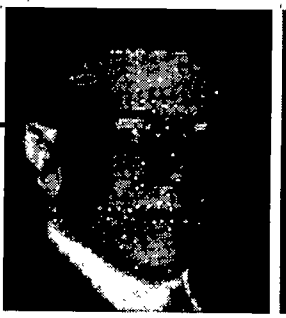
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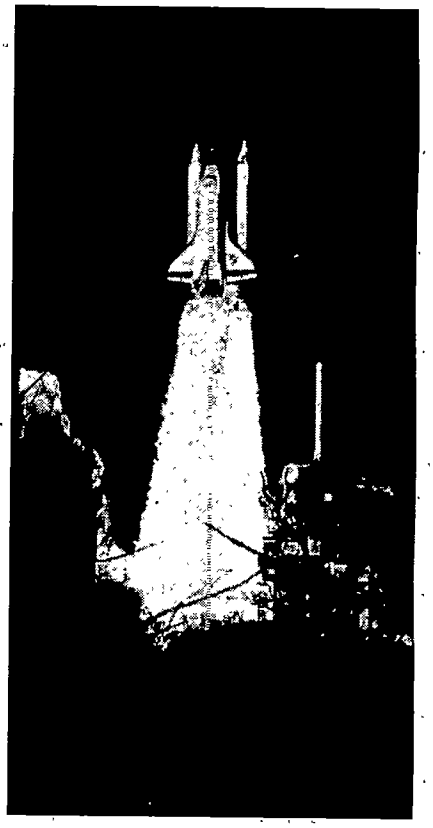
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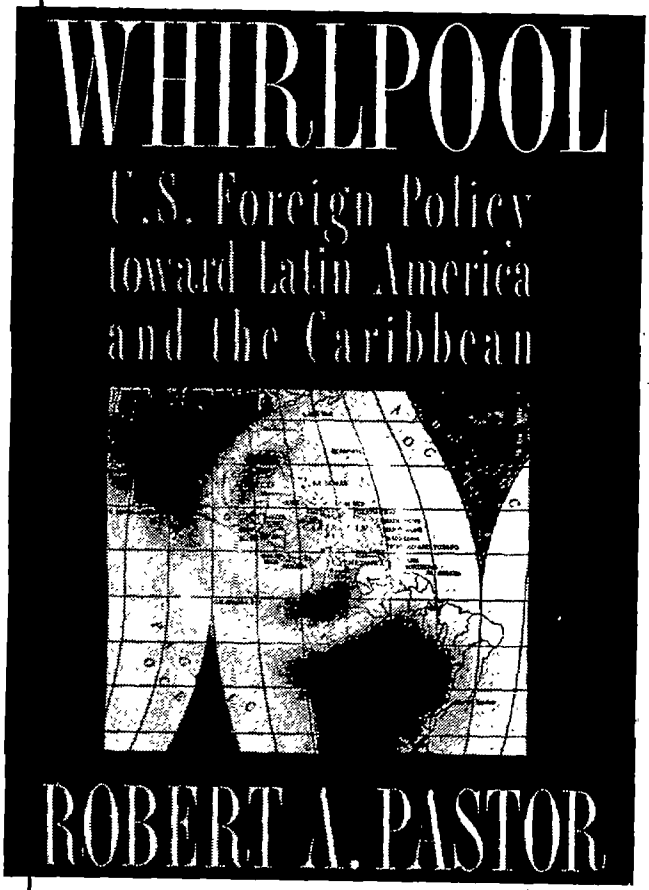
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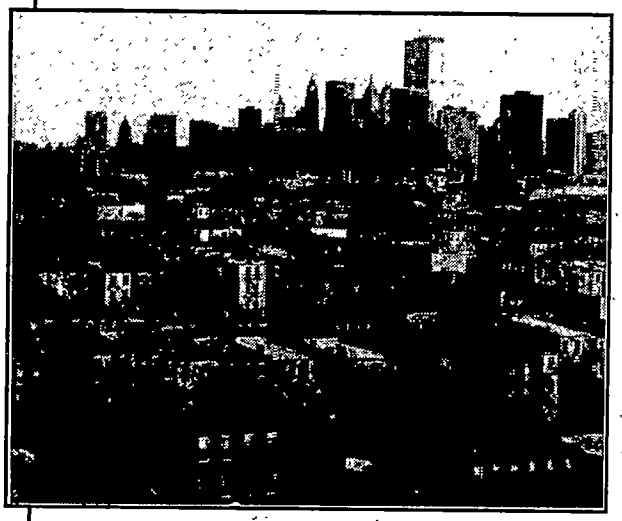
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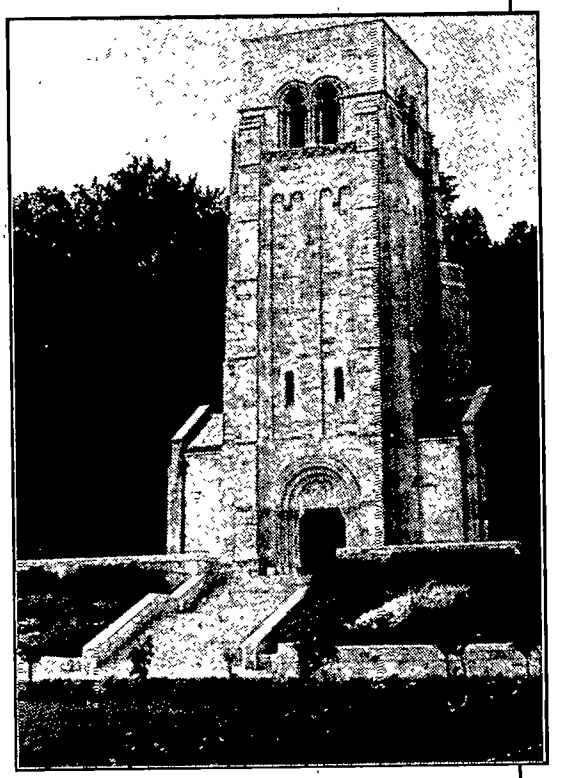
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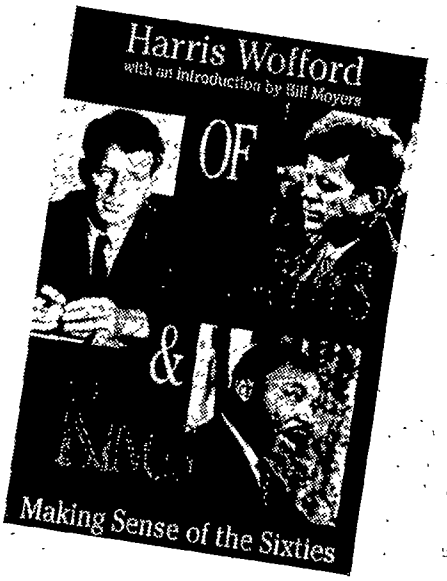
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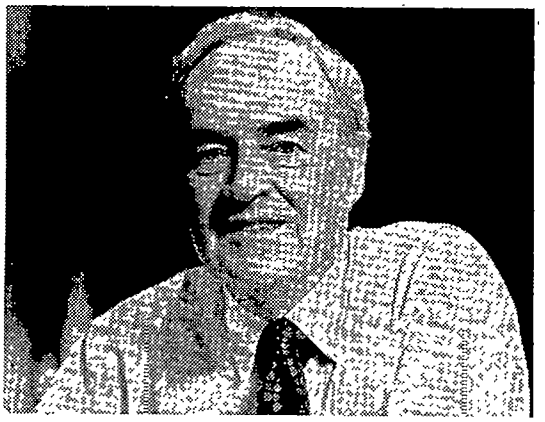
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Wofford in 1966 became founding president of the State University of New York's College at Old Westbury and was president of Bryn Mawr College from 1970 to 1978. In May 1991 he was appointed to fill the seat of the late Senator John Heinz and in November, when he defeated former Governor and U.S. Attorney General Dick Thornburgh, he became the first Democrat elected to the U.S. Senate from Pennsylvania since 1962.



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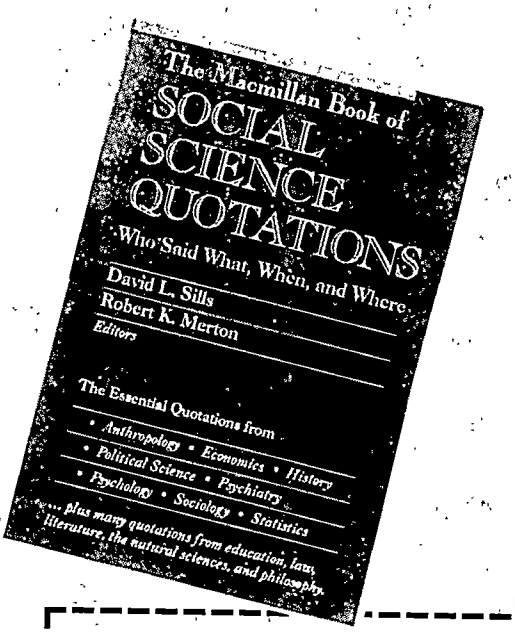
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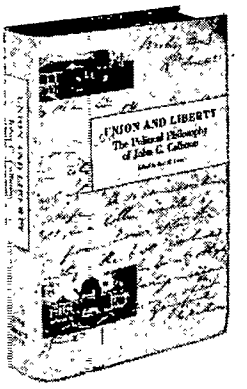
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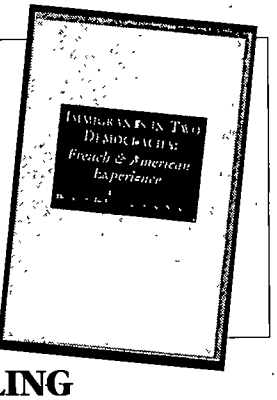
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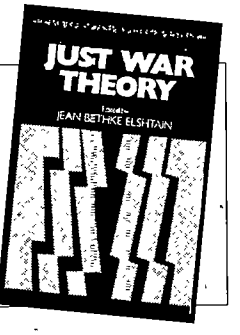
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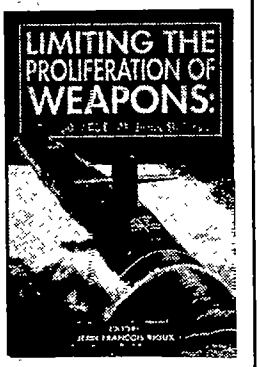
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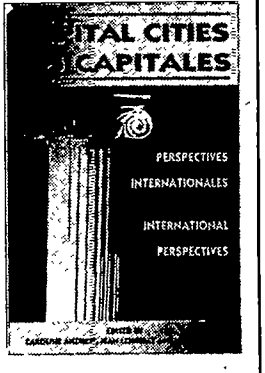
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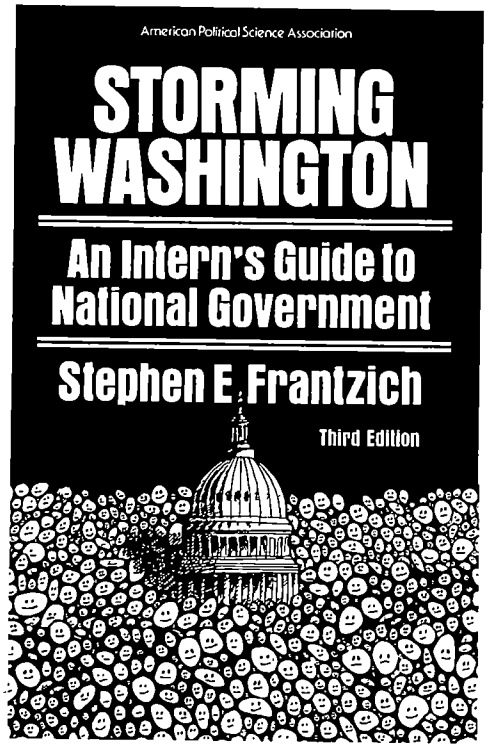
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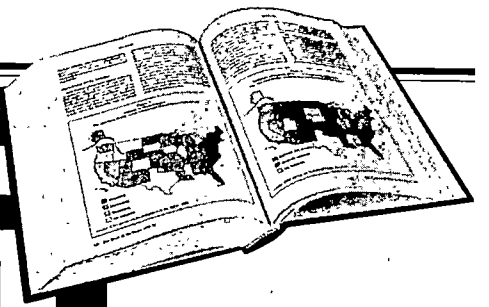
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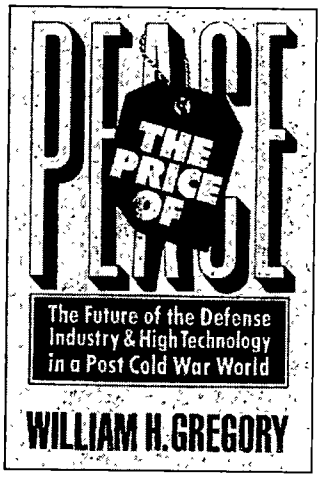
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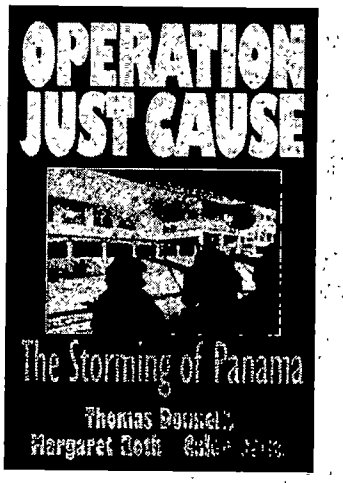
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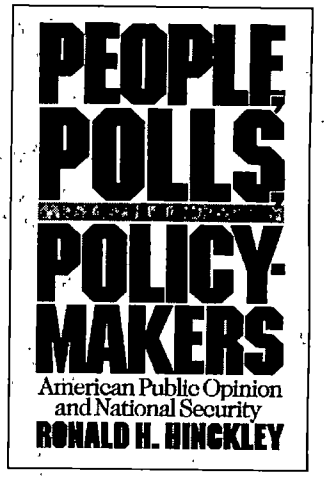
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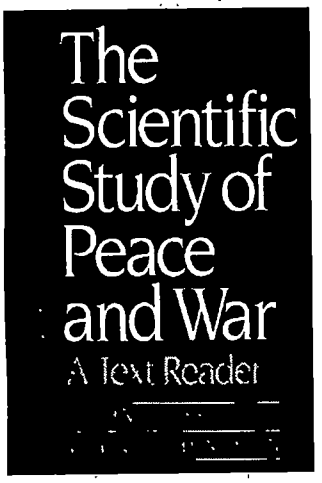
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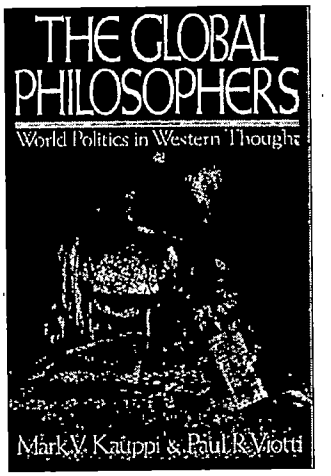
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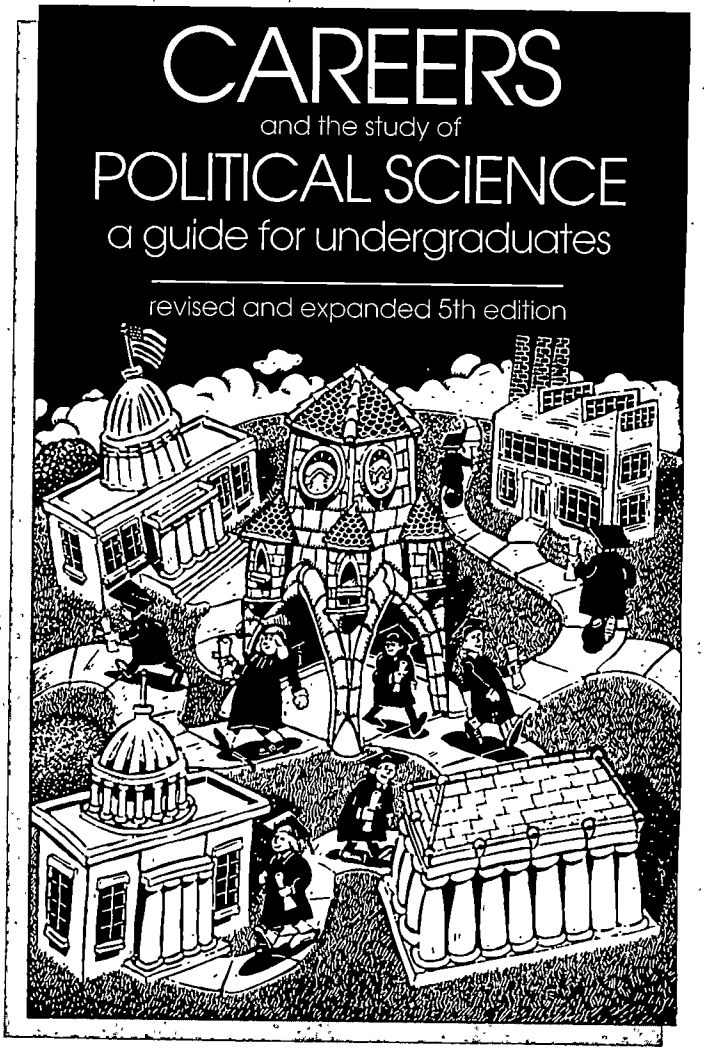
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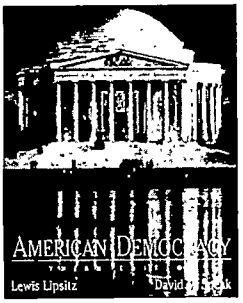
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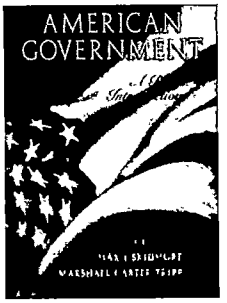
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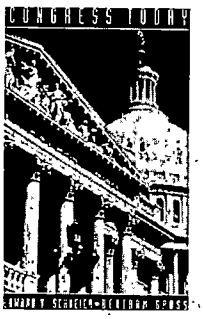
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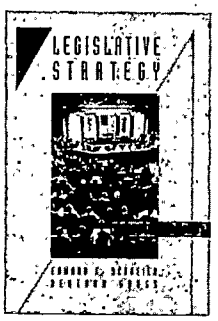


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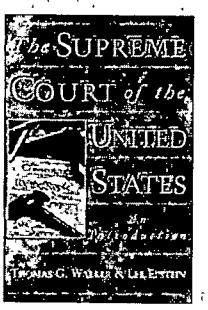


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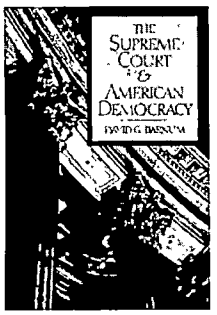
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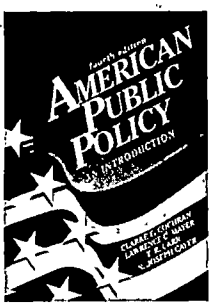
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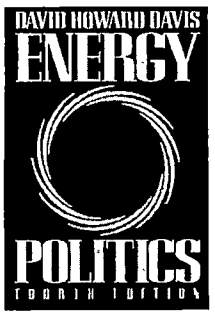
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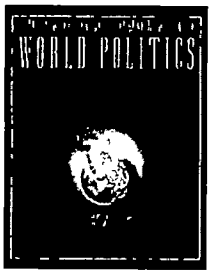
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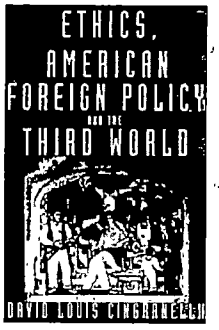
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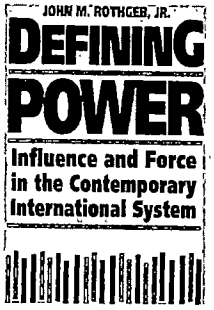


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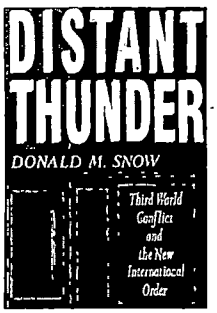
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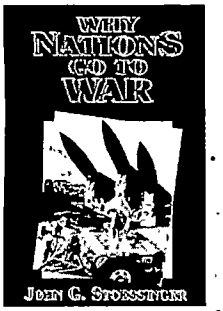
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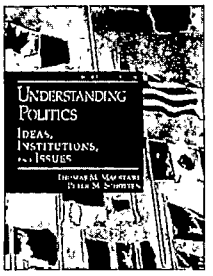


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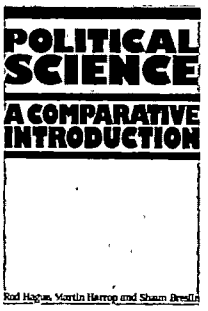


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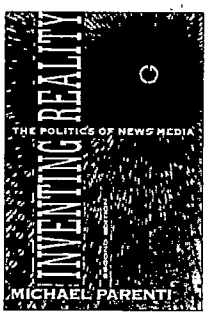
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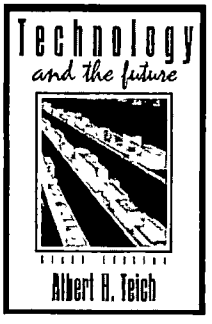
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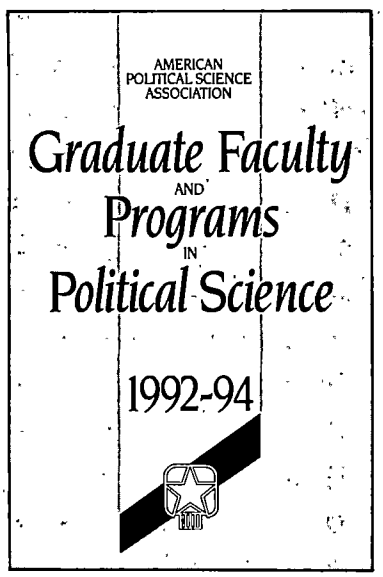
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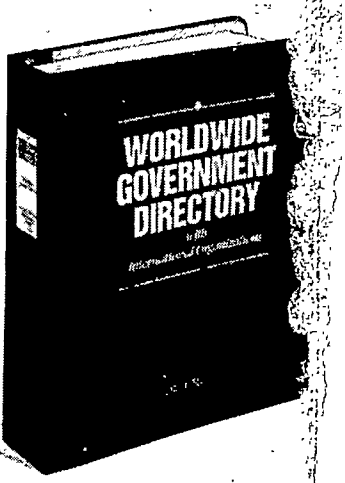
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